Handbook of
THE PHILOSOPHY OF SCIENCE

General Editors: DOV M. GABBAY, PAUL THAGARD, AND JOHN WOODS

PHILOSOPHY *of*
ANTHROPOLOGY
and SOCIOLOGY

Edited by Stephen P. Turner
and Mark W. Risjord

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GENERAL PREFACE

Dov Gabbay, Paul Thagard, and John Woods

Whenever science operates at the cutting edge of what is known, it invariably runs into philosophical issues about the nature of knowledge and reality. Scientific controversies raise such questions as the relation of theory and experiment, the nature of explanation, and the extent to which science can approximate to the truth. Within particular sciences, special concerns arise about what exists and how it can be known, for example in physics about the nature of space and time, and in psychology about the nature of consciousness. Hence the philosophy of science is an essential part of the scientific investigation of the world.

In recent decades, philosophy of science has become an increasingly central part of philosophy in general. Although there are still philosophers who think that theories of knowledge and reality can be developed by pure reflection, much current philosophical work finds it necessary and valuable to take into account relevant scientific findings. For example, the philosophy of mind is now closely tied to empirical psychology, and political theory often intersects with economics. Thus philosophy of science provides a valuable bridge between philosophical and scientific inquiry.

More and more, the philosophy of science concerns itself not just with general issues about the nature and validity of science, but especially with particular issues that arise in specific sciences. Accordingly, we have organized this Handbook into many volumes reflecting the full range of current research in the philosophy of science. We invited volume editors who are fully involved in the specific sciences, and are delighted that they have solicited contributions by scientifically-informed philosophers and (in a few cases) philosophically-informed scientists. The result is the most comprehensive review ever provided of the philosophy of science.

Here are the volumes in the Handbook:

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Philosophy of Anthropology and Sociology, edited by Stephen Turner and Mark Risjord.

Philosophy of Medicine, edited by Fred Gifford.

Details about the contents and publishing schedule of the volumes can be found at http://www.johnwoods.ca/HPS/.

As general editors, we are extremely grateful to the volume editors for arranging such a distinguished array of contributors and for managing their contributions. Production of these volumes has been a huge enterprise, and our warmest thanks go to Jane Spurr and Carol Woods for putting them together. Thanks also to Andy Deelen and Arjen Sevenster at Elsevier for their support and direction.

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PREFACE

There was a time in the philosophy of social science when a volume like this could cover the issues with half dozen essays. From the perspective of the early twenty-first century, however, gaining a systematic overview of twentieth century developments in the philosophy of the social sciences, and particularly the philosophy of anthropology and sociology, is difficult. To a much greater extent than most scientific disciplines, philosophical issues and perspectives have been a part of the internal development of the fields. The large "methodological" literature *within* the disciplines overlaps and often overwhelms the specifically philosophical literature *on* the disciplines. As a result, it no longer seems appropriate to treat the philosophical issues independently from the methodological and substantive theoretical developments in the field.

The first generations of cultural anthropologists and qualitative sociologists drew on German philosophical traditions of thought about interpretation, culture, and history. As hermeneutics and phenomenology developed, anthropologists and sociologists continued to absorb and adapt their views. The analytic tradition was influential as well. Wittgenstein's ruminations on language and the mind were picked up by Rodney Needham, Clifford Geertz, and Pierre Bourdieu. The unity of science movement acknowledged sociology as one of the sciences to be unified, and there was intense interaction between philosophers of science and sociologists during certain periods. In the nineteen-fifties, for example, there was a long-running seminar jointly conducted by Ernest Nagel and Paul Lazarsfeld, with Robert Merton's occasional participation, and Lazarsfeld engaged in correspondence with such philosophers of science as Patrick Suppes. The "positivistic" sociologist George Lundberg was involved with the journal *Philosophy of Science* in its early years, and was a personal sponsor of Carl Hempel. Hempel, in turn, wrote on such subjects as functional explanation in publications devoted to sociological theory. Sociologists were among the most avid consumers of Logical Positivism, motivated by the project of "making sociology a science."

Despite these strong connections, philosophical writing only intermittently engaged anthropology and sociology, and rarely at the level of actual explanatory practice. Discussion had its own internal direction and tended to revolve around a stock set of examples. The primary concerns were either metaphysical (e.g. the ontological status of social entities, or the analysis of intentionality) or epistemological (e.g. status of social theories, or the difference between social and natural sciences). Toward the end of the century, this changed. Even within standard philosophy of science the relevance of physics as a model was challenged in such papers as Clark Glymour's "Social Science and Social Physics" of 1983. The obsession

with the scientific status of sociology within sociology waned. Donald Davidson's "The Very Idea of a Conceptual Scheme" placed issues that were previously specialist topics in the philosophy of social science into the mainstream of philosophy. Problems of causal analysis in statistical sociology emerged as core problems in the philosophy of science. Feminist philosophy of science incorporated reasoning from the sociology of knowledge. The anthropological concept of "practices" became a standard usage in philosophy. Issues about the explanatory status of normative concepts of rationality were shared between philosophy and social science. It is now apparent that a philosophically adequate treatment of these issues needs to be sensitive to the role that the key ideas play in the empirical disciplines.

This volume attempts to present the philosophy of anthropology and sociology in the light of this on-going transformation of the field. Our aim has been to provide a technically adequate background to the many philosophical issues that arise in relation to anthropology and sociology. We have tried to be mindful of the history of these issues, which is often complex and deeply embedded in the histories of the relevant disciplines. We are very pleased that this outstanding group of contributors has done justice to the relationships among the philosophical questions, methodological debates, substantive empirical issues, and to the historical development of philosophy, anthropology, and sociology in the twentieth century.

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DEFINING A DISCIPLINE: SOCIOLOGY AND ITS PHILOSOPHICAL PROBLEMS, FROM ITS CLASSICS TO 1945

Stephen P. Turner

INTRODUCTION

The beginning of the 20th century coincides with the establishment of the modern disciplines of the social sciences, chiefly in the United States but on a smaller scale in Western Europe as well. These disciplinary structures, which varied from country to country, provide the organizing principle of this handbook. The early 20th century history of methodological, and more broadly, philosophical, writing in these areas is inseparable from this discipline-building process. Part of the rationale for the distribution of topics among these new "disciplines" had to do with methodological issues, notably the emergence in the most powerful of the new disciplines, history and economics, of methodological and theoretical orthodoxies which had the effect of excluding topics, on methodological or metatheoretical grounds, which had previously been important to them. This exclusion induced scholars concerned with these topics to seek alternative disciplinary homes and at the same time to construct methodological defenses and accounts of their own activities in the framework of these new homes. In this chapter my aim will be to identify and explicate the major alternative approaches to the problems of disciplinary identity and "method" broadly construed, and to indicate how each of them produced, and responded to, philosophical issues. I will confine the discussion to the period before 1945 for the most part. I will also, in line with the aims of this Handbook series, largely ignore the mid-19th century background to the disciplinary projects of this period, though Auguste Comte, J. S. Mill, and Herbert Spencer certainly also formed part of the consciousness of the thinkers who seized the disciplinary moment. I will also leave the parallel story of anthropology, which is closely bound up with the "culture concept," to others.[1]

1 STATISTICS, HISTORY, AND THE SOCIAL QUESTION

The immediate context of the disciplinarization of sociology was the transformation of two fields, statistics and history, which shed large chunks of content as they

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[1]In this volume, especially the chapters by Risjord, Jarvie, and Wylie.

took their current shape. The principal body of thought that was excluded from the discipline of history, for example, was philosophy of history. The philosophy of history had been provided with an influential exposition in Robert Flint's *Historical Philosophy in France and French Belgium and Switzerland* [1894], which a few years later would have been called social theory and have been part of sociology. Several founding figures of American sociology were from history, such as Albion Small, the central figure in the creation of the influential department of sociology at the University of Chicago and the editor of the leading sociology journal, *The American Journal of Sociology*, who had been trained as an historian at Johns Hopkins, the first American university to embrace the German model of graduate education. The history done at Hopkins during that period included work that was very much like what later came to be understood as sociology: a major project of the time was a study of cases of cooperative ventures. The principal product of Johns Hopkins' social science during this period was a multi-volume series of studies on cooperative production and profit sharing, which were important responses to the so-called "social question" of the period, the problem of the rapidly expanding working class. Franklin H. Giddings, the founding figure at Columbia, was an economist, who broke in with a study of profit-sharing and, as a journalist, had written on indexing inflation. The American literature on labor, labor statistics, profit-sharing cooperatives, had analogues elsewhere, and these were important to the circumstances in which the discipline of sociology was institutionalized. In the period of the "first globalization" of 1880–1905 parallel institutional structures developed, notably bureaus of labor statistics, which shared methods, and, along with censuses, shared computational techniques, notably the technique which led to modern data processing technology in the form of digital punchcards, Hollerith cards, whose first successful application was in competition with the colorchip mechanized counting technologies employed by the pioneering Massachusetts's bureau of labor statistics (which published Giddings's first study, under its director Carroll Wright).

Bureaus of labor statistics were official bodies in the states of the United States and eventually in the national government, and in England and Europe. They operated as a social analogy to the geological surveys that had been established in the same places in the previous half-century and, like these surveys, published bulletins. The bulletins contained not only the reports of statistical studies of labor but qualitative and opinion material on the cooperative movement. As in the geological surveys, much of the circulation took the form of exchanges with other survey bodies, so the bulletins provided a means of international communication. This body of official statisticians incorporated a methodological tradition, derived from the statistics movement of the 19th century that had been organized around international congresses, which in part was concerned with "the social question" but developed technically particularly in relation to the problem of suicide [Porter, 1986].

Statistics in the older sense of these world congresses, and indeed of this tradition, was a substantive discipline rather than a branch of mathematics. Ordinarily,

the product was commentary on descriptive statistical reports, with occasional gestures toward the idea of underlying laws (cf. [Mayo-Smith, 1910]). One strain in this tradition that was more ambitious: it asserted that statistical studies which produced tables, typically of rates kind, led to or pointed to underlying laws, but failed to explain how they did so. Adolphe Quetelet, one of the central figures in the congress movement of the 1840s and 50s, had developed a complex analogy between the idea of the *homme moyenne*, the hypothetical individual who was the embodiment of the statistical mean, and the center of gravity of planets, whose perturbations he likened to the statistical variations in rates over time. And at the end of the 19th century Gabriel Tarde proposed a theory of imitation to account for the way in which statistical patterns changed by spreading from geographical points. Yet as a discipline this older form of statistics failed to make the transition to disciplinary status, either in Europe or in the United States. Although it continued longer in Germany in association with labor statistics, with Catholic social reform inquiry, and to some extent on its own as a social or labor adjunct to economic statistics in the German university,[2] it was nevertheless kept apart from the new pure science of sociology as it was established in Germany. German sociology had a different philosophical rationale, and was focused typically on the philosophical problem posed by Georg Simmel at the turn of the century: what is society?

When sociology as a topic was reassembled out of the bits that were not taken by economics and history, it incorporated a great deal of the social statistics tradition as well as the theory of history, which was rebaptized as a social theory. In both instances, the incorporation involved methodological and metatheoretical regrounding, which, in the case of social theory in sociology, involved the construction of a new genealogy in which Auguste Comte, the originator of the term sociology, played a large role. The methodological ideas of the social statistics movement, which had been opposed to Comte and which Comte opposed, were also incorporated. This new disciplinary construction thus involved conflicting elements, and much of the methodological writing of the time, including the methodological writing of Émile Durkheim and Max Weber, as well as the less well-known sources of mainstream American sociology and its critics, was concerned with reconciling these conflicts. Both Durkheim and Weber, in different ways, were concerned both with statistics and with claims that amounted to a surrogate for universal history (cf. for Weber, [Mommsen, 1977, 1-21]), as were virtually all the figures discussed in this chapter: Georg Simmel, Franklin H.Giddings, Charles Ellwood, and Talcott Parsons. The range of issues of course went far beyond these, especially the other issues of disciplinary relations, with biology, psychology, the normative, ordinary language, ethics, legal philosophy, liberalism and anti-liberalism, and economic theory to name but a few. But the fundamental conflict between science-like fact and the problem of large scale historical truth inherited by "social theory" reappears in many guises.

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[2]Much of the content of the famous journal *Archiv for Sozialwissenschaft und Sozialpolitik*, for example, consisted of such studies (cf. [Factor, 1988]).

1.1 Durkheim and the Statistics Tradition

Émile Durkheim provided the earliest and most coherent reconciliation of this fundamental tension, and it is striking how similar the lists of intellectual sources and problems actually were between these principal solutions to the common problem of constructing a "science" out of the materials and topics in question. Durkheim took over one of the best developed topics of the older social statistics tradition when he decided to deal with the problem of suicide in terms of his new methodological conception of sociology. This conception incorporated and critiqued elements of Quetelet, J. S. Mill, Comte, and neo-Kantianism in its French form (deriving particularly from Charles Renouvier and his notion of representation), and represented not only a continuation of but also an attempt to realize the ideal of the social statistics movement: to eventually go from statistical observations of rates and stabilities of rates to the underlying explanatory laws.

Under the French system of academic patronage, Durkheim, once he secured his position in Paris at *L'École Normale Supérieure*, had the advantage of being able to assemble a body of talented but dependent protégés. They were set to the task of writing in a Durkheimian fashion about particular topics of interest from a Durkheimian standpoint, writing reviews of books in sociology and related disciplines which were rivals or which could potentially be incorporated, and to establish claims upon topics for the new discipline. Durkheim's vehicle for doing this was *L'Annee Sociologique*, which provided an outlet for publication for his protégés. Under his strong editorial hand, this periodical assured the consistent application of his methodological ideas and his (imperialistic) idea of what sociology was.

Durkheim began his methodological text, *The Rules of Sociological Method* ([1895]1982) with a central philosophical issue. If sociology is to be a science, what are its facts? He borrowed from his own philosophical teachers (notably Émile Boutroux) the notion that every science has its own distinct class of not only laws but facts, and borrowed from Quetelet the specific model of facts, namely that rates of sufficient stability were to be treated as facts. Curiously, the concept of the stability of rates was essentially dead among statisticians at the time Durkheim made it his own. Advances in the understanding of the combinatorial mathematics that produced the joint distributions of the kind of rates, such as age specific suicide rates, summarized in standard (Halley) tables, showed that those rates did not have any special, unexpected stability. Durkheim, in contrast, used these rates as a model for understanding non-statistical social facts, such as stable and repeated rituals, folk sayings, and even laws. But what Durkheim did with these facts was to reconceive them as indices, but imperfect ones, of underlying realities that are not directly accessible but are the true subjects of the laws of social science and the true determinants of social phenomena.

This way of formulating the problem is reminiscent of the tabular statistics tradition, which routinely gestured at the notion that there would be payoffs from the collection of statistics in the form of future knowledge of underlying laws. But

with the exception of Quetelet himself, who developed a complex analogy between laws about stable rates and the orbit of the planets, it had been only a gesture. Durkheim faced the same problem: there was no obvious way to get from the kinds of stable rates which statisticians in the 19th century collected to "laws" in any familiar sense. Durkheim himself attempted this task only once, and then used the attempt as a kind of model and exemplification of his conception of sociology rather than as a beginning of a large scale program of analyzing statistical rates. The text is *Suicide* ([1897]1951), which, despite its status as a classic of social science, was for his successors, and even his students (cf. Halbwachs *The Causes of Suicide* [1930]1978) not a paradigmatic work in the sense of an exemplar that would be used as a model. Today the work appears as a cleverly contrived empirical demonstration of the plausibility of what is best understood as an ontological thesis about the existence of social reality beyond the level of the psychological and the individual (cf. [Turner, 1996]).

The reasoning in this text depends on and exemplifies Durkheim's conception of what, for him, was the paradigmatic social fact: obligations. Obligations were at once psychic and external. They were psychic in the sense that they were experienced in the form of promptings by the conscience. As causes, these promptings were psychologically similar to the promptings of individual desire, but from a different source. They were external in the sense that they were experienced as something apart from the desires, beyond the will of the individual to change, and, Durkheim argued, derived from collective life. Thus they were psychic and collective, a "*conscience collective.*" The collective character of these promptings or constraints was something of a mystery because the psychic mechanism was problematic. They were nevertheless collective in a banal factual sense: they were not universal, but were specific to particular collectivities, such as the nation or "domestic society," the family, and their psychic force varied with and was dependent on the strength, frequency, and character of the social relations in question. Rituals, such as participation in religious life, strengthened them. Suicide was the result of imbalances between obligations and individual desires in two dimensions, with respect to integration and regulation, later neatly restated by Mary Douglas as "group" and "grid" [Douglas, 1982, 3-4; Ostrander, 1982, 17-18]. Where the obligations were minimal, there was under-regulation, or *anomie*, or in Douglas's terms, the social relations were low in grid. The result of lack of regulation, for example easy divorce, is that wants become unbounded, producing unhappiness [1951, 253]. Where social contact or integration was diminished, for example among Protestants, whose religious life fails to produce strong collective states of mind with as great a consistency as that of Catholics [1951, 170], the psychic force of the obligations was weak. In Douglas's terms the social relations were low in group. Where the group was omnipresent and blotted out individual psychic life, as in the military, the balance was also lost. Each imbalance conformed to an identifiable statistical pattern of differences in suicide rates.

Durkheim's analytic strategy in this text relies on the preexisting methodological tradition about cause and statistics that was established in Mill's *System of*

Logic ([1843]1974), but rejected Mill's own understanding of the role of probability in favor of the method of concomitant variation, which Mill had thought was inapplicable in social science. Durkheim wanted to argue that statistical relations which took the form of perfect parallelisms with respect to increases or decreases in rates between two variables, such as suicide and some measure of social integration, the seasons, temperature, or the rate of mental illness could be treated as genuine laws, as examples of concomitant variations. Those relationships which fail to exhibit perfect parallelism, he argued, could be dismissed as failing to establish the existence of law-like relations. The text itself consists of tests of numerous hypotheses using these criteria, in which the best-known relationships in 19th century social statistics, such as those between climate and suicide, were shown to be irregular and thus not properly nomic.

By the time of the writing of *Suicide* and *The Rules of Sociological Method* ([1895]1951), the statistical core of this ambitious argument was the claim that psychological explanations of social facts were invalid, because the relevant facts consisted of variations between people or groups who were "psychologically" the same. For example, there were regular and significant differences between countries and regions with respect to suicide rates in relation to such variables as religion, and there were significant differences with respect to such things as marital status. Official statistics, however, classified suicides in terms of individual "causes" that had no connection with these differences. Neurasthenia, for example, which was commonly cited as a cause, did not vary in a way that corresponded to variations in suicide. Thus the explanation of suicide in terms of psychological traits, such as neurasthenia, was understood to be incomplete or false — incomplete if there was no account of why suicide-producing neurasthenia varied socially, false if the explanation amounted to essentially circular characterizations of the suicide victims' psychology, in which the fact of suicide was taken as a sign of neurasthenia. This neatly confirmed Durkheim's basic idea, that the psychic forces that account for the patterns are not individual but collective.

The only relationships that do prove to be nomic are a series of relationships that Durkheim himself provided. Even these, as it happens, are perfect parallelisms only in a very peculiar sense which Durkheim somewhat inconsistently applied so as to favor his own laws. He allowed for the correction of laws in the parallelisms that result from interfering factors in his own cases but made no effort to save, for example, the climatic hypothesis by allowing its exceptional cases to be explained away. The apparent inconsistency in treatment is perhaps justified, because it is apparent that for Durkheim himself the important consideration in connection with the laws is not Millian but Baconian [1998]. Durkheim wished to use these successful identifications of parallelisms and not to assert that they directly represent causal relations, but rather to argue that they are what Bacon called luminous instances, or places where the operation of underlying causal reality shines through the mass of interfering and obscuring variations that are characteristic of social phenomena.

Durkheim's argument recalls the passages in Mill's *System of Logic* in which Mill concedes that the application of the method of causal inference in the social sciences is ordinarily confounded by the sheer complexity of social phenomena and their causal relations. Durkheim in effect accepted this reasoning, but argued that in some instances the underlying social realities which are governed by genuine laws shine through. Durkheim had already constructed a critique of Mill's account of causal analysis which was realist in character. He argued that the true task of casual analysis was the identification of real underlying causal structures, something that Mill's methods, which worked to reveal underlying causal structure if and only if the categories selected for analysis happened to correspond to the genuinely causal category, failed to assure. It is this realism that differentiates Durkheim from Mill. But the meaning of Durkheim's social realism has been in dispute ever since he wrote.

1.2   What was "Real" for Durkheim?

Although much about Durkheim's methodology and philosophy remains contested, especially with respect to his ontology or realism [Jones, 1999],[3] his "functionalism" or teleology,[4] the implications of his use of the term "representations" in a fashion derived from Renouvier [Stedman-Jones, 2001], and his discussion of categories,[5] current historical scholarship on Durkheim gives us a Durkheim who is dramatically different from the caricature of a functionalist and positivist that was endlessly attacked, especially in British sociology, in the dispute over "positivism" of the sixties and seventies. The issue of teleology is the most straightforward. Durkheim was regarded by many of his readers as a "functionalist," and he did employ the term "function" repeatedly. This need not mean that he accepted the idea of a pattern of "functional" explanation or analysis that is distinct from causal analysis. In a footnote to *The Rules of Sociological Method* he says this: "we note that, if more closely studied, this reciprocity of cause and effect could provide a means of reconciling scientific mechanism with the teleology implied by the existence and, above all, the persistence of life" ([1895]1982, 144 n4). This suggests that he rejected or anticipated the rejection of teleological explanation, and that he supposed that teleological explanations, if adequate, were incomplete causal explanations which, if fully developed, could be analyzed into causal mechanisms involving reciprocity between causes (an example of which would be feedback mechanisms, in which the changes in outputs produce changes in the external world which change inputs and then in turn change outputs). These explanations appear to be "teleological" only if the causal reciprocities are not specified.[6]

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[3] For further discussion of the philosophical issues, see Zahle and Little in this volume.

[4] For further discussion of functionalism, see Kincaid in this volume.

[5] This is the theme of the chapter by Schmaus in this volume and [Schmaus, 2004].

[6] The extensive muddles over teleology, necessity, and related concepts are discussed in Jones. Boutroux's criticism of Durkheim's use of "necessity" in *The Division of Labor* is particularly revealing as a motivation for Durkheim's later care with respect to these usages [Jones, 1999, 160].

Durkheim's "social realism" is a more confusing domain. The arguments of his major texts, notably his dissertation (*The Division of Labor in Society*), *Suicide*, *The Rules of Sociological Method*, and *The Elementary Forms of Religious Life*, established the "social" character of many phenomena, but did so in accordance with a theory that was deeply problematic, and which Durkheim himself altered as his views developed. We may begin with the "social realism" of his earlier writings. These asserted the autonomy, in the sense of explanatory irreducibility, of a certain class of facts, "social facts," the orderliness of this domain of fact, and the consequent reality of the collective theoretical entities that the explanation of these facts required. This reasoning was connected to a philosophical argument, found in Durkheim's mentor Boutroux, but also prefigured in Comte, to the effect that each science in the hierarchy of sciences had its own laws. Thus, according to Durkheim's argument in *The Rules*, the laws of sociology were laws governing the contents and operations of the collective conscience or consciousness.

These contents were understood to be simultaneously casual and representational — indeed to consist of representations whose combinations were governed by psychological-like laws. Durkheim avoided the problem of explaining precisely how this would work. Because both collective and individual consciousness had the same type of content and shared the mind, which was thus duplex, partly social and partly individual, the problem of being simultaneously causal and representational was shared with psychology. Durkheim's manner of describing the laws in question indicated that he meant to conceive of the connections between representations as similar to logical connections, i.e., concerned with the combination of representations, consistent with the neo-Kantianism of Renouvier (cf. [Stedman-Jones, 2001]). The idea of the substrate of society, the level of causal reality in which causal social processes subsisted, which in *The Division of Labor in Society* ([1893]1964) he had understood as the way individuals are grouped, also came to be more explicitly identified with the workings of the "collective consciousness" or "collective conscience" (cf. [Nemedi, 1995]).

By his later claim that "society is made of representations" he meant that the categories constitutive of cognitive experience in individual thought were themselves also constitutive of the distinctive social orders in which they appeared and were the basis of sociality. Durkheim then focused on the problems of the diversity and origins of distinctive categories, particularly those categories involving obligations and collective rights. This line of argument about categories could of course be turned onto science and social science itself. Indeed the implications of this argument begin to be drawn out in Durkheim's essay with Marcel Mauss on primitive classification ([1903]1963) in which Durkheim suggested that the categories of the natural world, including causality, are derivative of the constitutive categories in the social world generally.

The significance of this way of understanding the problem of society and social diversity for 20th century sociology and anthropology was enormous, but its influence has mostly been indirect, except in France itself, where there is a strong interaction between sociological and philosophical ideas about constitutivity. The

neo-Kantian tradition was extended by reference to Durkheimian ideas as in such texts as Henri Bergson's *The Two Sources of Morality and Religion* [1935] and Célestin Bouglé's *The Evolution of Values* ([1926]1969). The affinity of this approach to Alexandre Koyré and Jean Piaget, its transformation into "structuralism" by Piaget and Claude Levi-Strauss, and its subsequent development by Michel Foucault and Georges Canguilhem is obvious, and these ideas have appeared in various forms in American-Anglo philosophy of science, notably in Ian Hacking where they have been applied to social science and indeed the history of social science itself [1990]. The applications have characteristically served to expose the relative or historical character of constitutive assumptions, as in the case of Gaston Bachelard, in a way that served to undermine any sense of the validity or trans-historical merit of any existing scheme of intellectual categories.

Nevertheless, much of the history of the Durkheimian movement itself consisted of the abandonment of the specifically methodological claims, and of the psychology and ontology of the collective conscience in its original form, though this was done in an inexplicit and incomplete manner. By the thirties, even Durkheim's own students, such as Halbwachs writing on suicide, had retreated to the core argument, which Halbwachs understood in terms of the notion of "social" influences on suicide, for which there was a strong and unchallenged body of correlational fact in the form of systematic differences in suicide rates of persons in different "social" categories. This retreat left open the question of what the contents of "the social" were, while simultaneously establishing a strong presumption that there was something irreducible and ontologically distinctive that corresponded to the notion of "the social" or "society." Durkheim's students, including Halbwachs who also wrote on such topics as "collective memory" [1992], typically reasoned that this something amounted to collective ideation of some kind. This was a view shared with German thinkers, who amalgamated the theory of "objective mind" to the category of the social, thus making the basic fact of society into a fact about its shared cultural content (cf. Freyer (1923]1988). Marcel Mauss, Durkheim's most distinguished student, turned to anthropological material, and substituted the concept of practice for the problematic machinery of "collective consciousness," a substitution which allowed him to describe cultural diversity in terms of the distribution of practices and beliefs in social groups.[7] Mauss's notion of practice, exemplified by his notion of techniques of the body, reappears in the writings of Pierre Bourdieu, which characterize a practice as an ordering structure of dispositions ([1972]1977). However, in Bourdieu practice is given a teleological interpretation in terms of the reproduction or maintenance of social domination by a group, and in this form appears in cultural studies and cultural sociology.[8]

There is, however, a connection between Durkheim and the later literature on collective intentionality which does not reflect this pattern of dissociation from

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[7]The difficulties of separating the notion of practice from a collective ontology are nevertheless considerable. Practice theory is discussed in this volume in the chapter by Rouse, but see also Turner [1994] and Schatzski [1996].

[8]The development of these fields is discussed in Zammito in this volume.

the idea of collective consciousness. In 1926 the American philosopher Roy Wood Sellars wrote an introduction to his wife's translation of Celestine Bouglé's book *The Evolution of Values* ([1926]1969). To understand the connection between this text and later "Sellarsian" ideas about normativity requires a discussion of Durkheim's ethics, which is at once naturalistic and normative. Durkheim was a "relativist" about norms, but his was not an individual relativism, in which individuals "choose" their values, since the ordinary individual was of necessity born into a society made of representations and constituted himself out of these representations or categories and experienced them as real constraints or as a real categorized or represented reality. These were real obligations, but were misunderstood by ethical thinkers and religious thinkers, and indeed ordinary people, with respect to their character as "social facts." For Durkheim this meant that any sort of positive ethical theory was delusive and that the only feasible replacement for it was a kind of science of morality [1920]. Intellectual intervention in the reform of morals was possible only in times of moral change in which the categories were themselves becoming reconstituted in a period of collective effervescence of the flux of moral ideas and through a new social fusion. But this could only be accomplished indirectly, so to speak, by contributing to the formation of new collective categories with moral force, a casual process that philosophical ethical theory was at best incidental to. This is the thesis that appears in Bouglé ([1926]1969) and is explicated by the elder Sellars.

Sellars formulated this in terms of the slogan "values are collective because imperative and imperative because collective" ([1926]1969, xxxv). Sellars called this Durkheim's "social naturalism," and reiterated Durkheim's objection to monistic transcendental idealism, namely that it cannot account for the diversity of morals ([1926]1969, xxxi). The "naturalistic" solution solves the problem of objectivity by locating value not in the things valued but in the social medium. Consistent with his conception of social reality as genuinely causal but cloaked and obscured by false conceptions, Durkheim argued that primitive religion, which consisted of solidarity-producing collective rituals, was the embodiment of a kind of systematic error. The primitive concept of God served as a surrogate for the misunderstood and misrecognized reality of our dependence on society, so that, in effect, the solidaristic rituals of the Australian aborigines were about society itself, not only because of the solidarity inducing consequences of the rituals, but because society is God-like in its relation of superiority and causal priority to us as individuals. This was an argument which had a close relation to Comte, who also argued that the great lesson of sociology was the fact of our dependence. Durkheim took this for granted, and reasoned backward to account for religion as a primitive partial apprehension of this truth. This account also allowed Durkheim to explain why representations of the "superior reality," which we experience as morality or "hyper-spiritual" forces, enable them to be experienced as external and constraining (Bouglé [1926]1969, 143-4). They do so because they represent, imperfectly, the real constraint of our dependence on society and of society itself.

One may ask whether Durkheim's ethical argument can be salvaged without his notion of collective consciousness, which Bouglé continues to employ. One issue is the question of why, in Roy Wood Sellars's slogan, "values *are* imperative because collective," which is to ask why collectiveness implies imperativeness. Durkheim, who defines social facts in terms of "constraint," answers this by definition. But the definition is part of the package containing the notion of collective consciousness as a real force, the psychological model of *homo duplex*, and thus an ontological commitment to something like group mind. Here a connection with the younger Sellars becomes clear. Wilfred Sellars, in his "Imperative, Intentions, and the Logic of Ought" [1963] attempts to answer this question about the imperativeness of the collective without appealing to group mind by analyzing statements of the form "we disapprove of women smoking, but I do not" as non-contradictory statements distinguishing the "collective intention" of disapproval and the individual intention of approval, treating both as intentions that are self-binding.[9] But this also raises the question of whether the "collective intention" is merely a descriptive fact which is not binding in and of itself.[10]

2   "MAINSTREAM" AMERICAN SOCIOLOGY

The Durkheimian school was embroiled in controversies which other French sociological movements, notably those associated with labor (LePlay) and official statistics (Tarde), and lost key members in World War I, but dominated the university system through its central position in Paris, and its institutional and intellectual domination of anthropological inquiry, a position of power prolonged by American financial support. Where it lacked power, it also lacked influence, especially in German and American sociology, which operated under quite different institutional conditions and with different disciplinary rivalries and divisions of labor, particularly with philosophy.

American quantitative sociology, as a distinctive enterprise with something resembling a "paradigm," began to take a distinctive form in the 1890s and came to dominate the field in the first three decades of the 20th century under the leadership of Franklin H. Giddings, the first professor of sociology at Columbia (appointed in 1896). Giddings can be credited with inventing the account of the relationship between theory and statistical data which has been dominant in U.S. sociology. Like Mill and Quetelet, whose works he criticized, Giddings put the actualization of any full "resolution" of the problem in a fully empirical theoretical sociology off into the distant future. He was, nevertheless, eager to apply the methodological lessons being retailed by contemporary scientists (or by contemporaries in the

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[9]This paper is discussed from the perspective of the philosophy of social science in Turner [2003c].

[10]For an attempt to restate Durkheim in terms of collective intentionality, see Gilbert [1994]. For a more general philosophical introduction to Durkheim's ethics, see Miller [1994; 1996]. For a discussion of collective intentionality and the philosophy of social science, see Zahle in this volume.

name of science) to the question of what empirical inquiry can warrant in the
way of theory, and in the 1890s this meant Ernst Mach and Karl Pearson. From
Mach, Giddings took the idea that mechanics is nothing more than description
and concluded that "no other branch of knowledge can claim to be more" [1901,
45]. This prepared him for Pearson's similar, but even more extreme, message that
not only was developed science no more than description, but also that the laws of
physics themselves were not, properly speaking, descriptions. Rather, they were
idealizations. Pearson himself regarded the ideas of "cause" and "explanation" as
animism. Thus, Giddings's philosophical point of departure was unusually strin-
gent: the idea of theory itself had only a tenuous hold in the conceptions of science
on which he relied.

Giddings's embrace of these doctrines created obvious difficulties for him as a
theorist, which he resolved in a Pearsonian fashion. The logical status of socio-
logical theory, as Giddings explained it, is defined by its place in what he called
the three "normal" stages of the scientific method: guesswork, deduction, and
verification. The three stages are a modification of Pearson's stages of ideological,
observational, and metrical, which Giddings quoted in his lectures, and which it-
self was a modification of the Comtean stages, taught to Pearson by a Cambridge
librarian who served as his mentor. Giddings gave the following formulation:

> Science cannot, as a distinguished scientific thinker said the other day,
> even get on without guessing, and one of its most useful functions is to
> displace bad and fruitless guessing by the good guessing that ultimately
> leads to the demonstration of new truth. Strictly speaking, all true
> induction *is* guessing: it is a swift intuitive glance at a mass of facts
> to see if they *mean* anything, while exact scientific demonstration is a
> complex process of *deducing* conclusions by the observations of more
> facts. [1920, xvi-xvii], emphasis in the original)

His own work, he hoped, would enable his readers "to see that much sociology
is as yet nothing more than careful and suggestive guesswork; that some of it is
deductive, and that a little of it, enough to encourage us to continue our researches,
is verified knowledge" [1920, xvii]. The solution to the puzzle of the relation
between statistical sociology and social theory this suggests is that "speculative"
social theory is a source from which the sociologist may take basic concepts to see if
statistical data "mean anything," then deduce conclusions from strict formulations
of these guesses and test them on "more facts," thus gradually adding to the stock
of "verified knowledge," which thus consists of the accumulation of statistical
results that serve, if not to answer theoretical questions, to replace theoretical
"answers" with metrical descriptions of relations.

The novel assumptions that emerged in the writings of Giddings and his stu-
dents are separable into two areas. The first contains what we might call a theory
of statistical explanation, which held that the processes governing the properties
of interest to sociology were "causal" and could be approached by statistical meth-
ods which were the result of an amalgam of Pearson and G. U. Yule which could

serve as a substitute for experiment. Before becoming more or less standardized or conventional, and indeed largely tacit, this assumption went through a number of formulations; its various conceptualizations were designed to explicate the statistical practices of correlation and partial correlation, and to support the claims that partialling was a practical equivalent of experiment and that correlations were "generalizations." The second contains what might be called a theory of measurement, which held that the properties of interest to sociology were subject to significant measurement error, that they could ordinarily be measured in a *variety* of ways which *imperfectly correlated* with one another, and that their status as "measurables" could be established by common consent to the substitution of numbers for words.

2.1   Cause and Correlation

In the 1911 edition of the *Grammar of Science* Pearson argued that the supposed difference between cause and correlation is merely a matter of degree: the difference between the laws of physics and the relations between, for example, parental and adult children's stature, is the quantitative fact of degree of variation; but even observations in physics show *some* variation. It is hopeless, he thought, to claim that the quantitative degree of variation found in various relationships represents a qualitative difference (cf. [Turner, 1986, 219-24]), and, accordingly, he urged the abandonment of the distinction between cause and correlation. The price of this reasoning is high, because almost everything is, as Pearson himself insisted, correlated with everything else. Giddings and his students struggled, alongside their biometer colleagues, with the question of the proper middle ground between accepting the radical collapse of cause into correlation and adhering to the traditional notions of cause and law, and in searching for an adequate mode of formulation for the compromise. Giddings himself made several striking contributions to what was to be the ultimate resolution of the problem in sociology. In his *Inductive Sociology* [1901] the discussion takes the form of correcting Mill (whose discussion of complexity and the problem of disentangling causes had the effect of denying the possibility of significant causal knowledge in the social sciences) by redefining the task of social science. He remarks that although "it is not always possible perfectly to isolate our phenomena, as, for example, in Mill's familiar example of the effect of a protective tariff, we may nevertheless be certain that we have found the only sufficient antecedent if we know we have found the only one commensurate with the results" [1901, 17]. The paradigmatic means of establishing this is "systematic observations of the resemblances and differences of occurrence in a series, and of magnitude" [1901, 16], meaning correlational analysis. The terminology of resemblances is Mill's and John Venn's, the idea that the mathematical expression of a comparison between "classes or figures," or a correlation coefficient is "always equivalent to a generalization or law" is taken from Pearson, but also exemplified in anthropologist Edward Tylor's "much simpler" diagrammatic method, as used in his studies of the relation between matrlineality and matrifocal residence patterns [Giddings, 1901, 283].

If we begin with Giddings's 1901 comment on Mill, we can understand the particular dilemma he was addressing. To the extent that we want to retain the possibility of isolating "sufficient antecedents" or major causes, it is not sufficient to point to correlations, however high. In 1921 Sewall Wright, USDA scientist and animal geneticist, described the problem in his classic paper on path analysis:

> Birth weight and gain after birth are highly correlated. Here neither variable can be spoken of as the cause of variation in the other, and the relation is not mathematical. They are evidently influenced by common causes, among which heredity, size of litter, and conditions which affect the health of the dam up to the time of birth at once come to mind. [1921, 560]

Thus correlation, however high, does not assure cause. Sometimes prior knowledge of experimental evidence suffices to warrant the claim that a "causal" but imperfect relation exists, but this evidence does not eliminate the possibility that some or all of the observed correlation is a result of "common causes." In practice, then, interpreters of natural experiments are left with a negative definition of cause — a causal relation exists where there is no common cause and where there are some rough grounds for supposing a causal relationship exists.

Lacking experiment, sociology was left with this negative definition: a cause is a statistical association which is not spurious. Of course this definition has the problem that defining spuriousness itself requires an appeal to the notion of causality, whether spuriousness is the result of common causes or confounding. Giddings's positive version of the notion of causation, as presented in his methodology textbook of 1924, was this: "If, in a large number of cases, we find a high correlation of the occurrence frequencies of the result attributed to it, the presumption of causal nexus is strong" [1924, 179]. Giddings conceded that there was no *rule* which distinguished spurious correlations from causal ones, but he identified a key symptom: a high correlation "points to the major causal nexus," when it persists "while other factors and correlations come and go." A correlation can be presumed not to be a mere "arithmetical accident" under this circumstance when the hypothesis of a common cause can be excluded and where there are no highly correlated causes which may be confounded with the putative cause [1924, 180].

One suspects that Giddings may have been following the widely used methodological text of his youth, W. S. Jevons's *Principles of Science* [1874], in accepting that this was as much as could be usefully said on the subject. Jevons himself had said that "no rule can be given for discriminating between coincidences which are causal and those which are the effects of law" [1874, 262]. Yet, as a philosophical conception of cause, the formulation of cause as non-spurious correlated sequence is not very satisfactory. As noted, one obvious difficulty is in the definition of spuriousness as the existence of common causes or confounding — the concept of cause which is to be defined also appears in the presumed definition. Worse, the definition produces a regress in making judgments about the existence of common causes because claims about their existence or nonexistence always depend on

claims about the non-spuriousness of the relations assumed to be common causes, i.e., the nonexistence of common causes that would relegate the alleged common causes themselves to the category of "spurious."

By mid-century, apologetic writings on behalf of sociological research tended to evade these questions by assimilating natural experiment to psychological experimentation,[11] a process to which Giddings contributed by calling correlational results "uncontrolled experiments" and to which Giddings's student F. S. Chapin contributed. By 1945, we find Paul Lazarsfeld, who picked up many of his methodological ideas from Samuel Stouffer, W. F. Ogburn and thus, like Chapin's student George Lundberg, a member of the third generation from Giddings, arguing against "the futile controversies as to whether or not a correlation is a causal relationship." According to Lazarsfeld, "the meaningful way to put this question is: To what degree is a given correlation equivalent to a controlled experiment?" [1945, ix]. The flaw in this way of putting the problem, which was also Yule's way in the paper generally regarded as the *locus classicus* of the concept of spuriousness [1895; 1896], is this: there is no way to know whether a given correlation *is* "equivalent to a controlled experiment." If the apparent relation is spurious, the concept of experiment would not be valid. But the claim that a relation is spurious itself depends on the analogy to experiment, leading to a regress that cannot be ended "empirically."

Giddings understood this defect of the analogy to experiment, and thus combined the use of the analogy with the informal or prudential idea that a "major causal nexus" will persist while other correlations come and go. He did not attempt to say more than this, perhaps out of scruples derived from Pearson and Mach, specifically the idea that causality was a feature of science that was useful at the stage of guessing and observation but would ultimately vanish. This combination of scruples and distinctions is still evident in his student Ogburn's writings in the thirties. By 1934, Ogburn would formulate these claims by saying that statistics had "limitations" as a method, especially when compared to experiment, the use of genuine controls. Statistics, particularly multiple regression and correlation, is sometimes a "substitute for the laboratory," but one of quite limited application in social science, primarily because of the number of relevant factors and the difficulty of measuring and obtaining data on them, also because of difficulties in assumptions, such as the assumption of linearity in the relationships [1934, 16]. Ogburn is quite Pearsonian in his separation of description and explanation:

> Statistical tables are only a framework in which the data may be examined, and a coefficient or curve is merely an abbreviation of the table. As to what the arrangement of figures means depends on what the author or reader brings to them in the way of association — very

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[11] The methodological influence of psychology on sociology was substantial but complex, and largely unrelated to the correlational tradition discussed here, which was temporarily superceded at mid-century. Some of the historical aspects are discussed in Turner and Turner [1990]. The psychology side of the issue has been discussed brilliantly in Danziger [1990], which is primarily focused on the shifting conceptual preconditions for psychological methods.

much as one gets the meaning of a political cartoon in a newspaper.
[1934, 17]

The terms must be "explained" and the relation must be "interpreted," but this
must be done on grounds extrinsic to the table. The grounds must be "scientific"
for the interpretation to be "scientific." How this is to be done is not made
clear in this paper, apart from Ogburn's repeating without citation Giddings's
"three steps" from hunch to hypothesis to verification. He reasons that statistical
evidence provides only a partial check on hypothesis, or to put it differently, a
check on the part which is contained in the statistical tables themselves, not on
the "interpretation" as such [1934, 14-15].

By the time of the great post-1945 expansion of sociology, these subtle philo-
sophical distinctions were simply forgotten. The "*ex post facto* design" (a term
invented by Giddings's student Chapin in 1937, cf. Chapin [1947]1974, 95) stood
on its own as a communicable practice or tradition (cf. [Campbell and Stanley,
1966]), and the problem of spuriousness became a problem the prudent analyst
learned to avoid by appropriate design, especially by controlling for "nuisance
variables" such as prior distributions of demographic traits in sample populations
that produce illusory relationships. Correlational analysis itself diminished in im-
portance. The standard technology of card sorters lent itself to contingency table
analysis, and it was not until the introduction of computers in the sixties that
correlational analysis regained its position as the dominant technique. It was not
until then that the issues with correlation became philosophically salient (espe-
cially with [Meehl, 1970]), though many of the issues were apparent from a series
of papers by Herbert Simon in the fifties (cf. [1954]), including one with Rescher
[1966].

2.2   Measurement

The measurement reasoning of Giddings's circle also marked a significant break
with "statistics" as practiced by their predecessors. *The Science of Statistics*
([1895]1910), the work of Giddings's colleague at Columbia, Richmond Mayo-
Smith, who was the major conveyer of the European statistical tradition to Amer-
ican students during his career at Columbia, where he was one of the earliest
members of the faculty of social and political sciences. Mayo-Smith, is, with re-
spect to measurement, a part of the 19th century moral statistics tradition. The
statistical material studied is exclusively the sort collected by the state, such as
vital statistics which Mayo-Smith comments on with a "sociological purpose." In
contrast, Giddings's first methodology book, *Inductive Sociology* [1901], attempts
something quite different: the measurement of "magnitudes" which derive from
theories of sociology as well as from terms in common use, such as "labor unrest."
The kinds of data he proposes to use are not very different. But they are used,
and conceived, in a distinctly new fashion.

The theoretical problems which concerned Giddings for most of his life were
the problems of forms of political association and the interaction between them,

social relations, and the formation of human personality types. It was a tenet of Spencerism absorbed by early sociology that each of these "evolved." Spencer was also concerned with problems of interference with evolution, and American sociologists of the period, such as William Graham Sumner, who remained within the broad confines of the idea of social evolution through competition, concerned themselves primarily with the problem of the brakes and limitations on social evolution. Sumner's *Folkways* [1906], for example, was an examination of the most fundamental and unchanging morals and customs of society, the mores, which constrained social evolution by limiting change to those developments consistent with the mores. Giddings, who came to sociology from a chair of political science at Bryn Mawr, concerned himself with the limitations of evolution that forms of political association imposed and with the constraints on political evolution which result from conflicts with primordial ties, such as kinship, and from the inadequate psychological or characterological evolution of personalities. In particular, he was concerned with such questions as whether Italian immigrants would undermine American democracy as a consequence of their psychological traits and their primordial loyalties.

One might investigate this impressionistically, by deciding if some group has some characteristic, such as "forcefulness" (one of Giddings's four basic psychological types), which is pertinent to the theoretical question of their capacity for sustaining particular forms of political association. But only systematic quantitative evidence in the form of magnitudes can enable the precise determination of correlations. One constructs a magnitude in this fashion:

> Suppose that we desire to know whether the men of Montana represent a type of character that might be described as forceful, but that we find no testimony, no record of personal observations, directly bearing upon our inquiry. We know, however, that by the general consent of mankind, men who follow adventurous and daring occupations are described as forceful. Turning then to the census, we learn that a majority of men in Montana follow adventurous and daring occupations.
>
> Accordingly, by substitution, we affirm that a majority of the men of Montana are of the forceful type of character. [Giddings, 1901, 27]

One might say that the 19th century statisticians, and Mayo-Smith, reasoned informally "by substitution" in their commentaries on facts of vital statistics, and one can also find plausible instances of the idea of measurement "by substitution" warranted by "the general consent of mankind" (or "face validity" in later parlance), but the combination of the two in an explicit model of hypothesis testing was not present.

Perhaps a genealogy of this concept of measurement, thin to the point of transparency, is unnecessary. Nevertheless, because the problem of measurement subsequently became so important to quantitative sociology, some background and some explanation of its absence from competing traditions, such as the Durkheimian, is relevant. The primary source of Giddings's innovation here was the matter of

race, one of the defining issues for Columbia at the time.[12] Giddings's student, Frank H. Hankins, makes the point in *An Introduction to the Study of Society* [1928] that "all the customary indices of racial difference, viz. stature, cephalic index, hair color, eye color, skin color, nasal index, hair form, alveolar index, etc. in fact extensively overlap one another" [1928, 95].

> Norwegians are obviously taller on an average than Japanese, but some Japanese are taller than many Norwegians. White and Negro cannot be distinguished by stature; nor by cephalic index; even as regards skin color and hair form the border areas of distribution overlap. It is this overlapping that makes it necessary to think of a race as a *group set apart by a complex of traits inherited together within a limited range of variability*. Since tall stature shades into short, long head into round, and dark complexion into light, it must be shown that with tall stature are found also a certain head form, eye color, shape of hair, etc. [1928, 96, emphasis in original]

This is the Boasian, and Pearsonian doctrine of race, on which Giddings also relies. The underlying idea here is that a race is not definable by a single criterion, but by correlated statistical distributions of several properties. Analogously, as Giddings, speaking in Pearsonian mode, explained: "the fact that every manifestation of energy is associated with other manifestations, every condition with other conditions, every known mode of behavior with other modes" [1924, 196] justifies the practice of using a wide variety of measures, each of which can be plausibly used as an imperfect surrogate for the variable of interest. The *imperfections* were then treated as measurement error, that is, as a well-understood problem of traditional statistics.

Giddings did produce some interesting models of analysis, even at this primitive stage. The speculative concept of "consciousness of kind," derived from Giddings's rejection of other explanations of groups suggests the following deduction: "Concerted Volition," including that which is expressed through forms of political association depends on sympathy or consciousness of kind. Hence, there should be a relationship (which he spells out at length, discursively, on the theoretical level) between the degree of social sympathy and what we might call political culture. He "verified" this by constructing an "index number" corresponding to a weighted formula based on the addition of these fractions: proportion of the numbers of native born of native parents; of native born of native parents and native parents of foreign born to foreign born; and this proportion to the proportion of colored [1901, 287]. The number was understood to correspond to the degree of homogeneity in the population. After calculating this number for each state of the union, he constructed three categories based on the scores. The relationship is verified by examining the lists of states falling in each category. In the highest index number

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[12]The chapter by Root in this volume deals with issues about racial concepts. The methodological lessons of these issues was crucial to later ideas about how to measure underlying traits with multiple possible indicators — -a common situation in empirical sociology.

category, he said "it will be observed that the states which are distinguished for a rather pronounced 'Americanism' in politics and legislation are chiefly found, as might be expected" [1901, 289]. In those states where the population was "neither perfectly homogeneous nor excessively heterogeneous," signifying the existence of more intellectualized ties as distinct from primordial group feelings, are to be found the highest degree of "progress and social leadership" [1901, 289]. Had he measured "Americanism" and "progressiveness" in some fashion, the association could itself have been measured statistically. Here, simple inspection of the differences in index scores between categories sufficed.

This little statistical association between heterogeneity and progressiveness provides a kind of paradigmatic example of the kind of social research that became conventional in sociology by 1935, and omnipresent in the fifties and sixties. It was motivated by some "theoretical" ideas about the political effects of heterogeneity. Heterogeneity was an abstract variable measured indirectly. The association was imperfect, but it confirmed and metricized a theoretical idea. By the twenties, with the infusion of Rockefeller Foundation money in support of the transformation of the social sciences into statistical disciplines and in direct support of statistical research, a significant amount of research of this kind was taking place. In Giddings's department the first dissertation using multiple regression was published in 1920, and a major effort using extensive partialling to determine causal order was published in 1924. The Rockefeller-funded Institute for Social and Religious Research supported a sufficient staff of statistical clerks to perform the tedious calculations required for this type of analysis, and produced a significant number of books using these methods.

2.3   The American Enemies of Quantitative Sociology

The critique of this particular form of the use of statistics and the identification of these usages with "science" and the "scientific method" developed parallel to the rise of quantitative, statistical sociology. The first synthetic thinker in this oppositional tradition was Charles Ellwood, one of the earliest American Ph.D.s in sociology and a student of Dewey and Mead.[13] The book that he published in 1933, *Methods in Sociology: A Critical Study*, represented a synthesis of the critiques that had been developed by that time. The personal background to Ellwood's rejection of narrow quantitative sociology is quite interesting, because it reaches back to his undergraduate teacher at Cornell, Walter Willcox, who was important in many ways for Ellwood and who was the author of the very first empirical statistical social science dissertation — *The Divorce Problem* [1891] — on a sociological topic in the United States. Ellwood was also a student in Berlin of Simmel, and this encounter proved formative as well, also for negative reasons. Ellwood subsequently rejected apriorism of the kind represented by Simmel, whose philosophical practice and influential example will be discussed in the next section.

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[13]Many of Ellwood's ideas, especially about culture, and his methodological critiques, were paralleled in Anthropology by Alexander Goldenweiser.

Ellwood first encountered these problems in the early 1890s with Willcox. Ellwood recalled much later his negative reaction to Willcox's own promotion of the special and unique validity of the statistical approach. But he also took from Willcox what he thought was a better idea, the offhand suggestion that perhaps the best way for the social sciences to advance was to pay more attention to the psychological. Those two elements, the critique of statistics and the importance of the psychological, in a way epitomized Ellwood's career, for he followed the hint of pursuing the psychological throughout his sociological work and also resisted not so much statistics (which he himself collected and employed) but what he considered to be a misapprehension about the importance of the statistical method in sociology.

To understand Ellwood's critique, it is necessary to start with some very broad contrasts. There is a general contrast between Ellwood (and his American peers and sources) and the European tradition in writing about these topics. Ellwood was not in any sense anti-science, nor did he reject the notion that sociology ought to have theoretical knowledge in the form of universal principles [1933, 103]. While he took "culture" to be central to the best available understanding of society, he did not, and indeed explicitly abjured, any explicit *a priori* account of culture or meaning, such as Hans Freyer's theory of objective mind, which would then serve as the special topic of a sociological science of culture. The nature of culture, for Ellwood, was to be understood by reference to universal considerations about the process by which the contents of culture were transmitted, especially the consideration that culture was produced and transmitted in what he called an interlearning process between individuals. In Ellwood's hands, and the hands of his most famous student, the "symbolic interactionist" Herbert Blumer, this claim precluded an autonomous (or ontologized) notion of culture (or for that matter of society).[14]

One can distinguish two basic ways of being scientific. One is to follow a method, the scientific method. The second is to follow no particular method, but to concern oneself with the substantive results of science and fit new extensions of science with established results. Ellwood rejected the idea of imitating natural science by extracting a "method" and applying it to social life. In his view, the social theorist was constrained by the biology and psychology of his subjects, and ought to be conversant with the science that was the source of the constraints, and indeed he himself was. For him, social theory is continuous with the *substance* of science, and is open to whatever "continuous" turns out to mean. This is what drove his account of culture. Learning and interlearning for Ellwood were the real processes in which culture was sustained, and in terms of which innovation and change needed to be understood. These processes were not inventions of sociologists: learning was taken from established psychology.[15] Ellwood was well

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[14]The relationship between Ellwood, Blumer, and symbolic interactionism will be discussed briefly below. It is discussed at length in a controversial work by Lewis and Smith [1980].

[15]One of Ellwood's later papers was an appreciation of James Mark Baldwin with whom he shared his evolutionary view of culture as interlearning [Ellwood, 1936], discussed in this volume

aware that society and culture were terms that had been given various definitions, notably, in the case of society, the organic analogy. His reasoning was that it could not be taken for granted that society, understood as an organism, corresponded to anything real or was necessary to the explanation of the things that we know pretheoretically about social life.

Ellwood rejected the idea, made fashionable by Durkheim and later Parsons, that there was some sort of autonomous subject matter of sociology that was free of biological or psychological constraints. The idea of sociology as an autonomous subject is an idea that depends on a methodological conception of science. In Durkheim this takes the form of the doctrine of treating social facts as things, and then subjecting these invented "things" to analysis using the methods of science. Ellwood understood the grounds for claims about the reality of collective representations, which was that they were in some sense to be understood as objective facts. But he argued that there was an equivocation in the use of "objective" that was fatal for these arguments. Durkheim, Ellwood says, was "only half-hearted in his objectivism" [1933, 32]. He sought to do away with psychic elements and the explanatory role of individual psychological phenomenon, but

> Instead of going on to construct a sociology in terms of the behavior complexes of the aggregate [i.e. to be like a behaviorist about society], he accepted the hypothesis of "collective representations," such as popular beliefs and social traditions. Thus, Durkheim's objectivism was tainted with subjectivism of the worst sort [and the kind that behaviorists objected to in psychology], for his hypothesis of "collective representations" transcends his definition of fact." [1933, 32]

This is to say that Durkheim pretended to be constrained by objectivist methodological considerations, but proposed a hypothesis that no such facts could establish, and which rested on such "subjective" facts as feelings of obligation, necessarily formulated in concepts that derived from ordinary language, which Durkheim inconsistently derided as inadequate for science. Ellwood's point is familiar to anyone who has read the critiques of Durkheim found in writers like Jack Douglas [1967] and Peter Winch [1958]. The only way into the identification of social facts is through "members'" concepts, i.e., something "subjective" in Durkheim's own sense. Durkheim himself is unable to avoid appealing to such concepts, for example, in his definition of suicide in terms of the subjective notion of intention. And this is a fundamental problem, because the claim to be made for Durkheim's conception of social facts is that they are special facts of a new type, not accessible to other senses.

Beginning as he did, before the arguments of his opponents had fully taken shape, Ellwood was hard-pressed to define the issues, and characteristically the methodological problems arose first in fairly arcane forms and sources,[16] or in

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in the chapter by Kincaid.

[16]The first textbook to define the basic methodological ideas and scientific aspirations of statistical sociology was Giddings's of 1924, which was based on a series of articles published in

sloganeering that was difficult to unpack and critique. Ellwood discusses, for ex-
ample, "Karl Pearson's aphorism that 'science is measurement' " [1933, 100], and
claims such as Ogburn's and Goldenweiser's claim that "the 'scientific' future of
the social sciences depends on their amenability to statistical methods" [1933,
100], which Ogburn, in a notorious episode, had caused to be engraved on the
facade of the Social Science Building at the University of Chicago in the form of
a quotation loosely derived from Kelvin [Bulmer, 1984]. The fact that the basic
model of methodology discussed in connection with Giddings was not the subject
of a more fully developed methodological rationale or "philosophical" treatment[17]
forced Ellwood to find examples of thinkers who articulated some approximation
of one. Weber, who faced the same problem, devoted his methodological writ-
ings to figures who had already become obscure or obsolete, such as the historical
school economists Wilhelm Roscher and Karl Knies, the legal philosopher Rudolph
Stammler, and the energeticist chemist Wilhelm Ostwald. These figures have been
preserved for current thinking largely by virtue of appearing in these methodologi-
cal writings. It was rarely the case that the proponents of the positions Weber and
Ellwood were criticizing stated their views in the kind of overt manner that lent
itself to analysis. Thus, in a strange way, the critique of positivism was compelled
to invent its subject, which in turn allowed proponents to disavow the positions at-
tributed to them and ignore the critics, a pattern which proved to be characteristic
of later debates, notably the German *Positivismusstreit* of the sixties.

   To sharpen the issue of objectivity in sociology, Ellwood turned to a student
of Pavlov named Zeliony, who had attempted, as the behaviorists in psychology
of the same era characteristically did not, to apply the methodological reasoning
they employed in constructing psychology to the problem of the nature of a science
of sociology, which Ellwood treated as a *reductio ad absurdum* of the implications
of the conception of science professed by social science objectivists. Zeliony is
a convenient mouthpiece, for he argues quite explicitly that, as Ellwood puts
it, "the task of natural science is . . . simply the description of observable
phenomena, the discovery of new phenomena, and finally the deduction of relations
of law between phenomena" [1933, 33]. This implies that appeals to concepts such
as ideas, emotions, beliefs, desires and values, even in descriptive contexts, are
illegitimate, and more generally that

> *the mind of another cannot be considered as a phenomenon, nor as a
> fact.* Consciousness must be ignored by the natural scientist, as it is not
> available for his observation, neither can it serve as a transcendental
> hypothesis [1933, 34].

This means that "the whole of modern sociology is full of . . . mistaken desig-
nations," since such notions as crime and family "involve or build on the psychic

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Social Forces.
   [17]To the extent that there was a "philosophy of science" literature on this topic in contemporary
American philosophy, it was skeptical about the project of a physics like social science (cf. M. R.
Cohen [1959]1978, 321.

side of individuals, and thus must . . . be barred from scientific sociology" [1933, 34]. This was of course Durkheim's claim as well, but one which, as Ellwood said, he failed to carry through consistently.

The structure of Ellwood's response to Zeliony's argument, and his extension of it to his contemporaries, is this. He frames the problem in terms of "adequacy," the question of whether this conception of science and method is adequate for the study of society. This is framed in part in terms of the question of what is the "essential" character or "nature" of social life [1933, 55]. In response, Ellwood defends a kind of realism about the social processes of interlearning discussed above (cf. [1933, 70-71]), which he takes to represent the essence of culture, including its psychic side and considerations of meaning. His characterization of "behaviorism" reflects its use as a term for the denial of any "reality" or "objectivity" to "subjective, non-material phenomena" or any "non-physical entities or processes" (unnamed source quoted in [Ellwood, 1933, 47]), a rejection which was also ordinarily extended to the objects of theoretical concepts. Ellwood argues that this denial is simply *a priori* and dogmatic, and anti-scientific to the extent that it "does not preserve the experimental attitude in the matter of scientific methodology itself" [1933, 51], especially by denying all value to "sympathetic introspection" in connection with participant observation. Supporting the claim that behaviorism is "adequate" requires "some metaphysical dogmatism" to the effect that the subject matter that behaviorism cannot accommodate is not real or that the only possible explanations are mechanical and deterministic [1933, 52]. He is able to repeatedly quote Pearson endorsing the idea that the only facts are sense impressions, a claim that points to a crucial ambiguity in the position he is criticizing, which seems to imply not only that psychic phenomena are inadmissible to science because they are not observable, but more radically that no theoretical concepts of any sort are admissible to science.[18] Ellwood is able to dismiss this as metaphysics.

This does not excuse him from defending his own claims about the essential character of social processes. But he does this by arguing that his account is "adequate" to the understanding of social life as revealed through the "historical" and "psychological" methods [1933, 77], results we have no *a priori* reason, outside of a metaphysical dogmatism, to dismiss, and then he turns to arguing for the inconsistency between the standard statistical approaches to social life and the philosophy of science that is presumed to underlie it. The denial of introspection, for example, is central to "behaviorism," but the statistical material which is studied by statistical sociologists includes such things as questionnaires (which, in effect, record the introspections of the respondent), personal interviews, and the study of historical records, which, as Ellwood put it, "imply something more than behaviorism, because none of these methods could be used in the scientific study of the behavior of animals below man" [1933, 59-60]. The behaviorist rejection of "concepts" and imagination is also inconsistent, Ellwood noted, since they are

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[18]Blumer was to attack this thesis in one of his own early methodological writings, "Science Without Concepts," which Ellwood cites [Blumer 1931; Ellwood 1933, 22].

themselves compelled to employ both. He concluded that a synthetic approach, which employs various methods in conjunction with one another, which would require the extensive use of "logic" or more generally theoretical reasoning, is the most desirable strategy for the field.

These arguments were to take a different form in other thinkers, notably in the continental tradition, and in the writings of later critics of what came to be known as "positivist" social science, so a few features of this particular argument bear notice. The first is that despite his appeals to "essential" features, Ellwood was not an apriorist who began with a fixed notion of the contents of sociology. He was a realist about social processes, but the claim to reality is grounded in a claim of explanatory utility and adequacy with respect to phenomena established through extant methods of inquiry. The rejection of these methods, he argued, is indefensible without reference to metaphysical dogmas. He embraced the idea of science, and although he spoke of the problem of meaning [1933, 56] and cited Werner Sombart on *Verstehen* [1933, 102], he avoided the claim that there is an autonomous realm of meanings. Similarly, though he rejected materialism as dogmatic metaphysics and appealed to a notion of "values" that he contrasted to the material, he did not think that this constitutes an ontological realm beyond the normal social processes of "interlearning." Indeed, his conception of ethics is similar to L. T. Hobhouse's notion of the rational good, in which the evolution of morals is accounted for as a product of learning [1921].

Ellwood had allies. He was engaged in a continuous three-way correspondence with Pitirim Sorokin and Robert MacIver, each of whom carried on the fight. Sorokin's *Fads and Foibles of Modern Sociology* in 1944 went beyond *reductio* to ridicule of the scientific pretensions of the various "new Columbuses" and quantophreniacs. It remained in print for decades. MacIver's *Social Causation* [1942] also identified Karl Pearson's philosophy of science as the source of the model of causal explanation that inspired such American quantitative sociologists as Giddings and especially their students Ogburn and Chapin, and attacked it on standard philosophical grounds. This book was also in print for decades and continued to provide a justification for the rejection of the use of purely statistical grounds for claims about causality.

These texts reflected the bitterness of the division in American sociology over quantification and its scientific status, a division which is crucial to understanding the later reception of logical positivism. The students of Ogburn and Chapin were prominent figures in American sociology during the period of the reception, and one of them, George Lundberg, author of the popular scientistic work *Can Science Save Us?* ([1947]1961), was a supporter of Carl Hempel's entry into American philosophy and a participant in the politics of the journal *Philosophy of Science*. Yet there was an important difference between the logical positivists and the older Pearsonian (and Machian) generation. The older generation was hostile to "theory" and theoretical concepts, especially in sociology. The logical positivists enabled their successors, in the generation of Robert Merton and postwar social psychology, to accept theoretical concepts. Indeed, the later generation

bent the restrictive notions of the role of theoretical concepts in logical positivism in the direction of justifying appeals to those unobservables that could be indirectly associated with measurement, such as attitude, the concept central to social psychology. Logical positivism thus served as a source for claims to the status of "science," and the logical positivist model of theory came to be invoked, or, in its own way, followed.[19] This was a two-way relationship: for some years at Columbia in the fifties, Paul Lazarsfeld and Ernest Nagel co-taught a seminar (with frequent participation by Merton) on these issues, and Hempel wrote on functional explanation in a work on sociological theory [Gross, 1959; Hempel, 1965, 303-30]. The issue was shifted by this collaborative relationship to the question of what type of theory was legitimate in sociology, a question leading, especially in the sixties, to an extensive literature critical of "positivism" in sociology, some of which, ironically, relied on logical positivist writings as a resource.

The hostility to theory that marked the earlier quantification movement also persisted among quantitative sociologists, and took various forms. So did many of the other issues raised by Ellwood, including the challenge of "realism," issues about the centrality of processes of social interaction to any realistic account of such abstractions as "society," the validity of such "methods" as participant observation, and the pervasive problem of the tacit dependence of quantitative sociology on "data" which was the product of under-theorized processes of social interaction in interviews and questionnaires. These issues were an important impetus to ethnomethodology[20] and symbolic interactionism, and eventually led to a large literature in defense of the qualitative methods, such as participant observation, that Ellwood had championed.

"Symbolic interactionism" as developed by Herbert Blumer, claimed to be based on the thought of George Herbert Mead, used many concepts from Mead, and kept Mead's thought alive in sociology far more effectively than in philosophy itself. The relationship between this movement and Ellwood is spelled out in a letter from Blumer to Ellwood in 1944, discussing his argument that the role and character of culture precludes the use of "natural scientific methods" in sociology. Blumer rejects the idea that the "cultural approach" is adequate. To him it represents "a case of forcing on human behavior an abstraction that has been derived from an imperfect study of the so-called static patterns of simple folk people; and that it operates to obscure the fact that human beings are organisms that are active, seeking, avoiding, and imaginative." He added that "what we need, I feel, is a new framework for analysis — a framework that will do more justice to the character of human beings as we recognize them to be and act in our everyday experience" [1944, 2]. He finds this, he says, in George Herbert Mead's "recognition that the act is built up in the course of its execution, and that in being built up it may be anything but a mere release of habit or of fixed established pattern" [1944, 3].

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[19]The chapter by Hage in this volume illuminates the idiosyncracies of this project of "theory construction," as well as the different perspective which practicing sociologists had on the problem.

[20]Discussed in Lynch's chapter in this volume.

The methodological critique developed by symbolic interactionism depended in part on this notion of the primacy of interaction, which is to say the interactive process by which the act is "built up." The result is very similar, in terms of social ontology, to Ellwood's notion of interlearning: "society" is no more than the fundamental, ongoing, process of interaction. The emphasis is, however, different: Ellwood was concerned with the persisting and changing results of interlearning, i.e., culture, its consequences for the agent, and its development, Blumer with the immediate and ephemeral character of the course of action itself.[21] Ironically, the importance of "culture" eventually reasserted itself within symbolic interaction (cf. [Becker and McCall, 1990]), and the emphasis on the creation of social reality in the immediate moment of interaction eventually diminished. But the idea of the primacy of "everyday experience" and its inevitable conflict with "abstractions" forced on human behavior remained basic to the methodological critique by symbolic interactionism of standard sociology.

3   WEBER: SOCIOLOGY IN THE LANGUAGE OF LIFE

Although Ellwood was the most translated American sociologist of the pre-1945 period, neither his writings on methodology nor those of "mainstream" quantitative American sociology were widely discussed in Europe. One result of the emigration of scholars from central Europe was that after 1945 issues in the philosophy of social science tended to be discussed in terms of German sources, notably Weber. Weber came to be discussed as a sociologist in the United States only after 1930. His methodological writings were translated only in 1949. Yet Weber had the most coherent and elaborate account of social science methodology, and Weber was both a target and a resource for subsequent writers in the philosophy of social science, notably Alfred Schutz, Peter Winch, Karl Popper, and Alasdair MacIntyre. The reception of Weber's thought produced confusion. The writer who most closely resembles Weber is the one who claimed to have the greatest disagreements with him, namely Popper, and the writer who claims to follow Weber, namely Schutz, has the least to do with Weber's actual methodological writing. Moreover, for much of the 20th century, Weber was conventionally believed to be a kind of social scientific successor to Wilhelm Dilthey whose central methodological idea was *Verstehen* and who was properly understood as a proto-phenomenologist of everyday life.[22] It is perhaps useful to begin with the sources of this peculiar misapprehension.

Weber's *Economy and Society* ([1968]1978) begins with a definition, which as Weber might have said was a matter of decision, of sociology as concerned with meaningful social (meaning relevant to others) action. In the passages that followed he argued that explanations must be both causally and interpretatively

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[21]Blumer claimed to be inspired by Mead's lectures, and Mead's Carus lectures, one of which was entitled, "The Present as the Locus of Reality" contains the same idea [1932].

[22]For a discussion of the reception of Weber's methodological writings that deals with the issue of this misperception, see [Eliaeson, 2002, esp. 41-46].

adequate. What was adequacy with respect to interpretation? Weber was trained as a lawyer, and as we will see the notion of adequate cause comes from a contemporary theory of legal causation which Weber elsewhere [1949, 167] characterizes as the appropriate model for historical causal explanation. In his discussions of interpretative adequacy, which are very brief, he uses a legal distinction as well. A direct observation or understanding is one in which the point or meaning of an act can be discerned by in effect the immediate application of typifications of the kind that are contained in ordinary daily language. The example he gives is of a man chopping wood. The interpretation with the most inherent plausibility, or as Weber puts it, *Evidenz*, is that the activities of the wood chopper are instrumental and have the purpose of chopping wood. Other actions must be interpreted in an indirect way which corresponds to a different category of legal evidence involving inferences. The wood chopper, for example, may have the further purpose of chopping wood to provide a wood supply for the winter for his family, or may be chopping wood to sell in a market. These purposes cannot be derived or justified by simple reference to the act of wood chopping that is accessible to direct observation. They must be made evident by connecting the purposes to other observed facts. In these other cases, however, recourse to standard typifications is essential and the *Evidenz*, and therefore the adequacy of the interpretation, is the result of the way in which the connected facts, such as the related actions of the wood chopper, are clarified through the typification.

Weber argued that the greatest degree of evidentness attached to *rational* explanations or typifications, so that, for example, an account of the act of wood chopping that connected it to evidence of a rational strategy for the achievement of some goal and also connected it to a variety of other aspects of the actions of the individual provided the strongest kind of understanding. But a similar degree of "evidentness" might well attach to an interpretation of the actions of an enraged husband shooting his wife's lover. These interpretations are of course always, as in the law, corrigible, so that an even better interpretation that, for example, shows that the husband was only faking rage to avoid punishment by representing a killing for purposes like a crime of passion might prove to be the explanation that made the most sense out of the facts. Moreover, multiple, conflicting interpretations were always possible.

The problem of typification and type concepts was a conventional if not a terribly deep problem in phenomenology when this aspect of the argument was reconstructed and extended in phenomenological terms by Alfred Schutz. Schutz's problem, unlike Weber's, was the problem of other minds, that is, the problem of how an individual could understand another individual. So his question is what are the conditions for the possibility of understanding, and such conditions are understood phenomenologically to be a question about the structures of consciousness that are the necessary preconditions for understanding another person as a person. But Schutz, as we will see, was adamant about rejecting the "Diltheyan" interpretation of Weber's arguments about this topic. Perhaps the crucial document in the reception of Weber on this topic was a famous essay by Theodore Abel,

entitled, "The Operation Called *Verstehen*" [1948], which, in the spirit of Bridg-
man's operationalism, Abel asked what sort of "operation" this was? In Weber,
of course, there is no claim that *Verstehen* is a "method" or operation. It figures
rather as criterion of explanatory adequacy relevant to the causal explanation of
meaningful action, and as a means of distinguishing kinds of disciplines, those
which necessarily use the "language of life" [Weber, 1988, 209] and those which
do not.

The term "meaning" is used by Weber for a specific reason, namely as a sub-
stitute for purposive or intentional action in order to, as part of a larger concern,
eliminate teleological descriptions from social science. This desire to avoid tele-
ological explanations was provoked by, among other things, a desire for a purely
causal social science. Such a social science would not require for its completion
a general account of human ends based on some sort of philosophical anthropol-
ogy or historical teleology, both of which Weber rejected as intrinsically valuative
rather than scientific. In practice, when Weber says "meaningful" he means "in-
tentional," except in such instances as the meaning of a mathematical proposition
and the like. Weber's account of understanding and interpretative action con-
sequently is designed as a superficial one of matching conduct to typifications of
intentional action that requires no significant exercise of phenomenological prowess
or a "method" of *Verstehen*, and no special account of access to "meanings."

Abel's particular concern in this article involved the puzzle about what sort
of basis one could have for the attribution of intention in the context of under-
standing a statistical correlation. The example Abel gives is the relation between
marriages and crop prices, one of the oldest statistical issues (cf. [Morgan, 1997]).
The problem of giving meaningful interpretations to statistical relations is in fact
a topic which Weber discusses in passing in the "Introduction" to *Economy and
Society* [1914] under the heading of meaningful and meaningless statistical rela-
tionships. Here it would have been resolved by identifying the correlation with
"typical" and already understood courses of action, such as the peasant (or his
intended bride) marrying after deciding that he had enough money to marry.

The ordinary form of the problem of relating causality to interpretive typifica-
tions, however, operated in the other direction, that is to say it began with the
typification, not the statistical relationship. As Weber understood the problem of
causal explanation in the social sciences, it was to indicate the probability of some
outcome given a particular typification, where the typifications were derived from
or part of the language of life. If the question is one of motivation to commit a
murder, for example, a description of the act which typified it as "the man went
to dinner with his wife and disagreed with her about the choice of desserts and
consequently went home and murdered her" would be perhaps meaningfully ade-
quate in the sense that one might be able to conceive of a person so engaged by a
dispute over dessert that he could actually commit murder as a result. Neverthe-
less, the sheer fact of the rarity of such murders suggests otherwise. A different
typification, for example, "a couple with severe marital difficulties is bickering,
and the man, who has a history of violence, becomes enraged over the woman's

selection of dessert and kills her," would have a higher probability and thus be more credible as an explanation. In the first typification, the standard of minimal probability of the outcome, even if it was set at a very low level, probably could not be met.[23]   The supposed cause was so trivial that it would not sufficiently increase the probability of the husband committing murder to be deemed a cause.

Although this perfectly legitimate account of causality strongly resembles the much later statistical relevance theory of Wesley Salmon [1971], it is a form of analysis with very little practical significance. So much depends on the correct assignment of a case to a reference class, which is to say so much depends on how the facts are described, that the added value of the probabilistic analysis of causes is negligible, and this means that in effect a plausible description will invariably, except in made-up cases, be causally adequate. In this sense the naive understanding of Weber as a *Verstehen* theorist is true, not because he believes in a method called *Verstehen* but because the description of an act in terms of an already understood ideal-type provides all or virtually all of the explanatory payoffs available to the social scientists with respect to meaningful action. This is an issue we will take up again shortly in relation to Alfred Schutz, who made this point forcefully.

Weber's focus on meaningful action is associated with a series of other theses that have an important subsequent history in the philosophy of social sciences, notably what comes to be called by Karl Popper "methodological individualism." Weber was a relentless opponent of holistic explanations and appeals to social holism and argued for a prudential, or as Popper would later put it, "methodological" individualism, on the grounds that the cognitive purposes of sociology, whose object is the "subjective meaning complex of action" [Weber 1978, 13], do not require them. Another of Popper's key ideas, presented in *The Poverty of Historicism* ([1957]1961), that action explanations should be made in terms of what he called "the logic of the situation" such that considerations of rationality provide a model of how the decision to act would be made rationally and deviations from rationality are explained, which Popper calls the Zeroth method, is found in Weber in the form of a discussion of rational action as an ideal-type. The conflict between their comprehension of these models will be discussed later in connection with Schutz and Ludwig von Mises. The conflict rests on differences in their understanding of the problem of the *a priori* in social science, of the status of frameworks, and of the role of theory, which can only be understood against the background of the larger problem of the *a priori* in neo-Kantianism during the early 20th century.

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[23]This is a point made by Schutz, who then proceeds to reinterpret Weber's notion of causality as a subordinate element of the adequacy of interpretation, under the mistaken impression that Weber's notion of causality can be subsumed under the notion of rational expectations for the causal outcomes of action [1967, 229-34]. As shown below, Weber is also concerned with the fact that there may be other causes (e.g of a biologically based kind) that co-operate in the production of interpretable phenomena (such as, to choose one of his examples, succumbing to charismatic appeals). These added causes might affect probabilities but not be part of the agent's expectations.

The key to Weber's understanding of the problem of the *a priori* involves the notion of values, which he formulated in a distinctive manner. Weber's basic ethical position was that a very wide variety of value choices was possible and that rationality could do little to decide between them. Nevertheless, for Weber, rationality could perform an important unmasking function by revealing the role of concealed and unconscious value choices and commitments. Likewise, rationality could show that certain value choices could not be construed realistically as "this-worldly" value choices but could be adhered to consistently only as "other-worldly" value choices, that is to say value choices based on the outcomes or considerations that arose from God or heaven. For Weber an Al Qaeda suicide bomber or, as he himself usually put it, an anarchist, could "rationally" or consistently sacrifice his life for the cause, but only if he acknowledged the unrealizability in this world of the supposed goals of his movement. These same considerations applied to pacifism and, from Weber's point of view, a great variety of the conventional moral doctrine of his time, including the kind of Christian social reformism that was prominent in Weber's own background and in the thought of many of his contemporaries.

Weber of course did not invent the fact/value distinction, but he formulated it in relation to social science with great clarity in the course of a controversy within the *Verein für Sozialpolitik* (Social Policy Association), a group of economic experts who generally favored bureaucratic solutions and state control as a response to the various issues raised by the so-called social questions of the 19th century, as well as in his methodological writings. The latter were a relentless critique of the implicit teleology, usually presented as part of the "scientific" work, of the historical school of economists from whom these "Socialists of the Chair" derived their authority. Weber's strategy was to force his opponents to acknowledge the valuative and thus arbitrary character of their commitment to the state governed economy and the contribution it would make to the ever-increasing bureaucratization of social and economic life. The term central to Weber's formulation of the problem of values was "decision." Selections between values which had been clarified by the process of identifying problems of consistency between various value choices, making explicit the value choices, and determining whether they were realizable in this world left the individual in a situation of decision for which no further rational guidance was possible. This doctrine had implications far beyond the idea of a positive policy science, however, it also implied that our judgments of such things as progress are essentially valuative, a point he made in relation to Simmel. Weber discusses the example of Simmel's substantive "sociological" claim that progress consists in "differentiation" and argues the following:

> . . . whether one designates progressive differentiation as "progress" is a matter of terminological convenience. But as to whether one should evaluate it as "progress" in the sense of an increase in "inner richness" cannot be decided by any empirical discipline. The empirical disciplines have nothing at all to say about whether the various possibilities in the sphere of feeling which have just emerged or which have recently been raised to the level of consciousness and the new "ten-

> sions" and "problems" which are often associated with them are to be
> *evaluated* in one way or another. [Weber 1949, 28]

The claim is that there is no fact of the matter about what progress is: it is a
valuative rather than a factual question and could be turned into a factual question
only on the basis of some sort of ungroundable claim such as having discovered
in history the final human value. The claim would be ungroundable because such
a discovery would necessarily be circular, for reasons that will become apparent
shortly.[24]

Weber, like the cultural relativists, tended to collapse cultural distinctions and
distinctions between historical ethics into cases of value choice or nonrational value
decisions.[25] The actual process of value change, however, was not understood as
the conscious making of value decisions, though this is of course possible, but
rather as the experience of a kind of disorientation where the diminished relevance
of particular values at the close of some historical period is experienced as a kind
of intellectual twilight. It is these passages that were appropriated particularly by
Karl Jaspers. But some of the most important implications of Weber's construc-
tion of these issues came from the claim that the language of life was intrinsically
valuative.

Weber stressed, in a way that neither Durkheim (for the reasons we have seen)
nor cultural relativism stressed, that the character of ordinary historical categories,
necessarily expressed in the language of life, made them unfit for use in eternal or
trans-historical "laws." Weber's argument here is strongly reminiscent of Donald
Davidson's notion of anomalous monism [1980b]. For Weber, the social sciences,
or, as he called them to emphasize the valuative character of the subject matter,
the historical sciences, constituted their objects and their explanatory interests in
a terminology that was already valuative. He argued, in a passage that appears to
refer to Mill, that an astronomy of social science, even if it were possible, would
fail to answer the questions that we posed in our own valuative constitutive termi-
nology, but as we have seen, he argued that this did not preclude causal analysis
[Weber, 1949, 73]. This Kantian picture points to a more fundamental problem,
the problem of the *a priori,* that is to say, the problem of the non-empirical or con-
ceptual pre-conditions for empirical knowledge. In the social sciences this problem
took some standard forms: what did one have to know or possess in advance to
recognize an intentional act, or a belief as "rational," a pronouncement as "legal,"
or some set of events as a "war."

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[24]These arguments reappear with Popper, who assumes them. But the reconstruction of
Popper's sources is an exceptionally difficult task. His familiarity with Weber is nevertheless
easy to establish. There are multiple references to Weber in *The Open Society and Its Enemies*
[1945]. The concept of decision used in essentially the same way, as a residual category of ethical
choice, plays a central role in *The Logic of Scientific Discovery* [1959] in connection with Popper's
demarcation criteria, which is defended as a convention, grounded in nothing but decision, but
which has the consequence of permitting and enabling scientific progress, which itself is a kind
of contingent value choice.

[25]Relativism and historicism, and especially cultural relativism, is discussed in the chapter by
Jarvie in this volume. Many related issues appear in the chapter by Crasnow as well.

Weber's solution to this problem was to divide the *a priori* conditions for social scientific knowledge into the rational and the valuative. The valuative was necessary for constituting the subject matter: the language of life was itself valuative, and part of a valuative *Weltanschauung*. The determination of causal relations, consistency of means to ends, and the like was a matter of "logic," which he understood narrowly. This approach left him open to the charge that his conception of the problem of knowledge was itself valuative or metaphysical.

But his formulation of the problem made a critical distinction: between the worldview of the subjects of research (which he took to be a construction for the purposes of the analyst and his audience) and the worldview of the analyst and the audience. Neither was "rational." The historical scientist has no privileged starting point, and cannot escape the implications of the following considerations: the facts of "history" are themselves constituted for us by our own values and whatever we discover about the world constituted in this way can be meaningful only to others who constitute the objects of history in the same language, and they would do so only because they shared our values. But it is entirely contingent that they do so. Also, there is no privilege that arises from being at one or another place in the historical process of the development of values, because, as seen earlier, even the notion of progress is valuative. This meant not only that any valuative claims that could be thought to arise from history are relative to this starting point, but also that the questions themselves are intelligible only with reference to an essentially valuative and thus relative starting point. His solution to the problem of the *a priori* was thus to make the basic framework of the analyst relative to the analyst's time and concerns, with the exception of those elements that were universal parts of thought, which he called to be "logic," and narrowly construed to mean deductive reasoning and perhaps the kinds of calculation necessary to assess probabilities, and perhaps decision theory, but did not include the kinds of philosophical reasoning characteristic of Kantian philosophy as practiced at the time.

4 THE PROBLEMS OF THE *A PRIORI*

4.1 *Sociological System Building*

The problem of the *a priori* appears in a variety of guises in early 20th century philosophy of science but appears in many more hidden ways in connection with social science. The now standard historical story about the origins of Logical Positivism, as told by Michael Friedman, sees the logical positivists as taking a step beyond neo-Kantianism, but also largely remaining within the framework of neo-Kantian problems. In the case of physics, these problems arose from the fact, which Einstein's theory of relativity had made evident, that physical truth was relative to the geometrical form in which it was expressed and that there were no definitive reasons in particular cases to prefer one geometrical form over another. As Don Howard puts it,

> In a series of essays and reviews in the early twenties, Einstein and
> Schlick agreed with the neo-Kantians that empirical evidence underde-
> termines theory choice, especially the choice of deep theoretical prin-
> ciples like the axioms of geometry; but whereas the neo-Kantians ex-
> ploited the fact of underdetermination to insulate cherished principles
> from empirical refutation, and insisted that our choice between alter-
> native theories equally compatible with experience is determined by *a
> priori* considerations, Einstein and Schlick argued that no principle is
> immune to rejection or revision in the light of experience, and insisted
> that the choice between alternative theories is a matter of convention,
> guided at most by considerations of simplicity. [1990, 369-70]

The neo-Kantians wanted to insist that there were coercive *a priori* grounds for
preferring one formulation and therefore one statement of fact to others.[26] Ein-
stein, who was aligned with the logical positivists on this crucial point, rejected
this and treated this particular kind of underdetermination as irreducible.

Apriorism in physics arose as a reflection on the conditions of the possibility
of already established physical theory, and it was characteristic of the aprioristic
projects of neo-Kantianism to begin with some already established domain, about
which one could then reason transcendentally. In the case of sociology, the prob-
lem was different, and the order in which the problems arose was also different.
Sociology was not an established discipline, but itself a possibility whose condi-
tions needed to be established. The idea that it was possible to establish them in
advance reflected a dominant German conception of the role of philosophy, which
was understood as follows: philosophy was a discipline which inquired into, clar-
ified, and established the rational credentials of the basic concepts in an area of
inquiry or domain of validity, such as the law. This was a Kantian task in the sense
that it sought the uniquely rational system of ideas or categories which disciplined
thought required. By demonstrating the unique validity of this system, this effort
established the conditions for objective knowledge, which was knowledge arrived
at in accordance with these concepts.

Central to this conception of the task of philosophy was a particular view of
logic as a discipline. Logic could be construed narrowly or more broadly, but if
construed narrowly (as for example Weber had construed it) it could not fulfill the
role of establishing a scheme of concepts, and if construed more broadly it could.
Discussions in this literature routinely denounce narrow conceptions of logic in the

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[26]This topic is discussed at length in the writings of Michael Friedman [1999; 2000] where the
technical details are given. His discussion of Carnap's *Aufbau* and its critique of neo-Kantianism
is particularly important. The direct relevance of this to the social sciences was limited, though
indirectly there were strong connections, in that the episode stressed by Friedman in this book,
and even more so in his discussion of Cassirer and Heideggger [2000], was part of a long-running
crisis in the notion of localized or historicized synthetic *a priori* truth that was central to neo-
Kantianism's revision of Kant. As will become clear, however, the problems of the *Geisteswis-
senschaften* were significantly different from those of *Naturwissenschaften*, and this was nowhere
more evident that in the discipline of sociology, which worked with historical materials but pur-
ported to be a generalizing science.

course of defending their own conception of their task. Consider a representative
example, from Freyer:

> By dissecting the structural elements of thought from an existing con-
> text of knowledge, the pure categories that make the experience of
> the human sciences possible in the first place will (that is the goal) be
> found. Especially if someone, through his own work, has achieved both
> the necessary reverence for a genuine particular science and the proper
> intuition of its activity, then this way to a local foundation of the hu-
> man sciences will appear to him as the only appropriate and promising
> way. He will scrupulously avoid a philosophy of knowledge based only
> on a theory of logic. Must it not appear absurd to him to want to
> know the logical structure of the human sciences other than by rais-
> ing the factual procedures of brilliant historians and philologists into
> conceptual consciousness, and by gathering the concealed presuppo-
> sitions, effective basic concepts, and unconsciously practiced methods
> from their works of art into understanding? . . . Dilthey proceeded in
> this way when he studied the formation of the historical world in the
> human sciences ([1923]1998, 5-6).

Of course, the situation in sociology was different from the situation of the estab-
lished disciplines of history and philology. In sociology the same task of producing
a "logical structure" had to proceed by analogy to the established human and
natural sciences, such as legal science and history.

The law was a favorite topic of neo-Kantian thought, and not surprisingly: the
law is "generalizing" and "human." It shared the subject matter of the other social
sciences, particularly sociology which took the law as part of its subject matter, and
thus law provided one of the most consistent contrasts to sociology. The analogies
can be seen clearly in a crucial text by Emil Lask, published in a *Festschrift* for
Kuno Fischer, one of the founders of neo-Kantianism. At the time this was written,
Lask was a close friend and intellectual interlocutor of Max Weber, who was then
publishing his key methodological essays (but not on "sociology," a discipline then
more closely identified with Simmel, who is discussed by Lask). Lask notes "the
parallelism of methodological and pure value problems," and says that "insight
into this parallelism may again save us from confounding the empirical cultural
concept [i.e. the concept of culture relevant to the cultural sciences] with that
concept of culture which represents absolute value and world outlook" [1950, 24-
5]. The law is a paradigm case of the "dualism" that promotes confounding, as
it is both "cultural meaning" and "cultural reality" [1950, 27]. "The law may
be either regarded as a real cultural factor, a vital social process, or examined
as a complex of meanings, more exactly of normative meanings, with regard to
its 'dogmatical contents"' [1950, 27]. Understood normatively, it makes sense to
resolve it into a "teleological science," perhaps to be understood as governed by
the abstract notion of *Recht*, an abstraction which allows for new possibilities of
conceptualization unanticipated in common sense or a naturalistic approach [1950,

31], possibilities that arise, for example, when novel legal fact situations have to be understood in relation to established but insufficient legal abstractions.

Each effort at intellectually organizing the material of the law "is a transformation — partly prescientific and partly scientific — of epistemological 'reality' into an abstract world related to particular cultural meanings" [1950, 27]. The "sociological" approach is parallel, governed not by the concept of *Recht*, but by the concept of the social:

> all cultural types may well involve the element of the social, which in its complete isolation and unadulterated purity could be grasped only by an ultimate, most abstract analysis. That analysis would then be the "sociology" postulated by Simmel, which would then start from the final results of other disciplines and constitute their "general part." [1950, 26]

In effect, then, sociology becomes an aprioristic inquiry into "the element of the social" which takes as its material the notions of the social found in the special cultural sciences.

In one sense this is a transcendental problem on the first order, which is to say regarding "the social" as an empirical phenomenon. Simmel's own "sociological" efforts focused on such questions as the conceptual properties of particular forms of social relation, such as the dyad, and proceeded by elaborating various implications about the nature of the social relations conducted under these forms, which he exemplified through historical and anecdotal examples. A simple example of this, which happens to compare directly to Weber, is Simmel's conceptualization of authority. Like Weber, Simmel's account begins by placing it under a more general heading, in Simmel's case "interaction," *Wechselwirkung* (more literally "reciprocal effect" [Wolff, 1950, lxiv]. Simmel distinguishes three kinds: subordination under an individual, a group, or an objective force (social or ideal) [Wolff, 1950, 190)] The categories are elaborated to cover many topics, such as the subordination or exclusion of minorities by groups through "out-voting" [Wolff, 1950, 239], the potential role of conscience [Wolff, 1950, 254-256], and considerations of objectivity with respect to appeals to principles [Wolff, 1950, 256-261] in the case of subordination to an objective ideal force, such as an ethical principle. The status of this classification is muddy, and it seems both abstract and ad hoc. Nevertheless, it was grounded, in the sense that the neo-Kantian strategy grounded its constructions, in the extant conceptualizations that the historians and interpreters in the special sciences placed on facts. So the results represented a kind of analysis of pre-conceptualized material rather than simply stipulative definition. Weber, unlike Simmel, denied that his more famous classification of types of legitimate authority into traditional, rational-legal, and charismatic, was derived in this manner, or had any claim other than utility, and was thus free to simply stipulate.[27]

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[27] Critical Theory is the concern of Bohman's chapter and part of Outhwaite's chapter in this volume.

The contrast between Weber and Simmel is in their attitudes toward the problem of grounding systems of concepts. In the framework of the neo-Kantian methodology with which Simmel and the other thinkers in this tradition operated, there was an ongoing ascent, comparable to the semantic ascent of later analytic philosophy, to the transcendental epistemic level, particularly to questions of "methodology," meaning questions about the conditions for the possibility of *knowledge of* "the social" or of this "element." This ascent, into what Simmel called philosophical sociology, had a different significance than semantic ascent. As Simmel puts it, philosophical sociology is "the level on which factual details are investigated concerning their significance for the totality of life, mind, and being in general, and concerning their justification in terms of such a totality" (cited in [Wolff, 1950, 23]), which is to say in terms of a closed system of concepts:

> evidently, this type of question cannot be answered by the ascertainment of facts. Rather, it must be answered by interpretations of ascertained facts and by efforts to bring the relative and problematical elements of social reality under an over-all view. Such a view does not compete with empirical claims because it serves needs which are quite different from those answered by empirical propositions. [Wolff, 1950, 24]

Thus Simmel's project reflected the characteristic neo-Kantian idea that objectivity comes from raising particular issues to a higher level of abstraction under the discipline of the construction of coherent systems of concepts, nevertheless, he also grasped that this strategy was only partly effective in sociology. Issues over important questions of philosophical sociology, such as "Do meaning and purpose inhere in social phenomena at all, or exclusively in individuals?" arise from conflicting world views or party positions. Abstraction and systematization alone could not resolve theses types of conflict [Wolff, 1950, 25]. And this kind of preconceptualization of the material, on which both sociological and at the next level philosophical analysis worked, left open the possibility of the construction of a variety of different systematizations, taking different starting points and reflecting different worldviews and values. For Simmel himself, such considerations damped any drive to systematization, and commentators saw him as "unsystematic entirely by intention" Bohner quoted in [Steiner, 1999, 215]), indeed as arguing that every system falsifies our thinking, because it freezes our thought. One can hear in this language of the rigidity of concepts and the non-rigidity of life echoes of *Lebensphilosophie*, in which Simmel, by the time he wrote this in 1917, was already engaged, and prefigurations of Heidegger.

One might wonder why the strategy of system-building had so many adherents, given its unpromising character as a solution to its supposed aim: providing a univocal grounding and conceptual structure for the science of sociology. There were perhaps two reasons. One was that these inquiries, which in Simmel's case, for example, covered such topics as fashion, often produced gems of insight which did represent in some sense the fulfillment of the promise of the strategy. This was

particularly true for Simmel himself, whose essays "The Stranger" [Wolff, 1950, 402-8] and "The Metropolis and Mental Life" [Wolff, 1950, 409-23] and his long study of money ([1896]1990) became classics. Moreover, these now forgotten systems of categories did not on the surface appear superior to the stipulative system of categories provided by Weber in *Economy and Society* ([1968]1978), which introduced such enduring notions as charismatic authority, and which remains the most significant achievement of sociology and perhaps social science of the 20th century. But, as good neo-Kantians, these thinkers, with the exception of Weber, who differentiated himself decisively from them, could not conceive of a science that was not constituted by a closed framework of categories. So for them, there was no alternative to the *a priori* project, regardless of its propensity to merely produce sociological systems on analogy to (and for the most part not very different from) the philosophical systems still being produced in the departments of philosophy from which many of these sociologists had themselves decamped, or from philosophical circles with which they were associated.

   If one accepts the methodological notion that to be a science one needs a coherent set of concepts and that these should be produced by the kind of ascent I have described here, the problem of multiple possible schemes — the problem of underdetermination — appears to be soluble and perhaps can only be solved by the method of ascent itself. "Higher" epistemological or metaphysical considerations would decisively establish one scheme as genuinely basic. In this respect, these thinkers were like Einstein, accepting of underdetermination on the level of substantive sociology. Differences in worldviews were a fact. They nevertheless sought a perspective on the methodological or epistemic level that accommodated this fact while acknowledging the conceptual dependence of sociology on the pre-conceptualizations contained in worldviews. Weber's approach to this problem, which became the focus of a large amount of commentary and critical reflection, was to sharply distinguish that which could be said to be general, which for him consisted of logic in the narrow sense and the kind of calculation necessary for making probabilistic judgments, and that which was not general, as a result of historically specific conceptual pre-constitution of the subject matter. He stressed the error of treating ideal-types, which for Weber were value-related constructions rooted in worldviews or the language of life, as though they were uniquely valid descriptions of substantive reality. He also argued that the constructions which his predecessors in the German historical school (or for that matter Marxists) claimed to have "deduced from reality" would not be any more than value-related constructions rooted in worldviews or the language of life and subject to the consideration that worldviews (and ordinary language, which for him represented a worldview) were valuative and thus arbitrary in origin. It was precisely his narrow view of "logic" that entailed that the Simmelian project and its variants were misguided: for him there simply was no "logic" in the extended sense that such projects required. Thus in effect Weber, like Einstein, embraced underdetermination as the last word.

This was an unusual and controversial conclusion, for reasons that are important for understanding the subsequent literature not only of German sociology but of the philosophy of social science generally. As I have suggested, the more typical response was to seek a second or third order resolution to the problem of alternative conceptual systems. The examples are legion, but a few of the more consequential ones are these: phenomenology, which aspired to some sort of grounding of social knowledge in fundamental considerations, but which settled for grounding in a phenomenological account of the conditions for the possibility of *Verstehen* (exemplified by Alfred Schutz, to be discussed shortly); the idea of objective culture, grounded in the "fundamental" considerations about the conditions for the possibility of "meaning" [Freyer, 1998; Cassirer, [1923]1955]; solutions to the problems of the historical relativity of values or worldviews based on higher level theories [Scheler, 1963; Horkheimer, 1972; Mannheim, 1936, 78-83]; *a priori* theories of the ontologically superior status of society over the individual understood through liberalism; *a priori* accounts of action which supported the claim that laws of economics understood as a branch of sociology could be derived from the consideration that human action was "rational" by definition Mises ([1949]1963); and accounts based on philosophical anthropologies [Gehlen, 1940].

The rapid development of a debate over social science in the Weimar era was a result of the confrontation, in the form of critique, of these different strategies with one another, which produced a vast literature. Weber was the focus of much of this critical discussion, and the literature on Weber during this period (and long after) was concerned with such issues as these: Weber's implicit fundamental ontology Löwith ([1960]1982); his implicit philosophical anthropology and concept of freedom [Landshut, 1929]; his assumptions about rationality [Grab, 1927], the class and historical *Weltanschauliche* character of his fundamental concepts, including his rejection of dialectical "logic" [Lukács [1962]1980; Horkheimer, 1972; Neurath, 1959]; the character of his underlying theory of values and the fact value distinction; the sufficiency of his (or any) sociological approach to the concept of law and legal validity [Kelsen, 1945]; his assumptions about the nature of human action [Mises, 1960]; and his claims about the nature and limitations of *Wissenschaft*, which produced a large controversy [Curtius, 1919; Lassman and Velody, 1989; Salz, 1921]. Weber's defenders, notably Karl Jaspers, replied in kind, by elucidating (or inventing) and endorsing the underlying ontology, value theory, and so on which were claimed to be implicit in Weber's thought [Jaspers, 1989; Henrich, 1987].[28]

Phenomenology was the vehicle for many of the attempts to secure some sort of third-order grounding for particular systems.[29] Alfred Vierkandt, who attempted to ground his account of society in a phenomenological account of instincts, is perhaps the paradigmatic example of this strategy. Mises' brutal critique of his efforts is revealing with respect to the difficulties which the strategy faced. Vierkandt

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[28]This debate was recapitulated in only slightly changed terms after World War II by Henrich [1952], Strauss [1953], and Habermas [1971], and is dissected in a classic text by Bruun [1971].

[29]For further discussion of phenomenology, see Outhwaite, this volume.

rejected the individualist theory of action, and was in this respect a typically German anti-liberal. As Mises comments, "he is unable to support his rejection of the latter [i.e., the individualist theory of action] except by repeatedly referring to the rationalist, individualist, and atomistic character of everything that does not meet with his approval" [1960, 56]. Of course, from Vierkandt's point of view, this derogation of assumptions he rejected was an exercise in showing liberalism to be ideological, and leveling the playing field. As Mises characterizes it, Vierkandt's position is that

> human society is, so to speak, already foreshadowed in the relationship of the master to the dog he trains. The relationship of leader and led corresponds to the relationship of master and dog: it is healthy and normal, and it is conducive to the happiness of both, the master and the dog. [1960, 56]

Mises notes that "one cannot argue this point further with Vierkandt because, in his view, the ultimate source of cognition is

> phenomenological insight, i.e. what we directly experience personally in ourselves and can convey to consciousness with apodictic [i.e. incorrigible] evidence. (Vierkandt quoted in [Mises, 1960, 56])

Thus did the project of second and third-order grounding reproduce the conflict between systems, and impel much of German sociological thinking into a project of the analysis of presuppositions.[30]

4.2   Action and Normativity

The most consequential phenomenonological study from this era was Alfred Schutz's *The Phenomenology of the Social World* ([1932]1967), a classic work in the philosophy of social science in its own right. Schutz was a member of the Mises circle and a childhood friend of Hans Kelsen, rather than a member of the Weber circle, and his approach reflects this. He tells us that he became convinced of the correctness of Weber's approach ([1932]1967, xxxi) but believed that "his analyses did not go deeply enough to lay the foundations on which alone many of the important problems of the social sciences could be solved." He went on to add that only in Bergson and Husserl, "and especially in Husserl's transcendental phenomenology, has a sufficiently deep foundation been laid on the basis of which one could aspire to solve the problem of meaning," which he took to be the main task left unfinished by Weber. ([1932]1967, xxxi-xxxii). Yet even Husserl, he thought, had not yet solved the problem of the "Thou," of genuine interactional knowledge of other minds, so he proposed to go forward, in the absence of secure foundations, by way of a critique of Weber ([1932]1967, 97-8). Weber's account did not require a "Thou" understanding, but a "They" or third person application

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[30]This was also the strategy of Critical Theory, most fully developed in the thought of Habermas.

of categories of intentional action descriptions, in the form of ideal-types. Ideal-types are anonymized logical constructs ([1932]1967, 183, 189) that operate from the point of view of the interpreter and have nothing directly to do with the actual subjective states of the interpreted agent ([1932]1967, 188). They supply meaning rather than discover it ([1932]1967, 190). Schutz notes Weber's preference for rational action ideal-types, a preference which Weber bases on the consideration that interpretation seeks *Evidenz*, and that the ideal type of rational action is the type that is most clear and distinct. According to Schutz, this, together with the "they" character of his interest in action, decisively separates Weber from Dilthey:

> We must never cease reiterating that the method of Weber's sociology is a rational one and that the position of interpretive sociology should in no way be confused with that of Dilthey, who opposes to rational science another, so-called "interpretive" science based on metaphysical presuppositions and incorrigible "intuition." (Schutz [1932]1967, 240)

The issue that concerns Schutz, however, is one that arises between Weber and other practitioners of rational science, particularly Mises, and through him, as we shall see, Popper and, the rational choice theorists, and ultimately Davidson.

Schutz revised Weber in various small ways, one of which is consequential for what follows. He objected to Weber's way of formulating the distinction between adequacy at the level of meaning and adequacy at the level of cause. He notes the perplexing apparent pointlessness of the notion of causal adequacy: "if I start out from a real action as my datum, then every ideal-type construct that I base on it will already be in itself causally adequate" ([1932]1967, 232). Moreover, because having happened, it had to have happened causally. Rational action is a matter of choosing means that are appropriate to ends. To choose rationally would be to do so in accordance with past experience of the relevant causal relations. To do this requires the agent to conceptualize past experience into, as Schutz said, "typically comprehended meaning-adequate relations" ([1932]1967, 233), thus past causal experience enters into the conceptualizations of these typifications. Thus casual adequacy, rather than being an independent criterion, is "a special case of meaning adequacy" ([1932]1967, 234). This argument removes an important obstacle to collapsing the whole problem of explanation into considerations of interpretation.

A new issue then becomes apparent. What if there were, so to speak, a universal solvent to the problem of interpretation, an interpretive scheme which assured that actions could be understood at the highest degree of rationality? Weber assumed that there was and could be no such thing: for him, ideal-types of action were retail affairs, close to the language of life of the historically specific audience for the analyses, and useful for the limited purpose of making sense to this audience. They worked by abstracting from the details of individual action in a specific way: by selecting out and emphasizing certain features of the action shared with other actions. They often did not fit actual cases perfectly. Mises claimed to have something better with marginal utility theory, which he characterized as follows:

> For a long time men failed to realize that the transition from the clas-
> sical theory of value to the subjective theory of value [i.e. the principle
> of marginal utility] was much more than the substitution of a more
> satisfactory theory for a less satisfactory one . . . It is much more
> than merely a theory of the "economic side" of human endeavors and
> man's striving for commodities. It is the science of every kind of human
> action. Choosing determines all human decisions. In making his choice
> man chooses not only between various material things and services. All
> human values are offered for option. All ends and all means, the noble
> and ignoble, are ranged in a single row and subjected to a decision
> which picks out one thing and sets aside another. Nothing that men
> aim at or want to aim at remains outside of this arrangement into a
> unique scale of gradation and preference. (Mises [1949]1963, 3)

For Mises, the rationality of human action, understood as preference-fulfilling
choice, is an *a priori* truth. Weber, in contrast, had treated marginal utility and
self-conscious rational choice of means as ideal-types (even as distinct ideal-types)
among an array of other ideal-typifications that allow us to understand action,
that is, to describe action in terms of its subjective meaning in the language of
life. One can ask whether, from Weber's point of view, marginal utility necessarily
played a secondary role because an account in terms of marginal utility, which
relies on hypothetical estimates of subjective values and an assumed machinery of
choice, is significantly less transparent and intelligible than an account in terms of
the conscious rational selection of ends. Indeed, the marginal utility account gets
its intelligibility by analogy to the case of conscious decision, not the other way
around.

Weber makes an additional distinction between these cases of instrumental (or
Zweckrational) rational action and other kinds of action, including a) traditional
actions, which are on the borderline of meaningful action, shading off into pure
habit; b) purely affectual behavior, also on the borderline, in this case to mindless
reaction, and often a case of semi-rationalized sublimation; and c) actions guided
by the "deliberate formulation of ultimate values" using a systematic philosophical
technique, which is a specific type of "rationalization" ([1968]1978, 30). His point
about the boundaries between action and non-action is crucial: because intelligi-
ble action shades off into mindlessness, it may be intentional in a limited sense,
and is therefore only "action" in a limited sense. Indeed, he gives a number of
examples of action rooted in biology, in which the strength of the reaction can be
accounted for by the co-operation of biological causes, such as a biologically rooted
reaction to deviance (cf. [Turner and Factor, 1994, 88]). Here the category of "ac-
tion" shades off into the category of biological causation. From Weber's point of
view, then, rational action in the full sense of self-conscious articulate decision is
rare. Most "action" is often, perhaps typically, causally influenced by biological
causes, habituation, and so forth, and thus can only approximate the interpretive
ideal-types placed on it. The focus of his definition of action is thus subjective
meaning, but meaning is not everything. Other causal elements play a role, and

thus there is a difference of degree rather than kind between fully intentional and non-intentional. For Mises the focus is exclusively on the fact of decision.

Schutz deals with this wide-ranging conflict in terms of the conflict over the principle of marginal utility, which Weber treats as an ideal-type and Mises as an objective truth. The difference is significant, and Mises stresses a particular aspect of it. For Weber, all ideal-types are "historical" or at least potentially transitory in their utility — they are ideal-types "for us," that is to say, for our particular historical interpretive needs. "Rationality" is an interpretive framework that is especially important to us, as moderns, and making sense of the actions of past figures in these terms is useful and necessary for us, since this is our language of life. The idea that rationality is a potentially transitory framework is not odd for Weber, since the techniques of rationalization, such as the rationalization of accounting through double-entry bookkeeping or the rationalization of Continental law through techniques of law-finding which allow for the extension of legal principles consistently to new circumstances, are, quite clearly, historical products which may not have achieved their final form and in many cases vary from one place to another even in the present. The relevance of marginal utility to these cases is questionable at best. What Mises objects to, however, is the implication that the laws of economics, which he takes to be a logical consequence of the principle of marginal utility, are not universal. The principles of rational action, Mises says, though "they are acquired by means of abstraction, which aims at selecting for conceptualization certain aspects of each of the individual phenomenon under consideration" (quoted in Schutz, [1932]1967, 243), result "not in a statement of what usually happens, but of what necessarily must happen" ([1932]1967, 245). For Mises, this was an implication of the principle of marginal utility itself, which Weber endorsed, but seemed not to consider employing except in specifically economic contexts. Thus, for Mises, Weber is inconsistent or unaware of the real significance of the principle.

This is the conflict Schutz addresses. He accepts that, from an epistemic point of view, Weber is correct to see rationality as an ideal-type, in the sense that it plays a role in constituting its object of interpretation as an intelligible action. But he agrees with Mises that the principle of marginal utility is not the sort of thing that it makes sense to think of as non-universal or historically transitory, and thus nonobjective. He solves the conflict in a manner that the Vienna Circle itself might have suggested: by reinterpreting Mises as making the principle of marginal utility into a stipulative definition of economic action ([1932]1967, 245). Its objectivity is thus the *a priori* "objectivity" of the definitional relation between the principle and the term "economic action."[31] Mises went even farther than "eco-

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[31] Weber would have made a different point. Consider his remark that "many aspects of charisma . . . contain the seeds of . . . psychic contagion . . .These types of action are closely related to phenomena which are understandable either only on biological terms or can be interpreted in terms of subjective motives only in fragments" ([1968]1978, 17). This suggests that such "actions" are not fully explained by construing them in terms of subjective meanings, that to the extent they are the products of biological causes are only approximations to the ideal-type of rational action, and that action itself is an ideal-typical category which the actual

nomic action," however, noting that the principle had the effect of obliterating the distinction between economic and non-economic action because all choice involves considerations of the scarcity of means [1960, 61]. Thus, for Mises, all action is rational by definition.

The kinship between these claims and later discussions of reasons and causes and rationality are extensive, but I will limit the discussion to a brief overview. One can broadly distinguish between wholesale and retail versions of the problem of the explanation of action. Wholesale versions, like that of Mises, associate the whole of human action with the model of rationality, and replace the problem of explaining action with the problem of assimilating action to the model of human rationality. The justification for this is as follows: identifying a piece of behavior as action already amounts to identifying it as rational and teleological. The only appropriate next step in accounting for the act as an act is to subsume it more fully into the model of rational action itself, for example by more fully specifying the matrix of decision which makes the act "rational." This "method" closely resembles Weber's discussion of the problem of constructing and explaining the errors of generals in battle:

> The more sharply and precisely the ideal type has been constructed, thus the more abstract and unrealistic in this sense it is, the better it is able to perform its functions in formulating terminology, classifications, and hypotheses. In working out a concrete causal explanation of individual events, the procedure of the historian is essentially the same. Thus in attempting to explain the campaign of 1866, it is indispensable both in the case of Moltke and of Benedek to attempt to construct imaginatively how each, given fully adequate knowledge both of his own situation and of that of his opponent, would have acted. Then it is possible to compare with this the actual course of action and to arrive at a causal explanation of the observed deviations, which will be attributed to such factors as misinformation, strategical errors, logical fallacies, personal temperament, or considerations outside the realm of strategy. Here, too, an ideal-typical construction of rational action is actually employed even though it is not made explicit. [Weber [1968]1978), 21]

What distinguishes Weber from Popper is this: Weber rejects any claim that the model of rational action is necessarily universally relevant. It is merely a contingent fact for him that rational explanations provide the greatest clarity of understanding and it may be that in many cases, for example in actions done from passion, they provide little understanding. Understanding is a retail affair: there is no universal device for understanding, but only typifications useful in particular situations and for particular audiences. Where does Popper's account fit? Popper does nothing to ground the model of rationality that defines the logic of the situation in a more general *a priori* claim about human action, as Mises does. But at the same

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course of events often only approximates. For more examples see [Turner and Factor, 1994].

time he does not envisage an alternative to "logic of the situation" analyses of human action. Also implicit in Popper and Mises is the idea that an adequate rationalization excludes "causal" considerations of the usual sort, which is to say the kind that need to be established empirically. As Mises puts it, "The causal propositions of sociology are not expressions of what happens as a rule, but by no means must always happen. They express that which necessarily must always happen as far as the conditions they assume are given" [1960, 91]. The "necessity" in question here is, presumably, logical, and the validity of the claim to necessity rests on "the cognition of what is essential and necessary in every instance of human action" [1960, 90-1]. This same kind of reasoning, it may be observed, reappears in Davidson under different auspices, notably the indispensability of the axioms of decision theory for any description of action.[32]

"Retail" versions of this idea that adequate rationalizations preclude causal explanation or render it gratuitous appear in Schutz himself. As noted, he argues that causal adequacy is a special form of meaning adequacy, rather than an additional consideration. Thus a fully adequate "meaning" characterization of an individual action would include and subsume all relevant causal considerations. Parallel "retail" versions of this account appear throughout the reasons and causes literature of the fifties and sixties. A description of an action in terms of its intentions, in this literature, cannot be causal because the relation between intentions or reasons and the action that is intended is an internal or logical "in order to" relation rather than a causal one. But at the same time an intentional action can be intended only if the intender meets the following test: the act must be intended under a description that comes from the stock of descriptions available to the agent. This stock of action descriptions is, so to speak, a collection of retail items. But once one has described the act intentionally, there is no place for causal explanation.

The reasoning in this literature is slightly different from that of Schutz and Mises, but the results are similar. Identifying an action as an action is already a matter of ascribing not only intentionality, but a specific intention, as there is no test of intentionality apart from the identification of a specific intention. A full description of an intentional act qua action thus contains or refers essentially to the outcome to be explained. The sentence A) "John drove to the store in order to get

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[32] "It may seem that I want to insist that decision theory, like the simple postulate that people tend to do what they believe will promote their ends, is necessarily true, or perhaps analytic, or that it states part of what we mean by saying someone prefers one alternative to another. But in fact I want to say none of these things, if only because I understand none of them. My point is sceptical, and relative. I am sceptical that we have a clear idea of what would, or should, show that decision theory is false; and I think that compared to attribution of desires, preferences, or beliefs, the axioms of decision theory lend little empirical force to explanations of action. In this respect, decision theory is like the theory of measurement for length or mass, or Tarski's theory of truth. The theory in each case is so powerful and simple, and so constitutive of concepts assumed by further satisfactory theory (physical or linguistic) that we must strain to fit our findings, or our interpretations to preserve the theory. If length is not transitive, what does it mean to have a number to measure length at all? We could find or invent an answer, but unless or until we do we strive to interpret 'longer than' so that it comes out transitive. Similarly for 'preferred to'." [Davidson 1980a, 272-3]

a bottle of milk" contains a description of the act to be explained. The sentence B) "John drove to the store and John got a bottle of milk," in contrast, merely describes a pair of events, not an intentional act, and is not explanatory. But "explain" is a problematic and gratuitous notion here. Explanation requires the independence of the explainer and the outcome. Here there is no independence. A description in which the two events are independent, such as B, is not explanatory, since going to the store is not a cause of getting the bottle of milk.

The relationship between the intention and the outcome, however, is more than this. A sentence correctly ascribing an intention to ordinarily implies that the action will take place, or to put it differently, the evidence for the intention ordinarily, if the intention is correctly carried out, includes the thing to be "explained," namely the act that is the outcome. The relation between intention and act is a relation we recognize as correct, as valid or intelligible, and is thus in present parlance "normative" rather than causal. When we recognize an act to be the fulfillment of a given intention we are, so to speak, recognizing the validity of the fulfillment. Ascribing intentions is a "retail" process, in this sense: attributing intentions is not backed by a more general account of rationality, but by the normative considerations particular to the application of specific intentional attributions, the considerations that make an outcome a correct fulfillment of an intention. As with Weber's meaning-adequate ideal-types, this provides intelligibility to action by fitting facts to a typification drawn from a large tool kit of intelligible singular action descriptions: what came to be called the stock of descriptions available to an agent (cf. [Anscombe, 1958; Winch, 1958; Turner, 1980; 2003b; MacIntyre, 1962]).

4.3   Hidden Apriorism

The problem of the *a priori* is the ur-problem of 20th century philosophy, yet it is also, in the context of the philosophy of social science, one of the most confusing. Winch was explicit in endorsing the thought that social science was an *a priori* inquiry, or more precisely an inquiry of an *a priori* kind, namely the elucidation of concepts, into an *a priori* subject, namely the concepts of a particular group. But this seems odd in many ways. Is there no empirical casual knowledge in addition to knowledge about concepts? If not, then what is it that social researchers produce? The rationality of action is difficult to conceive in other than an *a priori* way, so if rationality enters into explanation, it seems, the explanation is itself *a priori*. Or should we say that none of this is, properly speaking, explanation at all, but only understanding, and that there is no explanation in social science? If the backing that we can supply to our singular explanations, such as our explanations of actions by reference to intentions consists of claims about rationality, it seems that we should say that there is no explanation in social science. Attempts to reconcile the two have not been satisfactory. Hempel, when he turned to the problem of rational action, converted rationality into a "broadly dispositional state," that is to say into something causal, to avoid a conflict [1965, 472]; a solution that

persuaded almost no one (cf. [Davidson 1980a, 272-75]).

The idea that social science has its own special "presuppositions" in the neo-Kantian sense, that is to say as distinct from stipulations or definitions, is another source of puzzlement, as we have seen. Is there a correct set? And if so, how does one determine what it is? Phenomenology, as we have seen, leads to results that are both diverse and in some cases strange, as with Vierkandt. And the fact that these presuppositions may also be shared with ideological movements raises the question of whether acknowledging the role of such presuppositions in social science amounts to the admission that social science is fundamentally ideological and that all claims are true relative to ideological presuppositions.

Popper, in a section on Mannheim in *The Open Society and its Enemies*, dismissed the search for presuppositions as irrationalist, and suggested that Einstein's success had shown that the neo-Kantian problem of frameworks of thought was trivial, because frameworks were shed and replaced every time a theory was replaced by a better one [1962, 220]. In the context of physics, this makes some sense, though it is not clear that the process of shedding the old framework is quite so trivial. But in the context of social science, shedding frameworks is not even the same problem. As we have seen, Weber made the point that the conceptualizations we employ are dictated by our interests, meaning the interests of the audiences to whom we direct our explanations as well as the interests we define when we specify the cognitive purposes of a discipline. He also made the point that we are faced with a generic problem of changing the subject that prevents us from creating an "astronomy," a social science in a language other than the language of life, that would answer questions posed in the language of life. Changing this framework, in the context of historical questions, *is* changing the subject.

The problem of changing the subject is so deeply bound up with the project of sociology that it requires its own discussion. When Comte invented the term sociology, he formulated his main "sociological" law of three stages, in which thought in a given domain predictably passed through three stages, the theological, the metaphysical, and the positive, in which it ended. His thinking reflected the fact that the subject domain of sociology was, so to speak, already occupied. What he called theological and metaphysical concepts already formed part of ordinary moral and political discourse characterizing the relations of dependence between individuals. To take this domain and subject it to scientific understanding required that the theological and metaphysical content of these concepts be drained from them, leaving classifications which enabled the making of nomic predictions. This idea was transformed by Pearson and Giddings into a model for sociology as a statistical discipline, something that Comte did not envision, and indeed was hostile to. Durkheim too was a careful reader of Comte. There is more than an echo of Comte in his idea that social facts in the real causal sense are concealed and obscured by our ideas taken from the marketplace, as he quotes Bacon, and in his insistence that studying society using these ideas results not in an investigation of society but in an investigation of the implications of ideas in an aprioristic project. Yet each of these thinkers was compelled to deal with the fact that their

starting point was in some sense dictated by the language of life, as Weber put it.

Durkheim, despite his overt hostility to apriorism in sociology, took for granted that the primary thing which sociology was concerned to explain was the fact of obligation. Ellwood, as we have seen, recognized that this choice was itself a piece of apriorism. Yet from a Comtean point of view this is a legitimate project. If we understand obligation to be a pre-positive concept that will ultimately be replaced with a scientific one, and if we are aware of the snares of thinking of obligation in an *a priori* way — as is done in Kantian ethics, for example — we can loosely say that sociology seeks to explain obligation. By "explanation," we mean that sociology seeks to replace our deluded, superstitious concept of obligation with a scientific one. But one may ask whether Durkheim's acceptance of "obligation" as a topic amounted to acceptance of a problematic *a priori* starting point. Durkheim's contemporary, the Swedish philosopher Axel Hägerström, who was himself emancipating his thought from neo-Kantianism, dealt with legal obligation as a fiction to be explained by other means, by identifying the magical sources of Roman legal thought and the magical notion of obligation on which it rested. Hägerström then criticized the philosophical reconstruction of these magical ideas in terms of the concept of the will of the sovereign or the will of the people as the basis of legal obligation, and replaced them with a notion of law as fact in which only the predictive aspect, the element of expectation, remains once the metaphysical elements had been drained away.

Durkheim did not go in this direction, or at least as far. And indeed there is a generic problem here to which the contrast between Durkheim and Hägerström points. One response to Hägerström, which became standard in the philosophy of law, is that he had failed to explain law because he failed to account for the essential feature of bindingness. Thus he had in effect explained nothing, but merely changed the subject. Hägerström's point, of course, was that the binding element of the law is not constitutive, except in a revisable sense, but rather entirely mythical and therefore not something which need be "accounted for" as anything other than the error and illusion that it was. The fact that 20th century legal philosophy chose instead to stick with the notion of obligation without ever getting a particularly satisfactory account of it indicates how sturdy the metaphysical notion of obligation and the law has been. Durkheim was more chary of the problem of changing the subject. He was thus more respectful of obligation and took it as one of the givens to be explained. But here we see the delicacy of the problem of the *a priori* definition of subject matter. It is unclear what general grounds we might have for accepting a topic as in some fundamental sense genuine and part of the factual world to be accounted for and when we are entitled to ignore it or treat it as delusion and error. Slight differences matter. Spencer, for example, took "feelings" of obligation as the "data" of ethics, without taking obligations themselves as data. But it is questionable whether these substitutions work and some substitution seems unavoidable. Even Durkheim did not pretend to explain obligations in their own terms, but rather the hidden social fact of obligation which produced the feelings.

There was a body of criticism that directly addressed the problem of substituting "sociological" concepts for normative concepts. This line of argument appears in Schutz's 1932 work, where he repeats the claims of his childhood friend Kelsen. Kelsen's argument against Weber with respect to the law foreshadows the "normativity" issue of the present.[33] Weber distinguished between "dogmatic" and "historical" questions about the kinds of events that figure in the history of law. The question of whether, say, the donation of Constantine, was a legal "fact" or "valid" was for Weber a dogmatic question appropriate to legal scholarship, but a quite different question when taken as an explanation of the actions of historical agents who believed, however wrongly, in its validity. But what about the facticity of the law itself? The dilemma here is this: either it is a "legal fact" or not; if it is not, however, we are no longer talking about law, but about something redescribed in such a way that it is no longer law, or no longer identical with law. Kelsen put this directly, and in terms that might be taken from the writings of Joseph Rouse or John McDowell today: the sociological conception of the law depends on the normative conception. Thus there is not and cannot be a non-normative sociological study of the law.

Kelsen's reasoning was this. Weber defined law in terms of certain beliefs in legality together with the probability that the law will be followed. This is to say that, in the end, there is nothing "sociological" to the law but effective acceptance, or legitimacy. For Weber additional claims about legality are "dogmatic," that is to say part of the legal discipline of the law, but are gratuitous for the explanatory purposes of sociology. To say that a particular pattern of enforcement and command that is believed to be legitimate or legal and that is actually effective in the sense that it probabilistically predicts the behavior of the participants is, in addition, "really" law adds nothing to either prediction or to understanding. The subjective meaning of the acts is contained in the agents' beliefs about the pattern; the causal part is established by the patterns that allow probabilistic prediction of their behavior. One of Kelsen's replies to this is specious: he comments that this cannot be a definition of the law because the criminal does not need to have the law in mind for the criminal's act to be a crime. Weber is not committed to this either, but is only committed to some agents holding these beliefs and some probability of the beliefs being acted on. Moreover, he is not concerned with whether the criminal's act is a crime: this is self-evidently a "dogmatic" question. But whether it is a crime, and the difference between "some" people and "everyone," is important for theories which aspire, as Durkheim, Bouglé, and Wilfrid Sellars did, to derive the "fact" of obligation from "collective" commitment.

Kelsen makes another claim that is more interesting. He argues that to explain the law in terms of the beliefs of the subjects of the law is not to explain the law, because the question of what is law is not a matter of public opinion. Rather, it is a legal question, which can only be settled by legal considerations. Indeed, beliefs about what is law can be false. So to explain the law, that is to say what is genuinely law as distinct from the various things that people believe, however

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[33]Discussed, in conflicting ways, by Henderson and Rouse in their chapters in this volume.

erroneously, about the law, requires attention to what it is that makes the law genuine. The thing that makes the law genuine, the specific legal considerations mentioned above, are themselves statements of law determining legality. As Kelsen asks,

> Is a constitution republican, for instance, merely because it announces itself as such? Is a state federal merely because its constitution calls it such? Since legal acts usually have a verbal form, they can say something about their own meaning. This fact alone betrays an important difference between the subject matter of jurisprudence, indeed of the *social sciences* as such, and the subject matter of the natural sciences. We need not fear, for instance, that a stone will ever announce itself as an animal. On the other hand, one cannot take the declared legal meaning of certain human acts at their face value; to do so is simply to beg the question of whether such declared meaning is really the objective legal meaning. For whether these acts are really legal acts at all, if they are, what their place is in the legal system, what significance they have for other legal acts — all these considerations will depend on the *basic norm* by means of which the scheme that interprets them is produced. (Kelsen quoted in [Schutz [1932]1967, 246], emphasis added in Schutz)

This notion of the basic norm, in the modified form of the concept of rules of recognition that determine what is law, was taken over by H. L. A. Hart. This idea fits jurisprudence: there is indeed a judicial procedure of determining what is law and what is not.

One may question, as Weber would, whether introducing this concept changes the situation in the intended way. The *Grundnorm*, from the sociological point of view, consists of nothing but belief in the validity of the judicial procedure itself, and some probability that decisions, in this case by judges with respect to legality, are made in accordance with it. Hart recognized that the idea that legal norms could be created by utterance in this way was strange, and seized on J. L. Austin's notion of performative utterance to replace it. But one may question whether the notion of performative utterance makes any sense without the presence of beliefs in the legitimate powers of the performers — in the case of the law, Kings or legislators — to make such commands. And one may then ask whether anything explanatory is added by discussing whether the powers are "genuine" as opposed to "believed in and thus effective."

5   FUNCTIONALISM AND PARSONS' SYNTHESIS

In a different form, the issue of normativity did, in the middle of the century, have very large, and as I shall suggest, continuing impact, through Parsonsianism. To understand this form of the problem of normativity it is necessary to begin with

the very large body of thought that might be given the label "functionalist[34]."
This culminates in Talcott Parsons' attempt to organize the social sciences under
the concept of "system." This attempt drew on and incorporated both American
"cultural anthropology" and British "social anthropology," and combined this
with the "culture and personality" anthropology of the Freud-influenced middle
part of the 20th century. But it did much more, and its consequences for the
history of not only sociology, but anthropology and area studies, were substantial.

Anthropological research employed "function" as an organizing idea, in which
such things as rituals were interpreted as serving hidden or misrecognized pur-
poses. Functional explanations appeared to provide novel explanations of mysteri-
ous facts, namely the apparently pointless or misguided rituals of primitive people.
But much of this "explanation" was no more than the invention of hypothetical
teleologies leading to some supposed good, such as the Durkheimian purpose of
increasing social solidarity. There were differences in emphasis, between analyses
that emphasized functions for society as a whole and those that emphasized the
collective meeting of basic human needs. As "theory" these ideas, such as the idea
of the functional requisites of society, were truistic or definitional, and the late in
life attempts by such figures as Bronislaw Malinowski to formulate functionalism
as a theory were unable to go beyond such results as these "axioms":

> A. Culture is essentially an instrumental apparatus by which man is
> put into a position where he is better able to cope with the concrete
> specific problems that face him in his environment in the course of the
> satisfaction of his needs.

> B. It is a system of objects, activities, and attitudes in which every
> part exists as a means to an end. [1944, 150].

Such claims were non-explanatory, and non-empirical: they were true, to the extent
that they could be said to be true, by definition, or were definitions is disguise. The
definitions, however, rested on a problematic intuition, the idea that institutions
or customs that persisted over time must serve purposes or they (or the societies of
which they were a part) would fail to persist.[35] This notion was an inheritance of
the organic analogy, which contributed many other ideas as well, such as the notion
that societies were homeostatic, equilibrium seeking beings. But, like the organic
analogy itself, it was difficult to make it into more than an analogy. Societies
did not "die," and the "ends" of "society," such as solidarity, were hypothetical.
The development of this body of thought also presents difficult historiographic
problems in relation to the philosophy of science because there was no single
strand of methodological reflection that paralleled it or informed it. Indeed the

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[34]For a full discussion of functionalism, see Kincaid, this volume.

[35]Claims of this sort were an easy mark for Claude Levi-Strauss, who was later to generate
many examples of rituals and ordering practices that had no apparent "function." Nevertheless,
the notion of function outlined by Malinowski is so broad that it is difficult to see how these
examples, or any others, could conflict with them in a strict sense, since by definition they are
part of a whole that does function in Malinowski's minimal sense.

philosophical affinities of organic and functional thinking changed frequently, and the issues were often mixed up with considerations from other areas of philosophy, notably political philosophy and ethics, as well as metaphysics. Alfred North Whitehead's account of the "organic" in his metaphysics, to take one especially outré example, was one source cited by Parsons [1937, 32].

One may wonder why this mishmash of ideas became, as it did become, overwhelmingly dominant in the middle part of the 20th century. Part of the answer has to do with the person and position of Talcott Parsons, functionalism's main leader, his unique situation, and the role of his thought in the definition of the disciplinary identity of sociology. Parsons was trained as an economist, but in the German historical fashion, and was soon marginalized in the Harvard department that originally hired him. He found other academic protectors at Harvard, notably in connection with a group known as the Pareto Circle, an interdisciplinary reading group with a membership including various powerful Harvard grandees such as L. J. Henderson and Walter Cannon. Members of this group were central to the transformation of Harvard into a modern research university. Parsons was a remarkably adept political player. By the forties he had created his own department, called Social Relations, based on his model of social science theory, including [36]portions of psychology and anthropology. This department had a significant role in the creation at Harvard of the new field of area studies, which also transmitted his ideas. He was generously funded in the late forties by the Carnegie Foundation. And the international impact of his thought was greatly increased by the new dominance of American social science after World War II.

The intellectual basis of this enormous institutional success and intellectual influence was, remarkably (and for sociology unprecedentedly), a project in "theory." How did Parsons construct such an influential set of ideas out of the unpromising and theoretically thin material of functionalism? The "philosophical" background provides some answers to this question. It would be a mistake to take the standard version of the story of the rise of functionalism and systems thinking provided by Parsons himself too seriously, but it is the inevitable beginning point. In the thirties, Parsons wrote an influential study, *The Structure of Social Action* [1961], which purported to be an empirical study of key thinkers in the social sciences, Marshall, Pareto, Durkheim and Weber. In it, Parsons claimed that between these thinkers there was massive historical "convergence" toward a model of social action that he described in the book — Parsons' own model. But Parsons did not claim the model for himself: he claimed instead to be merely describing the "emergence of the theoretical system" [1937, 14]. In many respects, however, this was a project of second order synthesis between approaches that closely resembled the *a priori* system-building of his German contemporaries, of which he was well aware. As a synthesis, it was a genuinely remarkable effort: the huge differences between Durkheim, Weber, "positivism," and the model of rational action of mod-

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[36]Even archeology was influenced by this novel alignment. The Kluckhohns, discussed in Wylie's chapter, were part of the Parsons group, and the "normative" approach she discusses derives from Parsons.

ern economics have ordinarily been seen as examples of incommensurability and an argument for irreducible theoretical and methodological pluralism. Parsons took on the task of showing that they could be reconciled into a common "conceptual scheme."

The salient feature of Parsons's model of human action was normativity, and in particular his own attempt to resolve the conflict between what he called "idealism" and "positivism" [1937, 282]. This attempt involved a rejection of positivism understood as a reduction of values to something else, an argument with striking similarity to the normativist claims discussed in the last section. "The inner sense of freedom and moral choice," he argued, "is just as ultimate a fact of human life as any other, as is its consequent, moral responsibility. In fact, a psychological explanation of moral obligation really explains away the phenomenon itself" [1937, 290], and he specifically mentions the problem of the binding character of obligation which he takes to imply "metaphysical voluntarism" [1937, 289-90]. The facts of morality thus implied that "the world of 'empirical' fact must only be a part, only one aspect, of the universe in so far as it is significant to man. . . . it is something transcending science" [1937, 290]. Ends, in short, were real, essential to the proper understanding of action, and irreducible to the "scientific." With this, he seems to have dismissed Mises's notion that everything could be assimilated to the subjective theory of value, and indeed he makes comments to the effect that certain ends could not be understood as culminating in subjective states [1937, 288]. The relation of this thesis to Weber was complex. Placing values on the side of "reality" distanced him from Weber, who assimilated them to choice. Yet Parsons was driven to his notion of the reality of "moral obligation" and values by the same means-end model that Weber employed in this account of *Zweckrationaltät*: that ends are "precisely the element of rational action that falls outside the schema of positive science" [1937, 288]. Where Weber had distinguished this-worldly and otherworldly ends, Parsons called the latter "transcendental ends" [1937, 219]. And, crucially, he dissents from Weber's view that the pursuit of ultimate ends leads not to a single good but to a situation analogous to polytheism, a kind of value pluralism [1937, 294].

Parsons claimed to have "cogent reasons" [1937, 294] for rejecting value pluralism. But these turn out to be arguments relating to social order. A situation of value pluralism,

> would be . . . a war of all against all — Hobbes's state of nature. In so far, however, as individuals share a common system of ultimate ends, his system would, among other things, define what they all held their relations ought to be, would lay down norms determining these relations. . . . In so far, then as action is determined by ultimate ends, the existence of a system of such ends common to the members of the community seems to be the only alternative to a state of chaos — a necessary factor in social stability. [1937, 295]

Parsons goes on to claim that there is "much empirical evidence that such systems

of ultimate ends exist and play a decisive role in social life" [1937, 295]. He also identified a psychological basis for the link between action and "common ends," "the fact of experience that men . . . in some sense try to conform their action to patterns which are, by the actor and other members of the same collectivity, deemed desirable" [1937, 76].

This "argument" is faulty on many levels. The "in so far" clauses are ambiguous between the analytic claim that each individual action with an end has an ultimate end, the claim that communities share systems of ends, and the hypothetical conditional applying to those communities, which may or may not exist, that have such a system of ends. Parsons can establish the first by definition, but the remaining claims do not follow and do not even seem to be plausible. As it stands, the reasoning is a variant on the erroneous leap, often attributed to Aristoteleans, from the idea that all chains must end somewhere to the idea that there is a somewhere that all chains must end — in this case end in social values that are a guarantor of social stability. Weber, in contrast, thought that the simple fact of a common interest in a variety of intermediate ends, such as the authority of the state, provided a sufficient basis for social stability without any need to share ultimate ends,[37] and also that "ideal" elements played a minor role in the cultivation of the stable patterns of action, and that habituation, convenience, self-interest, and rational responses to power were largely sufficient as explainers.

The conceptual analysis in *The Structure of Social Action* was basic to the first step in this argument — that all action was ultimately and essentially oriented to the valuative. As we have seen in connection with Mises, however, there is a problem with the logical status of such claims. Are they merely definitional and stipulative? Ideal-types? Parsons's responded to this question in terms of the concept of conceptual schemes. Normative considerations were part of a complete conceptualization of social action. And this produced its own puzzles, in the form of questions about what he meant by a "conceptual scheme" and what sort of necessities attached to one, and why (and how) completeness was a desideratum. Parsons did not attempt to provide any philosophical explication of this project, but rather took the view that his own activities as a "theorist" made sense to him, and that others could come up with a suitable philosophical rationale for the completed project.[38] However, there was a basic motivating idea: providing a conceptual scheme was understood as part of the project of making sociology a science.

Parsons was acquainted with the neo-Kantian model of conceptual refinement, which he imbibed as a student in twenties Heidelberg. He was also well-aware of Whitehead's Harvard writings of the twenties, which he cited repeatedly. But the term conceptual scheme has a specific Harvard history apart from these sources. It was a key term of his sponsor Henderson and part of a well-developed view of

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[37]This is an argument that is made explicitly in a text by Gustav Radbruch, Weber's protégé in legal philosophy [1950].

[38]As Bruce Wearne has shown in detail, Parsons actively avoided being pinned down on philosophical issues about the nature of his theory [1989].

science presented in Henderson's Harvard lectures and various other writings, also cited by Parsons [Parsons, 1937; Henderson, [1932]1970]; cf. [Henderson, 1970]. Henderson, like Parsons and Merton, quoted Whitehead with approval. Yet Henderson also quoted Percy Bridgman and Carnap with approval, especially in the 1932 philosophical article cited by Parsons. The thesis that claims about reality are meaningless is taken from Bridgman, on the grounds that "no operation can be agreed upon as a definition of the word reality" (Bridgman cited in [Henderson, 1970, 167]), and the notion of meaninglessness is applied to a table of claims, divided into factual and meaningless, in which the notions of meaningless and having no corresponding operation are assimilated to Pareto's notion of non-logical conduct [1970, 179-80]. "Conceptual schemes" figure in this account as well. Poincaré and Einstein are cited against the error of "endowing the conceptual world with absolute qualities" [1970, 165], Henderson's discussion of "fact" ([1932]1970), quoted at length by Parsons, defined it as "an empirically verifiable statement about phenomena in terms of a conceptual scheme" (quoted in [Parsons, 1937, 41]). The concept was a precursor to Kuhn's notion of paradigm, and shared the basic idea. Whitehead's discussion of the desiderata for systems of speculative philosophy is a fair statement of the desiderata for conceptual schemes as well.

> Speculative Philosophy is the endeavor to frame a coherent, logical, necessary system of general ideas in terms of which every element of our experience can be interpreted. By this notion of "interpretation" I mean that everything of which we are conscious, as enjoyed, perceived, willed, or thought, shall have the character of a particular instance of the general scheme. Thus the philosophical scheme should be coherent, logical, and, in respect to its interpretation, applicable and adequate. Here "applicable" means that some items of experience are thus interpretable, and "adequate" means that there are no items incapable of such interpretation. [Whitehead, 1929, 5]

This formulation, with its special stress on "logic," necessity, and completeness fits Parsons's actual form of argumentation closely.[39]

Parsons did not package his thought as speculative philosophy, though in practice he worked, as did speculative philosophers and neo-Kantians, *post hoc* and on pre-existing conceptual material, and he wanted to provide the basis of a science, like the German system-builders. But the science to which he aspired was of a quite different kind. In *The Structure of Social Action* itself we are provided with pages of equations [1937, 78-82]. And in "The Present Position and Prospects of Systematic Theory in Sociology [1945]," we are told that the millennium has arrived:

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[39]One might be misled, by Whitehead's later reputation as a theologically inclined metaphysician, into thinking that this kind of argumentation stood at the opposite pole from the concerns of Logical Positivism. But this would be an anachronistic judgment. Whitehead was concerned with and wrote on the same issues of underdetermination that motivated the break between the Logical Positivists and the neoKantians, and Whitehead himself was still thought of as a logician and philosophical interpreter of science.

> Sociology is just in the process of emerging into the status of a mature science. Heretofore it has not enjoyed the kind of integration and directed activity which only the availability and common acceptance and employment of a well-articulated generalized theoretical system can give to a science. The main framework of such a system is, however, now available, though this fact is not as yet very generally appreciated and much in the way of development and refinement remains to be done on the purely theoretical level, as well as systematic use and revision in actual research. It may therefore be held that we stand on the threshold of a definitely new era in sociology and the neighboring social science fields. ([1949]1954, 212)

If this sounds like the proclamation of a Newtonian revolution, it is no accident. He explains that the model for such a theoretical system was classical mechanics, because of its "possession of a logically complete system of dynamic generalizations which can state all the variables of the system . . . All other sciences are limited to a more 'primitive' level of systematic theoretical analysis" ([1949]1954, 212). "Functional analysis" provided the surrogate for completeness. It "appears," as Parsons says, " to be the only way in which dynamic analysis of variable factors in a system can be explicitly analyzed without the technical tools of mathematics and the operational and empirical prerequisites of their employment" ([1949]1954, 218). Here his scientific model shifts from mechanics to "structural functional analysis in physiology" as exemplified by Cannon's *The Wisdom of the Body* (1932, cited in Parsons [1949]1954, 218). Cannon, a sometime member of the Pareto Circle, popularized the notion of homeostatic mechanisms. We will shortly see the philosophical reasons why mechanics and physiology seemed for Parsons to be similar.

The idea of a *project* of providing a conceptual scheme is odd, if one considers the Kuhnian notion of paradigm as the lineal descendant of the concept (as it was, by way of James Bryant Conant, Kuhn's mentor). Yet in Henderson's usage this made a certain amount of sense. Henderson thought of Gibbs's physics as a model of scientific development, and understood Gibbs's physical model of static equilibrium as a paradigm case of the development of a conceptual scheme, and used it as an example both in his key 1932 philosophical article [1970, 163], and in a more elaborate way in his *Pareto's General Sociology: A Physiologist's Interpretation* ([1935]1967, 14). His comments match closely with those of Parsons:

> Gibbs's system is plainly a fiction, for no real system can be isolated. . . . So results are obtained and then extended even to systems that are far from isolated. Also, the enumeration of the factors, i.e. concentrations, temperature, and pressure, is incomplete . . . In other cases the consideration of other factors, like those involved in capillary and electrical phenomena, cannot be avoided. Sometimes, however, such considerations can be introduced after the first analysis in the form of "corrections." . . . such apparent defects are in truth

consequences of very real advantages. They are but signs of the well
chosen simplifications and abstractions that make possible a systematic
treatment of complex phenomena. ([1935]1967, 15)

The picture here is that the major task is to make a good approximation that
captures the major variables, and fill in the details later. Henderson thought this
was a good model for physiology, and this is what Henderson thought Pareto had
successfully done for society. "Pareto's social system contains individuals; they are
roughly analogous to Gibbs's components .... As Gibbs considers temperature,
pressure, concentrations, so Pareto considers sentiments, or, strictly speaking,
the manifestations of sentiments in words and deeds, verbal elaborations, and
the like" ([1935]1967, 16). Parsons' model, which relied so heavily on aprioristic
considerations, would not seem to lend itself to this interpretation, as we have
seen, but Parsons himself understood matters differently. The theory was made
scientific by the concept of equilibrium: social stability was guaranteed by the
equilibrating processes of society. Because Pareto was the point of comparison,
these claims did not seem peculiar, nor did the fact that these speculations had
little connection with empirical data. As Henderson says, "Pareto's social system
is an invaluable conceptual scheme, but . . . it is now, and will probably long
remain, an implement of limited usefulness in the digging up of data" ([1935]1967,
95)

Philosophers who looked into the Parsons phenomenon came away perplexed.
The most elaborate study was made by Max Black, who concluded that, once
translated from the jargon which Parsons invented, the theoretical claims that
could be identified were truisms.[40] This is a fair sample of Black's translations:
Parsons's model of action becomes "Whenever you do anything — you're trying to
get something done"; his claim that normativity is essential becomes "Choosing
means taking what seems best for you or what others say is the right thing";
the systems claim becomes "Families, business firms, and other groups of persons
often behave surprisingly like persons" [Black, 1975, 279]. Black questions whether
these really say anything, or, as he puts it, "whether it is plausible for fundamental
social theory to be so close to common sense" [1975, 279]. Parsons's reply to Black
retranslates these claims back into his dichotomous category scheme of pattern
variables, and, unmoved by the question of whether they go beyond common sense,
claims that "on the question of the pattern-variables, I think it can now be said
that they *are* essential and that they are exhaustive" [Parsons, 1975, 336; emphasis
in the original]. By "essential," he explains, he means that if the pattern-variables
"were not used, essentially the same concepts under different names would have
to be introduced" [1975, 336].

So what did Parsons provide other than a systematized jargon and truisms?
The answer can be found in part in connection to the problem of disciplinary
development and one non-commonsensical claim about central values. To put the

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[40]The idea that truisms might actually be the explainers that backed historical explanations
was seriously explored in the philosophical literature [Scriven, 1959].

point simply, the convergence that Parsons manufactured out of the writings of Weber and Durkheim had the effect of producing a distinctively "sociological" conception of values in which values were both essential to action explanation, thus providing an ineliminable and basic place for the science that studied them, and at the time made values essentially social, and, through the confused argument described earlier, in some sense necessarily univocal This meant that values could no longer be understood as individual choices, as they were for Weber (and more generally for the economists), but had to be understood as something distinctively "sociological," namely as the contents of a central value system which played the role in regulating action that the conscience collective had played for Durkheim. The dogma, as one of Parsons' students, Bernard Barber, put it, was this:

> The structure of values in a social system influences action at all levels, from interaction in small groups to that in the total society. This happens as very general values such as equality or rationality are made more specific in the form of norms for more specific interactive relationships. [1998, 39]

For Parsons the central value system was the key component of the process of equilibration that produced social stability [1975, 336]. This language became intensely unfashionable in the sixties, when it was taken to be an affirmation of an ideological representation of a consensus model of society that was at variance with the evident social and political conflicts of the period. Yet the model survived in other forms. It re-emerges, for example, in the writings of his student Clifford Geertz in the guise of interpretivism and in the form of the thesis that the mind is full of assumptions, frameworks, and templates. The transition in Geertz's own usage can be traced through his work. He continued, for a time, to think of society as having a strong center, which was "symbolic."

> At the political center of any complexly organized society . . . there is both a governing elite and a set of symbolic forms expressing the fact that it is in truth governing . . . It is these — crowns and coronations, limousines and conferences — that mark the center as center and give what goes on there its aura of being not merely important but in some odd fashion connected with the way the world is built. The gravity of high politics and the solemnity of high worship spring from liker impulses than might first appear [1977, 152-3].

And Geertz provided a mechanism that was a surrogate for Parsons' notion of a psychological basis of conformity, arguing that without the assistance of cultural patterns

> a human being would be functionally incomplete . . . a kind of formless monster with neither sense of direction nor power of self-control, a chaos of spasmodic impulses and vague emotions [1973, 99]

But he also used the language of postmodernism, thus providing a bridge by which Parsonians, such as Ann Swidler and Jeffrey Alexander, could emerge as "cultural sociologists," re-labeling the central value system as culture, which was conceded to be more "plural."[41]

As we have noted in relation to "mainstream sociology," Parsonianism was not the only strategy for "scientizing" sociology during the mid-century period. But similar issues arise with the other major examples that attracted philosophical commentary. Alfred Louch examined the explicitly propositional behavioral theory of Parsons' colleague and rival George Homans and concluded that the claims were true by virtue of the interdependence of the definitions of the key terms [1966]. Symbolic interactionism, as theory, was, like Parsonianism, a set of conventions for the redescription of action in other terms. In addition to these "grand" theoretical approaches, there was a systematic attempt to reconstrue ordinary empirical social research in "theoretical terms," motivated in part by the logical positivist idea that theory was essential to science. Paul Lazarsfeld and Robert Merton promoted the idea of what Merton called middle-range theory, and many books with titles similar to "Theory Construction" were published. But despite their collaboration and contact with Nagel in the classroom, and Lazarsfeld's ongoing discussions with such philosophers as Patrick Suppes, nothing recognizable as "theory" in the classical Logical Positivist sense developed. This story, though it can be traced to debates in the late thirties, is almost entirely a post-1945 matter, and thus beyond the limits of this chapter.[42]

6   EPILOGUE: AFTER 1945

Apart from a few prospective remarks, I have closed this account at a time before logical positivism had its full impact and before ordinary language philosophy developed its distinctive critique of the idea of causal explanation of human action. The consequences of these two movements were profound for what was to become philosophy of social science in the sense that they shaped the language and issues of philosophy of social science as it emerged as a subfield of philosophy. Their relations to the disciplines of sociology and anthropology are more ambiguous. These fields were established as disciplines by 1945, and took on their current

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[41]This is the background to the story of the rise of cultural sociology told by Zammito in this volume.

[42]Hage's chapter deals with his own efforts in this direction, and there was a program of formal theory construction that attempted to provide such theories. Some of these books attempted to treat correlational analysis as "theory" (e.g. [Stinchcombe, 1968; Blalock, 1969], while others, particularly a group of Stanford sociologists, promoted particular theoretical programs as exemplars of genuine theory [Berger and Zelditch, 1993]. These programs remained marginal, and did not concern themselves with topics that were conventionally regarded as important. Other works, by "theorists," formulated propositions in the form "the greater the x, the greater the y," following the influential book by Zetterberg, *On Theory and Verification in Sociology* ([1954]1963), under the impression that such statements could be treated as though they were confirmed by evidence of statistical correlations between x and y (cf. [Turner, 1974; 1987].

form in the years after the war. Philosophy increasingly faced those disciplines as established facts rather than as hypothetical possibilities. Many of the issues that arose in the course of defining the disciplinary boundaries and character of the disciplines, such as the problem of normativity and holism, persisted. But much changed.

In the form of active research disciplines, sociology and anthropology presented some challenges that earlier discussions had not grasped. The argument of ordinary language philosophy that "reasons" accounts precluded the possibility of causal explanation of action, was applied to sociology by Peter Winch in his classic *The Idea of a Social Science and its Relation to Philosophy*[43] [1958]. Winch was presented with the obdurate anthropological reality that the "reasons" primitive peoples provided for their actions were often not unproblematic and *a priori* valid, but instead unintelligible as genuine "reasons."[44] His response to this problem in "Understanding a Primitive Society" ([1964]1970) produced a whole field in the philosophy of social science on rationality, and led to two influential volumes [Wilson, *Rationality*, 1970; Hollis and Lukes, *Rationality and Relativism*, 1982]. These issues turned out to have significant implications for philosophy generally. Davidson linked this form of the problem of rationality with the problem of incommensurabilty in Kuhn. He salvaged the notion of rationality by defining it in terms that retained its *a priori* character at the cost of relativizing it to our purposes — a position reminiscent of Weber's relativization of the explanatory concerns of social science to our interests. But he noted that beliefs that we cannot understand as rational in something akin to our terms we cannot judge to be irrational.

Logical positivism in the period of the unity of science movement had anticipated the transition from understanding social science as a hypothetical possibility to understanding it as a going concern. The basic impulse of the unity of science movement was to show that all sciences and all scientific knowledge could be assimilated to a single, logically integrated structure. This went with an astonishing openness to the social sciences, even an eagerness, as Neurath said, "to abandon for good the traditional hierarchy: physical sciences, biological sciences, social sciences, and similar types of 'scientific pyramidism'." [1944, 8]. The *International Encyclopedia of Unified Science* made an effort to include sociology and economics, and did so not in terms of hypothetical possibilities but treated them as actual fields of knowledge with pre-existing logical structure open to analysis and with their own methodological literature. Neurath's volume *Foundations of the Social Sciences* [1944], is studded with references to George Lundberg's methodology book *Social Research* [1942], and deals with such topics as the appropriate use of index figures, disapproving of the use of ordinal numbers as though they were cardinal numbers — and measurement [1944, 33-4][45]. It recognized the role of the

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[43]A title which itself reflected the view of social science as a hypothetical possibility rather than as existing intellectual enterprises.

[44]The literature on this topic is discussed in the chapter by Lukes in this volume.

[45]The history of this topic, which developed largely in psychology and in the areas of social

ubiquitous correlations that social researchers actually deal with, rather than simply restating the model of scientific law. Indeed, the tolerance Neurath practiced here was extended to functional explanation in social science [1944, 22], and, as I have already noted, this was later echoed by philosophers such as Hempel and by Ernest Nagel [1961].

The emergence of logical positivism in the United States after 1945 as a kind of philosophical orthodoxy coincided with the rapid expansion of the "behavioral sciences" and the reformulation of their "scientific" ambitions, a reformulation in which the idea of "theory" played a large role. Indeed, the movement was defined by the rapprochement of theory with empirical sociology, under such slogans as "middle-range theory" and by a considerable optimism about the scientific prospects of these disciplines. The optimism contrasted with the empiricism of the older Pearsonians, who believed that statistical sociology could only yield correlations. The logical positivists' emphasis on theory, though it could only with difficulty be reconciled with the available "theories," validated the idea that to be scientific a discipline needed theories, and this was consequential.

The two-way traffic between sociology (and to a more limited extent anthropology) and the philosophy of science shaped both sides of the exchange. Not only did philosophers take sociological and anthropological issues on board, sociologists and anthropologists took these philosophical issues about explanation and logical form seriously. Scientistic sociologists redefined science in terms invented by logical positivism. But the mutual attraction between the two was ill-fated. "Functionalism," which the logical positivists attempted to accommodate, was, to paraphrase Sidney Morgenbesser's remark on pragmatism, dysfunctional. It failed to meet the needs of these disciplines, and both it and the "positivist" program in sociology lost its credibility during the sixties and seventies. The "theoretical concepts" which logical positivism was used to legitimate, such as the social psychological concept of attitude, did not lead to "theories" of interest. Ironically, the philosophical critics of the scientistic pretensions of social science turned out to have a longer and more influential afterlife in sociology than the positivism they critiqued. The critique of sociology in Winch, for example, found a ready-made audience among the opponents of "positivism" and was absorbed into such movements as the Wittgensteinian version of ethnomethodology.

The discussion of the earlier 20th century methodological and philosophical literature in this chapter suggests that the issues contemporary philosophy of science raised were novel only in form. The problems of perspectives, pluralism, normativity and the *a priori*, as well as the problems of measurement, cause and correlation, and the nature of rationality and action, were there, and in a developed form, very early. In a sense these problems were obscured by the episode of mutuality mediated by logical positivism during the period of the new scientific ambitions of the "behavioral sciences" immediately after 1945, and have now returned to their original centrality.

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psychology shared with psychology, is discussed in this volume in the chapter by Michell.

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MEASUREMENT

Joel Michell

In *Styles of Scientific Thinking in the European Tradition*, A. C. Crombie observed that one 'could dramatize the history of scientific thinking from Greek antiquity as a continuous attempt by mathematics to impose everywhere a simple, homogeneous, postulational, axiomatic form of argument met by a continuing resistance from the complex, heterogeneous enigmas of experience' [1994, 93]. Given that within physics this mathematical 'imposition' was via measurement, it is unsurprising that quantitative imperatives pervade the history of social science. To some social scientists it seemed that 'we must measure what is measurable and make measurable what cannot be measured.'[1] Emerging soon after the Scientific Revolution, this attitude dominated social science in the past century. From the early 1900s, practices routinely described as 'methods of social measurement' have been employed, and while these mimic methods of physical measurement, they differ sufficiently to spark questions and success is too modest to allay doubt. This chapter traces the vicissitudes of the concept of measurement, as forced by the quantitative imperative across a terrain of questions and doubts.

1 MEASUREMENT IN SOCIAL SCIENCE

The social scientist, Louis Guttman, wrote that 'I was taught by a number of teachers, and read in many books, that the social sciences — especially sociology and psychology — could become "scientific" only to the extent to which they progressed in "measurement"' [1971, 329]. This was typical of social science education then, but it is worth noting that the quantitative turn in social science did not begin in the twentieth century. So powerful were the examples set by Galileo, Harvey, and Newton that social philosophers explored the possibility of quantification in the immediate wake of the Scientific Revolution [Cohen, 1994]. However, to theorise quantitatively is one thing, to quantify, quite another. Quantification leads to numerical data and it was only with the development of social surveys [Converse, 1987; Kent, 1985; Yeo, 2003] that social scientists acquired data like this. While social survey data involves quantification of a kind, it is not necessarily measurement. In antiquity, Aristotle distinguished two sorts of quantities: 'A quantum is a *plurality* if it is numerable, a *magnitude* if it is measurable' ((my italics) *Metaphysics*, 1020^a, 9-10 [McKeon, 1941, 766]). Pluralities are discrete quantities and their assessment is by counting frequencies (identifying natural

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[1] As Galileo is reported to have said in a different context (quoted in Klein [1974, 509]).

numbers), while magnitudes are continuous quantities and theirs is by estimation of ratios (or real numbers). In physical science, measurable attributes are taken to be continuous, and as a body of theory, physics involves laws relating continua. As long as social surveys serve up frequencies, nothing is thereby *measured* and social science is not quantitative in the sense that physics is. As long as physics is the model, pressure to conceptualise social attributes as continuous magnitudes and to develop methods for their measurement continues.

At the beginning of the twentieth century, because of links to philosophical, religious and political movements [Turner and Turner, 1990], social science was methodologically eclectic. However, institutionalisation and professionalisation produced trends toward unification and many social scientists wanted a unity that gave priority to measurement. Displayed upon the Social Science Research Building at the University of Chicago is a slogan (attributed to the physicist, Lord Kelvin): 'When you cannot measure your knowledge is meagre and unsatisfactory'. It was part of a political campaign promoting measurement as the primary method of social science [Bulmer, 1981; 1984]. A little later, economic forces cemented measurement's place. In 1950, Truman signed an act creating the National Science Foundation. Apprehensive lest they be denied financial support for research, the Social Science Research Council crafted a 'public relations strategy that defined the social sciences as part of a unified scientific enterprise' [Solovey, 2004, 394], the unity being methodological and emulating 'hard' science. Research published in American sociological journals from the 1920s to the 1960s became 'increasingly empirical and quantitative in character' [Platt, 1996, 196].

An example is the measurement of social attitudes. The methods still remain as described by Green [1954]. Developed without clear ideas about what attitudes are, Green was able to report half a dozen proposed definitions and now there are more (e.g., [Chaiken, 2001]). However, transcending uncertainty about the concept of attitude, Green was sure that the set of possible attitudes towards some specific social issue (such as, say, abortion) constituted a 'latent variable'. In the discourse of social measurement, this is a hypothetical attribute (i.e., one not directly observable). A 'manifest variable,' on the other hand, is an observable attribute, such as overt responses to a questionnaire. Green thought that 'to obtain a more precise definition of an attitude, we need a mathematical model that relates the responses, or observed variables, to the latent variable' [1954, 336]. Such models may be necessary, but history has demonstrated that they are not alone sufficient.

The methods proposed for measurement of attitudes typically ask people to make judgments about statements expressing, directly or indirectly, specific attitudes towards the relevant issue. Each method is based upon a theory about how judgments relate to the attitudes of the judges or to the attitudes expressed by the statements or both. The best known were proposed by Thurstone [1928]; Likert [1932]; Guttman [1944]; Lazarsfeld [1950] and Coombs [1950]. The most widely used is Likert's 'method of summated ratings', according to which people rate their degree of preference for each of a series of statements, typically expressing

extreme attitudes towards a targeted issue. An attitude measure is based upon the sum of a person's ratings, each rating being weighted positively or negatively according to whether it indicates a positive or negative attitude. Andrich [1996] proposed an elegant quantitative theory (based upon the contributions of Georg Rasch [1960]) that succeeds in unifying a number of these methods under a single theoretical umbrella, but, typical of the area, fails to articulate how the concept of attitude is able to accommodate more mathematical structure than mere order.

For a feature that theories of attitude measurement share is the hypothesis that latent variables are *quantitative*, that is, that their structure matches that of the real number line. This is essential if attitudes are to be measured in the same sense that physical attributes are measured. Despite this, experienced directly, attitudes manifest no more than ordinal structure. Quantitative structure is pure speculation. Of course, speculation is a one engine driving science, but, in relation to this issue, social science speculations differ from those of natural science in two respects: first, social science ignores the need to test the hypothesis that relevant attributes really are quantitative; and, second, in social science, there is no evidence supporting the hypothesis that attributes are quantitative. However, in so far as social scientists aspire to measure as physicists, testing whether attributes are quantitative and interpreting the structure of attributes in the light of evidence is indispensable. What is the source of this anomaly?

2   THE MEANING OF MEASUREMENT IN SOCIAL SCIENCE

In physics, the meaning of *measurement* is embedded within a conceptual network. Terrien [1980] characterised it thus:

> Quantities are abstract concepts possessing two main properties: they can be measured, that means that the ratio of two quantities of the same kind, a pure number, can be established by experiment; and they can enter into a mathematical scheme expressing their definitions or the laws of physics. A unit for a kind of quantity is a sample of that quantity chosen by convention to have the value 1. So that, as already stated by Clerk Maxwell,
>
> $$\text{physical quantity} = \text{pure number} \times \text{unit}.$$
>
> This equation means that the ratio of the quantitative abstract concept to the unit is a pure number. [765-6].

The reference is to Maxwell's *Treatise on Electricity and Magnetism*, which says,

> Every expression of a Quantity consists of two factors or components. One of these is the name of a certain known quantity of the same kind as the quantity to be expressed, which is taken as a standard of reference. The other component is the number of times the standard is

> to be taken in order to make up the required quantity. The standard
> quantity is technically called the Unit, and the number is called the
> Numerical Value of the quantity. [1891, 1].

Specific magnitudes of the same quantitative attribute stand in numerical rela-
tionships to each other. These relationships are of relative magnitude or *ratio*.
If some known magnitude of the attribute is taken as the unit, then each other
magnitude is specified by its ratio with it. Hence: **a *measurement* of a magni-
tude of a quantitative attribute is an estimate of the ratio between that
magnitude and whichever magnitude of the same attribute is taken as
the unit of measurement**.[2]

The most striking feature of quantitative social science is that the definition of
measurement typically given is nothing like this. Within social science, 'the most
widely accepted definition of measurement has been that advanced by Stevens:
"the assignment of numerals to objects or events according to rules"' [Sjoberg and
Nett, 1968, 271].[3] S. S. Stevens proposed the above definition of measurement
in 1946 and promoted it assiduously for thirty years [Stevens, 1946; 1951; 1958;
1959; 1967; 1968; 1975]. He need not have bothered. It was absorbed so rapidly
into social science's post-second world war methodological consensus that by 1954
it was described as 'classical' [Coombs *et al.*, 1954], a description unchanged
years later (e.g., [Salkind, 1994; Bulmer, 2001]). It was not universally accepted
(e.g., [Duncan, 1984]), but perusal of social science methodology texts published
over the past half-century confirms its dominance (e.g., [Black and Champion,
1976; Borgatta and Bohrnstedt, 1981; Crano and Brewer, 1986; Dominowski, 1980;
Francis, 1967; Frankfort-Nachmias and Nachmias, 1992; Galtung, 1967; Green,
1954; Lemon, 1973; Marlowe, 1971; Phillips, 1971; Shaw and Wright, 1967; Simon,
1969]).

Stevens's definition differs profoundly from the physical science definition. First,
according to Stevens, it is 'objects and events' that are measured, not magnitudes
of quantitative attributes. Quantitative attributes (such as mass and velocity, for
example) are *properties* of objects or events, or *relations* between them. It makes
no sense to talk of 'measuring objects or events,' unless it is attributes of them that
are measured. To define measurement, ignoring quantitative attributes, ignores
what measurement is *of*.

Second, according to Stevens, measurement involves *numerals*, not *numbers*.
Numerals are the signs used to denote numbers. Numbers, in so far as measure-
ment is concerned, are *relations* (i.e., ratios) between magnitudes of quantitative
attributes, and these relations are present, named or not. Confusing numbers
with numerals is like confusing a meal with a menu. Measurement involves nu-
merals because measures are sometimes recorded. Numerals are no more necessary
to measurement than other symbols (e.g., words) are to the recognition of other

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[2]I have departed slightly from the usage of Maxwell and Terrien. I use the term *quantity* to
denote the general class (say, the class of all possible masses) and the term *magnitude* to denote
specific instances of that class (say, the mass of an electron).

[3]The same is true in psychology. See [Michell, 1997a].

relationships.[4] We can know things without describing them and this includes quantitative features as much as any. Numbers are always present in measurement; numerals, not necessarily.

Third, according to Stevens, measurement involves the *assignment* of numerals to objects or events, whereas, measurement, as it occurs in natural science, is no more concerned with assignments than is any other cognitive act. Measurement is an attempt to come to *know* something (viz., the ratio between a magnitude of a quantitative attribute and the unit employed) and *knowing* is not *assigning*. When, for example, the colour of a traffic light is adjudged red or its shape, circular, the cognitive act is not that of assigning the words 'red' or 'circular' to anything because assigning words to objects involves no commitment to truth. On the other hand, judgment does, because in judging something to be the case we commit ourselves to the truth of the judgment. Measurement is a judgment about some matter of fact and, so, it is not a mere assignment of a symbol to something.

Fourth, according to Stevens, there is nothing, which, in principle, cannot be measured. Numerals can be assigned to anything. However, according to the physical science definition, the only measurable attributes are those that possess the kind of structure that sustains ratios. This kind of structure is a specific empirical condition and there is no logical necessity that any attribute should possess it. Because it excludes nothing from the scope of measurement, Stevens's definition is vacuous. Far from defining *measurement*, Stevens dissolved its meaning.

Given the physical science concept, measurement is *not* the assignment of numerals to objects or events according to rules. Endorsement of Stevens's definition within social science is, therefore, not just striking, but also puzzling. If the aim is to model social science on physics, then it might have been expected that social scientists would endorse the physical science concept. However, around mid-century, Stevens's definition seemed an ideal option to social scientists.

3   THE REPRESENTATIONAL THEORY OF MEASUREMENT IN THE TWENTIETH CENTURY

One new specialisation that flourished in the twentieth century was the theory of measurement. The disciplinary affiliations of contributors was diverse (philosophy, psychology, physics, and mathematics), but their contributions were generally to the development of a single idea, viz., that measurement is a species of numerical representation, the so-called, *representational theory of measurement*. Furthermore, most of these contributions relate to a single controversy, viz., the issue of whether measurement of psychological attributes is possible. This theory was energised during the twentieth century by disputes between those critical of psy-

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[4]For example, a draper doling out cloth by the ell [Fenna, 1998] might measure with a ritualised flourish from left shoulder to opposite, outstretched hand. No *numeral* is involved, but the *number* (the ratio between the cloth's width and the ell) is observed. Another example was Galileo, who, when discovering his law of free fall, measured regular stretches of time, it is said, by noting beats in a tune he sang [Drake, 1990].

chological measurement and those defending its possibility. It attended little to the range of philosophical views about number (e.g., to those considered in Irvine [1990]), to the history of measurement in science (i.e., either to its full sweep (e.g., [Roche, 1998]), to particular attributes (e.g., [Chang, 2004]), or to the related discipline of metrology (the science of physical measurement) (see [Knorring and Solopchenko, 2003]). It has been, largely, the pursuit of one idea in the context of one controversy.

3.1   Bertrand Russell and the Origin of the Representational Theory

The representational theory of measurement was first proposed[5] in 1903 in Bertrand Russell's *Principles of Mathematics*. Russell needed a new concept of measurement because he rejected[6] the traditional view that numbers are ratios of quantities and its associated concept of measurement. According to the traditional concept, number and quantity are logically connected, which dovetails neatly with the physical science concept of measurement. Despite this, Russell became convinced that all of the concepts of mathematics, including those of number, are completely definable in terms of purely logical concepts. This was part of his 'logicism'[7]. According to Russell [1903], a quantitative attribute, such as length, is a class of properties, all of the same general kind, strictly ordered according to magnitude and, so, Russell thought, it entails the existence of things beyond logic. Numbers, he thought of as classes of classes: *one* is the class of all singletons; *two*, the class of all pairs; *three*, the class of all triples; and so on. Thus, numbers require nothing beyond the purely logical concept of class. From this he concluded that the 'separation between number and quantity is complete: each is wholly independent of the other' [1903, 158].

Therefore, a new concept of measurement, one respecting this independence, was required. Russell proposed that

> [m]easurement of magnitudes is, in its most general sense, any method by which a unique and reciprocal correspondence is established between all or some of the magnitudes of a kind and all or some of the numbers, integral, rational, or real, as the case may be. ... In this general sense, measurement demands some one-one relation between the numbers and magnitudes in question — a relation which may be direct or indirect, important or trivial, according to circumstances. [1903, 176].

Russell's view was that there is nothing intrinsic to the concept of magnitude[8] that entails any particular numerical measure for any specific magnitude. The

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[5]For a discussion on the origin of the representational theory see Michell [1993a].

[6]Russell's reasons for rejecting this were given in *Mind* [Russell, 1897]. His complex and arcane arguments are analysed in Griffin [1991] and Michell [1997b; 2003].

[7]This term was first used by Carnap [1929] to describe the view of Russell and others that all of pure mathematics follows from the axioms of logic.

[8]For Russell, *magnitudes* are universals. For example, each specific length is a magnitude (of length). Two objects may be of the same magnitude, but no two, distinct magnitudes can be equal. He held that the magnitudes of a kind (or 'species' as he calls them) — say, the class of all

numbers used result, he held, from the fact that all of the magnitudes of a species form a series, as does each of the various kinds of numbers and the elements of any two distinct series may correspond. It was Russell's view that one to one correspondences (or isomorphisms) between the magnitudes of a species and, say, the real numbers, exist and measures of the magnitudes result from locating any such correspondence.

He saw the task of locating such a correspondence as a 'practical' one and did not concern himself much with it, other than to say this:

> Concerning measurement in the most general sense, there is very little to be said. Since the numbers form a series, and since every kind of magnitude also forms a series, it will be desirable that the order of the magnitudes measured should correspond to that of the numbers, *i.e.* that all relations of *between* should be the same for magnitudes and their measures. Wherever there is zero, it is well that this should be measured by the number zero. These and other conditions, which a measure should fulfil if possible, may be laid down; but they are of practical rather than theoretical importance. [Russell, 1903, 176].

However, he conceded that some species of magnitude possess more structure than mere order. For instance, lengths possess 'additive' structure: one length may be, say, twice another. This would seem to imply that some magnitudes stand in numerical relations to one another, e.g., the ratio of one length to another may be two. In this sense, number appears to be intrinsic to some species of magnitude, contrary to Russell's logicism.

Russell responded that ratios attributed to lengths hold only in virtue of relations between *objects* possessing length: any object will be entirely composed of two discrete parts each of the same length. He stressed that this relation between objects does not mean that any length is thereby divisible into two equal lengths because 'to divide a magnitude into two equal parts must always be impossible, since there are no such things as equal magnitudes' [1903, 178]. However, were this argument valid, an analogous point would apply to Russell's numbers: while any class having just two members is divisible into discrete classes, each a singleton, it would be nonsense to assert that the number *two* (i.e., according to Russell, the class of all pairs) is thereby divided into equal parts, each the number *one* (where *one* is understood as the class of all singletons). So, if Russell's argument means that magnitudes do not really stand in numerical relations, it also means that numbers do not really stand in numerical relations, an obviously false conclusion. This means that Russell's reasons for inaugurating the representational theory of measurement were insufficient. If the ratio theory of number is not refuted, there is no reason to reject the physical science concept of measurement.

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specific lengths — constitute a strict simple order (i.e., are ordered by a transitive, asymmetric, and connected binary relation).

Neither of the other 'founders' of logicism, Gottlob Frege and Alfred North Whitehead, followed Russell's line. Frege [1884] proposed logicism well before Russell, but did not push it as far, considering it chiefly as applied to natural numbers. Later, as part of his logicism, Frege defended the traditional view that the real numbers are ratios of quantities [Frege, 1903].[9] A similar view was also proposed in volume three of *Principia Mathematica* [Whitehead and Russell, 1913] in the part on quantity and measurement, which was written by Whitehead.[10]

Russell's representational theory of measurement may be summarised as follows:

1. A quantitative attribute is a class of properties all of the same kind and ordered according to magnitude; and

2. Measurement is a one-to-one correspondence between a quantitative attribute and numbers of a given kind (integral, rational, or real).

The representational view is that measurement is a mapping of a kind between things of one sort and things of another. The various versions of the representational theory differ mainly with respect to these three components: the characterisation of the *field* that the mapping is *from*; the characterisation of the *range* that the mapping is *to*; and the characterisation of the *form of the mapping* itself.

Russell's characterisation of the field was liberal, in that magnitudes were said to be distinguished by order, not additivity, and, so, it included psychological attributes. In debates over psychological measurement, his views were cited (e.g., Dawes Hicks [1913]. At the time, psychological measurement involved attempts to measure intensities of sensations[11], using methods devised by G. T. Fechner [1860]. The consensus amongst experimental psychologists was that sensation intensities are magnitudes (in the sense of sustaining relations of more and less), not quantities (in the sense of sustaining additive relations) (e.g., Myers [1913] and Titchener [1905]). However, the proposition that sensation intensities are measurable was deeply controversial.

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[9]Frege's *Grundgesetze*, unfortunately, is still not published in English. However, useful discussions are given by Dummett [1991] and Simmons [1987].

[10]See Russell [1919] and Grattan-Guinness [2000, 408-410]. Quine [1941] and Bigelow [1988] summarise Whitehead's view.

[11]This is psychophysical measurement. Some psychologists argue that there is no separate realm of sensations mediating perception of the environment (e.g., Holt [1915] and Gibson [1979]). Perception is a direct relation between the person and the environment. Psychophysical measurement is then understood, not as measurement of something psychological, but as the 'measurement' of some feature of the environment via the person as an 'instrument' [Luce, 1972]. The dominant view, on the other hand, is that sensations are mental entities and psychophysical measurement is the attempt to measure their intensities. For a recent assessment of this enterprise see Laming [1997].

3.2 Norman Robert Campbell and the Reconstruction of Physical Measurement

Campbell's views on the theory of measurement were directly informed by his experience in physics[12] and they differed from Russell's in two important respects. First, for Campbell [1920], the field of the representational mapping consisted of two classes: (1) attributes possessing not just an order of magnitude, but also an additive structure (*quantities*, as he called them); and (2) attributes measurable only because they are implicated in numerical laws of nature (*qualities*). Second, for Campbell, the range of the representational mapping was not numbers, but *numerals*. He defined measurement as **the assignment of numerals to represent properties in accordance with scientific laws**. He dealt with the matters that Russell sidelined as merely practical and there is a complex fine grain to Campbell's theory that is glossed over here because the focus is on those of his ideas that affected Stevens's definition.

Campbell's best-known contribution is his distinction between 'fundamental' and 'derived' measurement.[13] According to Campbell, *quantities*, or 'A-magnitudes' [Campbell, 1928], are similar to numbers in possessing additive structure and they are always identified via specification of a concatenation procedure. When, for example, a rigid straight rod is extended linearly by another adjoined end to end with it, the length of the newly formed rod stands in a relation to the lengths of the concatenated rods that has the same form as the relation of addition between numbers, in the sense that it conforms to *associative* $(a + [b + c] = [a + b] + c)$ and *commutative* $(a + b = b + a)$ laws, a *positivity* law $(a + b > a)$, and *the Euclidean law* that equals plus equals gives equals (i.e., if $a = a'$ and $b = b'$, then $a + b = a' + b'$) [Campbell, 1928, 15]. Evidence that these laws are true of lengths is a matter of observation and experiment and, so, in this respect, they are scientific laws[14]. Therefore, the hypothesis that some attribute or other is a quantity raises an empirical issue. It can only be considered scientifically in relation to evidence. Measurement cannot come by fiat; but only by finding laws of nature.

If laws of this kind obtain for any attribute, said Campbell, then its similarity to number allows numerals to be assigned to magnitudes of the attribute. Magnitudes of quantities are measured fundamentally, said Campbell [1920, 280], by constructing a 'standard series'. This is a series of objects manifesting multiples of a unit magnitude. If u is the unit magnitude, then standard series display a set of

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[12] Campbell worked at the Cavendish Laboratory [Buchdahl, 1964] and, later, while on the research staff of the General Electric Company, was involved in the attempts to establish standards for the measurement of light intensity [Johnston, 2001].

[13] A similar distinction was drawn by Helmholtz [1887]. Campbell [1920] made few references to earlier works because his index of references was lost in World War I. Of his book, he noted that 'there is hardly a paragraph which is not a paraphrase of something that can be found in well-known treatises or papers' [vi] and he was deeply influenced by Helmholtz, amongst others (Buchdahl, 1964). It is therefore more likely than not that Campbell's distinction between fundamental and derived measurement came from Helmholtz, as Darrigol [2003] suggests.

[14] Uncharacteristic of most representational theorists, Campbell believed that these laws have the same kind of empirical status in relation to numbers as well.

nu, for $n = 1, 2, 3, \ldots$, etc., consecutively, up to whatever value of n is manageable. If an object is compared to a standard series in an appropriate way, a measure of the relevant attribute can be estimated.

Not all magnitudes, however, can be fundamentally measured, held Campbell. There is also derived measurement, which is achieved by discovering constants in laws relating attributes already measured. The discovery of such laws is also a result of scientific research and must be sustained by relevant evidence. An example is density. For each different substance, the ratio of mass to volume is a constant, different for different substances, say, for gold compared to silver. These constants are in the same order of magnitude as levels of density, when ordered by other methods. Thus, argued Campbell, they are measures of density. Because measurable, attributes like density are magnitudes, but they are not quantities (unless also fundamentally measurable). They are 'qualities' or 'B-magnitudes' [Campbell, 1920; 1928].

Campbell's claim that B-magnitudes are measurable is reasonable. The fact that the ratio of mass to volume is perfectly correlated with the kind of substance involved suggests that each different kind of substance possesses its own level of a general property accounting for this correlation. Furthermore, because the effect being accounted for (the constant ratio) is quantitative, the property accounting for it (viz., density) must likewise be quantitative, otherwise the complexity of the cause would not match the complexity of the effect and, so, the effect would not be fully accounted for. Campbell did not reason like this and he never accepted that B-magnitudes are quantitative in the same sense as A-magnitudes because he never realized that an attribute's internal structure is not logically tied to *how* we get to know that it is measurable.

The class of A and B-magnitudes exhausts the class of attributes measurable in physics and, so, thought Campbell, exhausts the scope of measurable attributes. While attributes that psychologists claimed to measure (like sensation intensities and, by then, intellectual abilities [Spearman, 1904]) did not rate a mention in his writings at this stage, Campbell's theory implied that psychological methods are not methods of scientific measurement.

3.3   Morris Cohen, Ernest Nagel and the Liberalisation of Campbell's Theory

This implication was, at first, somewhat muted, coming, as it did, to many psychologists via the popular textbook, *An Introduction to Logic and Scientific Method*[15], by Morris Cohen and Ernest Nagel[16] [1934]. Their chapter on measurement borrowed elements of Russell's and Campbell's theories. Neither were mentioned, but

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[15]This textbook presents a view of scientific method from the perspective of the kind of naturalistic realism popular in America before the deluge of logical positivism. This text is still in use today.

[16]Nagel had completed a dissertation on measurement in 1931. (He also had a direct influence upon methodological thinking in the social sciences, giving, for many years, a methodology seminar with Paul Lazarsfeld at Columbia University [Turner and Turner, 1990]).

Nagel [1932] had a deep knowledge of the relevant literature. An interesting feature was their conception that mathematics possesses empirical content. In an earlier chapter, they dealt with formal, mathematical systems and they saw numbers as elements of such a system. Such systems, said Cohen, 'apply to nature because they describe the invariant relations which are found in it' [1931, 204]. Numbers, they noted,

> have at least three distinct uses: (1) as tags, or identification marks; (2) as signs to indicate the *position* of the degree of a quality in a *series* of degrees; and (3) as signs indicating the quantitative relations between qualities. On some occasions numbers may fulfil all three functions at once. [Cohen and Nagel, 1934, 294].

Use (1) is not something that Russell or Campbell would have called measurement. Nor did Cohen and Nagel, but its presence in their chapter suggests uncertainty. Prior to the twentieth century, the central concept in discussions of the logic of measurement had been that of *quantity*. However, once the concept of representation became central, the concept of quantity slid from sight. The idea that measurement is exclusively of quantitative structures was abandoned and, later in the twentieth century, so was the idea that measurement must involve numbers, it being argued that representation by any set of 'abstract entities' or symbols is measurement (e.g., [Stevens, 1968; Heise, 2001]). Once representation is made pivotal, restrictions upon the field and range of the representing relation appear arbitrary, and the concept of measurement drifts inexorably to that of coding.

Use (2) is the case where numbers are assigned to an ordered series of objects or attributes so that a relation of *greater than* or *less than* is represented numerically. For this, Cohen and Nagel required that the proposed order relation be shown by observational methods to have certain properties that characterise the series of numbers, such as transitivity and asymmetry. Numerical assignments achieved in this way they called the measurement of 'intensive[17] qualities.' Effectively, inclusion of this category, meant that their understanding of measurement was at least as liberal as Russell's [1903].

Use (3) covered 'extensive qualities' (by which they meant Campbell's 'fundamental' measurement) and 'derived measurement.' In relation to extensive and derived measurement, the treatment given by Cohen and Nagel added little to Campbell's and they emphasised the point that the achievement of fundamental and derived measurement is always contingent upon scientific discoveries.

In each of these kinds of measurement, numbers are used to represent different relationships between the qualities involved and, so, they thought, this has implications for what can meaningfully be said about the qualities measured. For example,

> When we assert that one man has an I.Q. of 150 and another one of 75, all that we can mean is that in a *specific* scale of performance (requiring

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[17]The concept of *intensive magnitude* has a rich history from medieval times [Sylla, 1972] to the nineteenth century [Michell, 2003].

certain specialized abilities) one man stands "higher" than the other.
It is nonsense to say that the first man has twice the intelligence or
the training the other has, because no operation for adding intelligence
or training has been discovered which conforms to the ... conditions
necessary to make such a statement meaningful. [Cohen and Nagel,
1934, 298].

If the field of representation includes attributes in relation to which additive struc-
ture has not been demonstrated, then certain relationships that hold between
numbers, such as ratios or differences, may not represent features identified within
the field. For example, if numbers are used to represent no more than ordinal
structure, then the fact that one number is twice another may not represent one
quality's being twice as great as another. The identification of this problem by
Cohen and Nagel was recognition of the, so-called, 'problem of meaningfulness.'[18]
It is accorded a central place by some representational theorists (e.g., [Narens,
2002]).

The theory presented by Cohen and Nagel had important consequences for psy-
chology. If no more than ordinal structure is identified for the sorts of attributes
that psychologists aspire to measure, then it follows that psychological measure-
ment is neither fundamental nor derived measurement (i.e., it is not measurement,
as in physics). Cohen and Nagel did not labour this, but it did not take long for
critically minded psychologists to hammer the point home. H. M. Johnson [1936]
inferred that not many, if any, psychological attributes are really measurable. His
conclusion was not welcome. By the 1930s, the practices called 'psychological
measurement' occupied an important place in the USA and Britain, especially
attempts to measure intellectual abilities [Michell, 1999]. Psychologists had de-
voted considerable energy to constructing numerical assignment procedures (such
as intelligence tests), but very little to investigating the structure of the relevant
attributes. There was little evidence to support even the hypothesis that these
attributes are ordinal. Johnson prefigured a collision with the representational
theory of measurement.

3.4   The Collision between Psychological Measurement and Represen-
tationalism

This collision came when the British Association for the Advancement of Science
established a committee, containing Campbell as a member, to investigate psy-
chophysical measurement. As their reports indicate [Ferguson *et al.*, 1938; 1940],
they remained at loggerheads[19]. Campbell argued that psychologists needed to

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[18]They were not, of course, the first to notice it. Discussing the measurement of temperature
via its association with height in a thermometer, Russell said, 'the association is correct only as
to the more or less, not as to the how much: to say, for example, that one degree corresponds to
the same increase of temperature at any point of the scale, would be simply meaningless' [1896,
55]. See Michell [1986] for a discussion of this so-called problem.
[19]For a discussion of the interim and final reports, see Michell [1999].

advance their claim to be able to measure the intensities of sensations via either fundamental or derived measurement. Instead, he said, 'having found that individual sensations have an order, they assume that they are measurable' [Ferguson *et al.*, 1940, 347], but 'measurement is possible only in virtue of facts that have to be proved and not assumed' [Ferguson *et al.*, 1940, 342].

Most of the psychologists on the committee[20] considered Campbell's concept of measurement too narrow, but they proposed no effective alternative. This led one non-psychologist to retort that

> [m]easurement is not a term with some mysterious inherent meaning, part of which may be overlooked by physicists and may be in course of discovery by psychologists. It is merely a word conventionally employed to denote certain ideas. To use it to denote other ideas does not broaden its meaning but destroys it: we cease to know what is to be understood by the term when we encounter it; our pockets have been picked of a useful coin. [Ferguson *et al.*, 1940, 345].

This is not so. The last word is never said on any concept. Furthermore, many apparent certainties had recently collapsed with the acceptance of relativity and quantum theories in physics; the foundations of logic and mathematics were reeling from an earthquake (viz., Gödel's incompleteness theorem); and psychology experienced its own revolution, behaviourism, which threatened the certainties of mentalism. The times were such that redefinitions of fundamental concepts seemed to signify progress.

This collision led not to a reassessment of psychological measurement, but to a quest for less threatening concepts of measurement. From the mid-1930s and throughout the 1940s, psychologists experimented with revisions (e.g., [Bartlett, 1940; Bergmann and Spence, 1944; Brower, 1949; Cattell, 1944; Comrey, 1950; Cureton, 1946; Gulliksen, 1946; McGregor, 1935; Nafe, 1942; Reese, 1943; Smith, 1938; Stevens, 1946; Thomas, 1942]). However, the representational framework did not appear to have much flexibility beyond the interpretation already proposed by Cohen and Nagel. Within this framework, in any instance of measurement, the field is understood to possess a certain kind of structure (e.g., ordinal or additive) and it is this that is captured in the numerical representation. Representational theory could be expanded by exploring different sorts of possible structures. In principle, there is no end of these. However, even if more structural possibilities were identified, moving in that direction would throw the onus of proof on psychologists to produce evidence that they possessed structures of the relevant sort, thereby raising questions, not securing existing claims.

Russell, Campbell, and Cohen and Nagel interpreted the structure of the field in a *realist* way. That is, they thought of the field's structure as given by nature. Scientists might, through observation or experiment, discover the character of the field in specific instances, but they could not project a wished-for structure into the

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[20]C. S. Myers, H. Banister, F. C. Bartlett, R. J. Bartlett, W. Brown, S. Dawson (later replaced by K. J. W. Craik), J. Drever, S. J. F. Philpott, and R. H. Thouless.

field. The representational function, via which numerical assignments are made, is a *structure preserving function*. In the numerical mapping, structure already in the field prior to the mapping, is preserved. Proposing that the function is *structure generating* was not on.

3.5   S. Smith Stevens and the Operational Interpretation

Stevens solved this problem by relocating representational theory within the framework of operationism. He actually revised representational theory in three ways. First, he interpreted the range of the representing function, in the light of logical positivism, as devoid of empirical content. Second, he introduced his theory of scales of measurement. This clarified the mathematical character of different representing functions. Third, most radically, he interpreted the field of the measurement function operationally, construing the representing function, effectively, as structure generating.

When it came to revising the concept of measurement, Stevens was ahead of the game. His mentor at Harvard, E. G. Boring, long critical of psychological measurement [Boring, 1920; Newman, 1974], exposed Stevens to such a critical milieu that he wrote, 'my own central problem throughout the 1930s was measurement, because the quantification of the sensory attributes seemed impossible unless the nature of measurement could be properly understood' [1974, 436]. He had developed his own psychophysical procedures, which, he claimed, measured loudness on his *sone* scale [Stevens and Davis, 1938]. This scale had been discussed by Ferguson's Committee [Ferguson *et al.*, 1940] and when the final report appeared, Stevens was primed to respond.

Indeed, as a psychologist, he was uniquely, philosophically primed. He had been an early advocate of P. W. Bridgman's [1927] operationism in psychology [Stevens, 1935; 1936] and see [Hardcastle, 1995]). He belonged to a group that met regularly to discuss the philosophy of science, which included Bridgman, Rudolf Carnap, and the mathematician, G. D. Birkhoff [Holton, 1993]. The fifth International Congress for the Unity of Science was held at Harvard in 1939. According to W. V. O. Quine, who was the Congress's secretary, it was 'the Vienna Circle, with accretions, in international exile' [Holton, 1993, 31]. Stevens attended and presented an early version of his theory of measurement [Stevens, 1974].

Carnap's, *The Logical Syntax of Language* [1937], his 'most important work to date' [Stevens, 1939, 258] presented the view that logic and mathematics are systems of symbols, each with a syntax (i.e., rules for constructing formulas and deductions) consisting of conventions, rather than empirical truths. Any such system was taken to assert nothing about the world, but it was thought by Carnap to provide a framework within which the kinds of issues investigated in empirical science might be considered. Stevens accepted this doctrine, concluding that 'mathematics is a human invention, like language, or like chess, and men not only play the game, they also make the rules' [1951, 2]. This view entails what Michael Friedman [2001] has recently termed, a 'relativized *a priori*,' not unlike the rela-

tivism embodied in Thomas Kuhn's [1962] later concept of *paradigm*, which, had he known of it, Stevens would have happily agreed with, given his endorsement of a social criterion of truth [Stevens, 1936].

The idea that mathematics is a human invention runs up against the fact that successful applications of arithmetic, to mention just a small part of the corpus of mathematics, are ubiquitous. This suggests that its 'syntax' is far from arbitrary. Stevens argued that

> the rules for much of mathematics (but by no means all of it) have been deliberately rigged to make the game isomorphic with common worldly experience, so that ten beans put with ten other beans to make a pile is mirrored in the symbolics: $10 + 10 = 20$. [1951, 2].

However, if the rules of arithmetic are *isomorphic* with 'common worldly experience', then they are neither mere conventions nor lacking in empirical significance. Stevens's representationalism was no more compellingly motivated than Russell's.

However, the mood of the times was with Stevens. As many have noted (e.g., [Goodman, 1994]), since the Second World War, the 'received view,' as Narens [2002, 5] calls it, on the foundations of mathematics has been that arithmetic can be reduced to set theory and that set theory can be constructed on the concept of the empty set and some extension of the Zermelo-Fraenkel axiom system. From this point of view, the number system is simply a formal, axiomatic system, with its own 'syntax' and devoid of empirical content, and numbers, being sets, are thought of as 'abstract entities' [Quine, 1953, 114]. Stevens's view was that the range of the representational function was the system of numbers (which he thought of as a single system incorporating the real numbers, the rational numbers, and the integers), understood as a purely formal, axiomatically structured language.

While, in endorsing this kind of view, Stevens depended upon logical positivism, when it came to the representational function, he made a more independent contribution. It was implicit in Russell [1903] and Cohen and Nagel [1934] that not all representations are of the same kind. Stevens made this explicit, using insights of von Neumann and Morgenstern [1944] and G. D. Birkhoff. He distinguished four types of representing functions (or measurement scales), *nominal*, *ordinal*, *interval*, and *ratio*, distinguished on the basis of the invariance properties of the numerical assignments made. He believed that the type of scale involved in any instance of measurement could be determined by asking how the numerical assignments could be altered without altering the purpose of the scale. The numerical assignments made to the 'objects or events' measured are always arbitrary to some extent, but the degree of arbitrariness varies. It was Stevens's view that in any case where scale values are altered, the purpose of making numerical assignments on a *ratio scale* is unaltered (or invariant) only if all scale values are multiplied by a (positive) constant (a positive similarity transformation); the purpose of making numerical assignments on an *interval scale* is unaltered only if all scale values are multiplied by a (positive) constant and a (positive or negative) constant is added (a positive linear transformation); the purpose of making numerical assignments on

an *ordinal scale* is unaltered if all scale values are altered by an order-preserving function (an increasing monotonic transformation); and the purpose of making numerical assignments on a *nominal scale* is unaltered if all scale values are altered by a one-to-one substitution (a one-to-one transformation). This was an important contribution to the theory of numerical representations, one preserved and investigated in subsequent versions of representational theory (e.g., [Narens, 1981]).

Stevens's interpretation was most radical in its understanding of the field of the representing function. According to representational theory, it is not just objects and events that are numerically represented in measurement, it is also relationships between them. Stevens [1951, 25] referred to these relationships as 'basic empirical operations' and they included what he called the 'determination of equality' (for all scales), the 'determination of greater or less' (for all scales other than the nominal), the 'determination of equality of intervals or differences' (for interval and ratio scales), and the 'determination of equality of ratios' (for ratio scales). It is the determination of these basic empirical operations that is the source of the invariance properties defining the type of scale involved.

However, there is an ambiguity about 'determination': is it a matter of ascertaining something that is already the case (the realist interpretation) or of instituting something not hitherto present (the operationist interpretation)? If the latter, then the investigator's act of making numerical assignments constitutes, at least in part, the 'empirical' structure that the numerical assignments represent. In this sense, the representing function is taken to be a *structure generating function*. If the representing function is so construed, then according to Stevens's theory, a ratio scale, for example, may 'represent' an 'equality of ratios' in the objects that is nothing more than a convention imputed by the investigator. It was Stevens's view that 'the most liberal and useful definition of measurement is the assignment of numerals to things so as to represent facts and conventions about them' [1951, 29], allowing that sometimes that which determines the type of scale used may not be an independently existing feature of the structure represented, but a 'convention' about it. In other words, relations of equality, order, and equality of intervals or ratios may be, in part, determined by convention, not nature.

While aligned with logical positivism in spirit, Bridgman's [1927] operationism differed with respect to specific doctrines. Stevens followed Carnap's views on mathematics and rejected Bridgman's[21]. However, he sided with Bridgman in holding that for a class of things instantiating a concept, the meaning of the concept 'is defined by the operations which determine inclusion within the class' [Stevens, 1939, 234]. This meant that he could argue that operations used to 'determine equal ratios' define equal ratios. The procedures used to construct his sone scale for the measurement of loudness [1936b], asked subjects to judge loudness ratios directly and Stevens took the resulting scale 'at its face value' [407] as a ratio scale.

He admitted that if postulating a ratio scale in such circumstances 'is thievery,

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[21]See Bridgman [1936].

it is certainly no petty larceny' [1951, 41][22], but he never faced up to the issue of testing whether loudness intensities stand in relations of ratio independently of operations employed by investigators. He remained convinced, not only that 'the numbers that issue from measurements have strings attached, for they carry the imprint of the operations by which they were obtained' [1968, 856], but also that 'the man in the laboratory, the maker of measurements, must decide the meaning of numbers and their capacity to advance empirical inquiry' [1968, 851].

Stevens's operational interpretation of representational theory ties his theory of scales of measurement to his definition of measurement. Without it, they do not mesh well. His definition is wider than his theory of scales requires, if the latter is understood from a realist perspective. In this case, a better definition would have been something like 'measurement is the assignment of numerals to objects or events so that independently existing empirical relationships between the objects or events are represented by numerical relationships.' From this viewpoint, even a nominal scale requires evidence that a putative empirical equivalence relation between objects and events is actually reflexive, symmetric and transitive before numerical assignments are made and a nominal scale established. On the other hand, from Stevens's viewpoint, if numerals are assigned to objects or events, then a nominal scale, at least, is always operationally defined, in the sense that objects or events can be counted 'equivalent' if assigned the same numeral. Thus, given this interpretation, the assignment of numerals to objects or events according to rule, 'provided a consistent rule is followed' [Stevens, 1959, 19], always produces measurement.

Stevens's laxity about what is represented in measurement delivered exactly the sort of conceptual plasticity that psychologists needed to claim measurement without having to interrogate their established methods in the way that realist interpretations of representational theory require. His definition was made to measure. From the mid-1950s, the mainstream of psychologists and social scientists disengaged from the theory of measurement. In the decade immediately prior to 1951 (i.e., the period 1940-1950), eleven papers analysing the concept of measurement were published in mainstream, American psychology journals, but in the decade immediately after (i.e., period 1952-1962), only one [Coombs et al., 1954]. As already noted, this paper regarded Stevens's treatment as 'classical'. From 1951[23] onwards, psychologists had Stevens's definition of measurement and his theory of scales with its ambiguities about the field of the representing function.

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[22]In his reference to thievery, Stevens was alluding to Russell's maxim that the 'method of "postulating" what we want has many advantages; they are the same as the advantages of theft over honest toil' [1919, 71; see Stevens, 1951, 14; 1958, 386]. Stevens's behaviour was more in line with Bridgman's maxim that the 'scientific method, as far as it is a method, is doing one's damnedest with one's mind, no holds barred' [1950, 535].

[23]Stevens was a member of the 'Psychological Round Table' (see Benjamin [1977] and Hardcastle [2000]), a highly selected group of young American experimental psychologists who met annually to discuss new ideas, from its foundation in 1936 to 1946. Many of its members became important opinion leaders in American academic psychology in the generation after the Second World War. This, no doubt, contributed to the seamless acceptance of Stevens's views on measurement within psychology.

Accepting Stevens's definition allowed them to claim that they were measuring and the ambiguities about how structure in the field of the representing relation is defined allowed them to construct a case for at least interval scale measurement whenever this was deemed convenient [Michell, 2002]. Despite the fact that for the overwhelming majority of procedures for making numerical assignments in psychology, it has never been demonstrated that the attributes to which the numbers are assigned possess even ordinal structure, let alone anything stronger, the following are typical of received 'wisdom':

> ... the probability is high that many scales and tests used in psychological and educational measurement approximate interval measurement [Kerlinger and Lee, 2000, 635];

> ... the vast majority of psychological tests measuring intelligence, ability, personality and motivation ... are interval scales [Kline, 2000, 18];

> ... interval measurement is probably the most common scale in psychology [Lehman, 1991, 54]; and

> ... most measures of psychological states and traits and of constructs such as attitudes and people's interpretations of events are interval level [Whitley, 1996, 117].

This 'wisdom' contradicts the view that in science, evidence matters. If the only defence against Campbell's criticism is that of accepting a vacuous definition of measurement and allowing scale structure to be operationally defined via numerical assignment procedures, then whatever the popularity of this manoeuvre amongst the scientists involved, logically speaking, its adoption is tacit admission of the validity of the original criticism.

3.6 *Patrick Suppes, R. Duncan Luce and the Axiomatic Approach*

Mainstream psychology's disengagement with measurement theory was premature. It left the arena before the main event. In the same year as Stevens's influential paper appeared, so did another on measurement [Suppes, 1951]. It was the first in a research program that revolutionised the representational theory of measurement and gave it an unassailable hegemony within the philosophy of science for the remainder of the century. Suppes later collaborated with R. Duncan Luce, David Krantz and Amos Tversky. Louis Narens [1985; 2002] also made fundamental contributions. Results are published in the *Foundations of Measurement* [Krantz *et al.*, 1971; Suppes *et al.*, 1989; Luce *et al.*, 1990].

Suppes' paper possessed a number of features that became hallmarks. First, it was an axiomatic approach to measurement theory. Suppes proposed a set of seven conditions and proved that any system satisfying these is isomorphic to a subsystem of the positive real numbers (using a theorem of G. D. Birkhoff [1948],

widely known as *Hölder's theorem*).[24]

Second, Suppes used set theory as a mathematical framework for developing representational theory. As noted, it was popular at this time to characterise the numbers as sets and the various number systems (say, the system of real numbers) as set-theoretical systems involving relations or operations.[25,26] This characterised the range of the representational function as a set-theoretical, relational structure. Suppes also characterised the field of the representational function as a set-theoretical, relational structure. For example, the field might be a relational system consisting of a set of objects possessing various lengths (say, a set of rigid, straight rods), a transitive[27], strongly connected[28] relation that weakly orders[29] the objects by length, and a concatenation operation that combines objects linearly, end to end to form new objects and, which, with respect to length, mimics some of the formal properties of numerical addition. The relational system is characterised by specifying a set of *qualitative*[30] axioms or conditions that the system is hypothesised to satisfy. The problem of proving for any case of measurement that a numerical representation exists then reduced to that of proving that one set-theoretical relational structure can be mapped to another by a one-to-one (*isomorphic*) or a many-to-one (*homomorphic*) function. Later (e.g., [Suppes and Zinnes, 1963]), Suppes designated such a proof, a *representation theorem*. Furthermore, it could be proved whether the numerical representation achieved in any instance admitted transformations of the numbers assigned that would preserve the structure of the representation and, if so, what the class of such admissible transformations is. Suppes called a proof of this kind a *uniqueness theorem* [Suppes and Zinnes, 1963]. The capacity to prove uniqueness theorems meant that Stevens's classification of nominal, ordinal, interval and ratio scales was given a firm mathematical basis and the way in which these different types of scales depended upon the structure of the field was clearly displayed.

Third, it was deemed to be important that the empirical relational system and its associated axioms be specified in terms of empirically identifiable objects, operations and relations and, as far as possible, directly testable conditions. The axioms, if true, were regarded as empirical laws [Krantz *wet al.* 1971]. This was in the spirit of Campbell's theory, but the fact that Suppes' approach did

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[24]This theorem is that an Archimedean ordered group is isomorphic to a subgroup of the positive real numbers. The proposition is first claimed in a footnote in Hölder [1901].

[25]Within set theory, an *n*-termed operation can always be redefined as an $(n + 1)$-termed relation, so talk of operations is said to be just another way of talking about relations.

[26]Suppes [1960] was later to display this approach in his book, *Axiomatic Set Theory*.

[27]Taking xRy to mean x stands in relation R to y, transitivity may be defined as follows: a binary relation, R, is *transitive* upon a set if and only if for every x, y, and z in the set, if xRy and yRz, then xRz.

[28]A binary relation, R, is *strongly connected* upon a set if and only if for every x and y in the set, either xRy or yRx.

[29]A binary relation upon a set is a *weak order* if and only if it is transitive and strongly connected upon the set.

[30]By *qualitative*, in this context, is meant non-numerical. The idea is that this relational system is empirical, and 'abstract entities', such as numbers and numerical relations, are not part of it.

not necessarily require additive operations or relations meant that it was liberal enough to include ordinal and nominal scales [Suppes and Zinnes, 1963]. However, unlike Stevens's, Suppes' approach was realist in the sense that for measurement to be attained, the structure that the empirical relational system was hypothesised to possess had to be investigated and confirmed prior to measurement being claimed.

To establish the plausibility of this approach, it was important to show that it covered physical measurement. This was straightforward for extensive magnitude [Suppes, 1951]. Suppes and Zinnes [1963, 42] stated axioms for extensive measurement[31] (in which A is a non-empty set, with a, b, and c being any elements, $\leq$ is a weak order on A, where $a \leq b$ means that a is not greater (in the relevant sense) than b, and $*$ is a binary, empirical concatenation operation on A):

An extensive system $\langle A, \leq, * \rangle$[32] is a relational system consisting of the binary relation $\leq$, the binary operation $*$ from $A \times A$[33] to A, and satisfying the following six axioms for a, b, c in A.

1. If $a \leq b$ and $b \leq c$, then $a \leq c$;

2. $(a * b) * c \leq a * (b * c)$;

3. If $a \leq b$, then $a * c \leq c * b$;

4. If not $a \leq b$, then there is a c in A such that $a \leq b * c$ and $b * c \leq a$.

5. Not $a * b \leq a$;

6. If $a \leq b$, then there is a number n such that $b \leq na$ where the notation na is defined recursively as follows: $1a = {}^{34}a$ and $na = (n-1)a * a$.

These axioms can be variously interpreted, depending upon the attribute involved. For example, with length, $a \leq b$ means that rod a is not longer than rod b and $a * b$ signifies the rod formed by connecting rods a and b end to end linearly. Whether $a \leq b$ holds for a given pair can be tested by placing the rods side by side, and axioms involving the concatenation operation, $*$, can be tested by constructing the rods signified. Axiom 1 is that the order relation, $\leq$, is transitive. Axiom 2 is that $*$ is associative. Axiom 3 is a combined monotonicity (if two rods are extended by rods equal in length, any equality or inequality is preserved) and commutativity (order of extension is unimportant) condition. Axiom 4 is that any difference between rods can be compensated for by concatenating some other rod with the shorter. Axiom 5 is the requirement that all lengths are positive. And

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[31]Similar to those in Suppes [1951].

[32]Brackets of the kind, $\langle \rangle$, are used to indicate an ordered set.

[33]A set, $A \times B$, is the product of A with B and consists of the set of all ordered pairs, $\langle a, b \rangle$, obtained if each element, a, of A is paired with each element, b, of B. $A \times A$ is the product of A with itself.

[34]The relation involved here is not numerical equality, but a relation of identity with respect to the relevant attribute.

axiom 6 is an Archimedean condition: any rod, no matter how short, extended by some finite number of replicas will exceed any other rod, no matter how long.

Suppes and Zinnes [1963] proved that a homomorphic mapping from any extensive system, $\langle A, \leq, * \rangle$ into a subsystem of the positive real numbers, $\langle N, \leq, + \rangle$ exists, where N is a subset of positive real numbers, and $\leq$ and $+$ have the usual interpretation on the reals. Furthermore, they proved that any two such mappings are related by a similarity transformation (i.e., by multiplication by a positive constant) and, so, any such mapping is a ratio scale. So this approach accommodates 'fundamentally measurable' magnitudes.

However, what about Campbell's category of 'derived measurement'? This problem was solved by the development of the theory of *conjoint measurement*. This was a term introduced by Luce and Tukey [1964].[35] A more complete exposition is given in Krantz *et al.* [1971]. This theory specifies conditions necessary and/or sufficient for a weak order upon a product set to be additively or multiplicatively representable. A weak order, $\leq$, upon a product set, $A \times X$, (for non-empty sets A and X) is *multiplicatively representable* if and only if there exist homomorphic functions, f and g, from A and X respectively, into the positive real numbers such that for any a and b in A and x and y in X

$$\langle a, x \rangle \leq \langle b, y \rangle \text{ if and only if } f(a).g(x) \leq f(b).g(y)$$

(where $f(a)$ is the real number into which the function f maps a, etc.). That is, the fact that the $\langle a, x \rangle$-pair does not exceed the $\langle b, y \rangle$-pair is represented by the numerical fact that the *product* of the numbers assigned to a and x does not exceed the *product* of the numbers assigned to b and y. If the weak order upon the product set is multiplicatively representable, then the functions f and g must be ratio scales, otherwise the products would not consistently reflect the order upon the elements of the product set. Also, if a product set is multiplicatively representable, then it is additively representable because for any a and b in A and x and y in X,

$$f(a).g(x) \leq f(b).g(y) \quad \text{if and only if} \quad log_n[f(a)] + log_n[g(x)] \leq log_n[f(b)] + log_n[g(y)]$$
$$\text{if and only if} \quad F(a) + G(x) \leq F(b) + G(y)$$

(where $F(a) = log_n[f(a)]$, etc. and $n > 1$). The order relations on the pairs is represented via the relevant *sums* of the numbers assigned to the individual elements. In the case of additive representations, the functions, F and G, are interval scales (although with a common unit).

The core of the theory states conditions, which if satisfied by $\leq$ ensure the existence of a multiplicative or additive representation. Krantz *et al.* [1971, 257] proved that a weak order upon a product set, $A \times X$, (for non-empty sets A and X) is *multiplicatively* (or *additively*) *representable* if it satisfies three conditions, *double cancellation*, *solvability*, and the *Archimedean condition*, defined as follows.

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[35]For an indication of the extent to which the work of Luce and Tukey builds on that of others, see the historical note in Krantz *et al.* [1971, 259-261].

1. *A weak order, $\leq$, upon a product set, $A \times X$, satisfies* double cancellation *if and only if for any a, b, and c in A and x, y, and z in X, if $\langle a, y\rangle \leq \langle b, x\rangle$ and $\langle b, z\rangle \leq \langle c, y\rangle$, then $\langle a, z\rangle \leq \langle c, x\rangle$.*

The necessity behind the double cancellation condition may be illustrated via the additive representation. If an additive representation exists, then

$\langle a, y\rangle \leq \langle b, x\rangle$ implies that $F(a) + G(y) \leq F(b) + G(x) \equiv F(a) - F(b) \leq G(x) - G(y)$ and $\langle b, z\rangle \leq \langle c, y\rangle$ implies that $F(b) + G(z) \leq F(c) + G(y) \equiv F(b) - F(c) \leq G(y) - G(z)$ and

summing these inequalities and cancelling common terms leaves

$F(a) - F(c) \leq G(x) - G(z) \equiv F(a) + G(z) \leq F(c) + G(x)$, which implies that $\langle a, z\rangle \leq \langle c, x\rangle$.

That is, the double cancellation condition is a generalisation of the Euclidean condition that equals plus equals gives equals. In this more generalised form, it means that unequals in a particular direction plus unequals in the same direction gives unequals in that direction. However, when the additive representation is expressed as differences, it is evident that it also means that differences between elements within each set, A and X, are additive. So it can be interpreted as an additivity requirement, as well.

2. *A weak order, $\leq$, upon a product set, $A \times X$, satisfies* solvability *if and only if given any three of a and b in A and x and y in X, the fourth exists such that $\langle a, x\rangle \leq \langle b, y\rangle$ and $\langle b, y\rangle \leq \langle a, x\rangle$.*

Again, the meaning of this condition is clarified by considering it via the additive representation.

If both $\langle a, x\rangle \leq \langle b, y\rangle$ and $\langle b, y\rangle \leq \langle a, x\rangle$,
then $F(a) + G(x) \leq F(b) + G(y)$ and $F(b) + G(y) \leq F(a) + G(x)$,

which implies that

$$F(a) + G(x) = F(b) + G(y) \equiv F(a) - F(b) = G(y) - G(x).$$

Expressing the additive representation as differences, it is evident that the solvability condition can be interpreted as meaning that for any difference between elements of A, equivalent differences exist between elements of X, relative to each element of X; and, similarly, for any difference between elements of X, equivalent differences exist between elements of A, relative to each element of A. That is, the order of elements of both A and X is either equally spaced (as is the order of the natural numbers) or is dense[36] (as is the order of the rational numbers).

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[36] An order upon a set, S, is dense if and only if between each pair of elements of S there lies another.

The solvability condition means that relative to the difference between any arbitrary elements, x and y, of X, and relative to each element, a_1, of A, there will be a *standard series*, $a_1, a_2, a_3, \ldots, a_n$ (for any natural number n) with step-size equal to the difference between x and y. Considering then, the difference between any pair of elements, x and y, of X, no matter how small, and the difference between any pair of elements, a_1 and b, of A, no matter how large, the Archimedean condition requires that there exists, for some finite integer, n, a standard series, $a_1, a_2, a_3, \ldots, a_n$ from a_1 to b (i.e., $a_n = b$) or to just beyond b (i.e., a_{n-1} comes before b and a_n comes after b). Because in such circumstances, it is bounded by a_1 and b, the series, $a_1, a_2, a_3 \ldots, a_n$, is called a *strictly bounded standard series*. Similarly, the Archimedean condition requires the existence of a strictly bounded standard series of elements of X between each pair of elements of X, of a step-size equal to any difference between elements of A.

Put as succinctly as possible, *the Archimedean condition is that every strictly bounded standard series of elements of A and of X is finite.*

That is, the Archimedean condition does not allow for any differences between elements within A or within X to be either infinitely large or infinitesimally small relative to any other differences.

The theory of conjoint measurement accommodates cases of derived measurement. Consider, for example, the derived measurement of density via the ratio of mass to volume. This relationship can also be expressed multiplicatively as the mass of a body is the product of its volume and its density. If A is interpreted as volume, X as density, and $A \times X$ as mass, then the theory of conjoint measurement specifies conditions upon an ordering of masses (viz., double cancellation, solvability, and the Archimedean condition) sufficient for it to be represented as the product of volume and density.

To apply the theory of conjoint measurement in this context, one simply needs to be able to (1) order the masses of objects (say, via observing the movement of the arms of a beam balance), (2) to detect whether any two volumes are the same or different (say, via the heights of columns of displaced liquid in standard jars following immersion of the relevant bodies), and (3) to detect whether two densities are the same or different, which is relatively easy given the assumption that density is correlated with kind of substance. Measurement of all three attributes (mass, volume, and density) is then achieved simultaneously as the multiplicative representation of the order on the masses relative to the classifications of volume and density. The manner in which the measurement of density may be interpreted as a case of numerical representation is then clearly displayed. This is something that Campbell left obscure. Thus, this version of the representational theory provides a more uniform treatment of physical measurement than any before it. Not only fundamental measurement, but also derived measurement, are presented as cases of numerical representation, in which the empirical structure represented is clearly displayed.

The fact that the double cancellation condition is directly testable means that the relationship between density, mass and volume is an empirical law and not

a definition. Rudolf Carnap [1966], for example, believed that the derived measurement of density resulted from *defining* density as the ratio of mass to volume. However, if the double cancellation condition did not hold, then density would not be this ratio. For example, double cancellation implies that if the mass of some volume (call it v_1) of iron exceeds the mass of a different volume (v_2) of aluminium, and if the mass of v_2 of gold exceeds the mass of another volume (v_3) of iron, then v_1 of gold must exceed v_3 of aluminium. However, there is no logical necessity that this prediction should hold. The theory of conjoint measurement displays the fact that the measurement of density is as much based on empirical laws as the measurement of extensive quantities, as Campbell taught.

In the above discussion, density is simply a typical example of Campbell's category of derived measurement. Because all measurable, physical attributes form a tight-knit algebraic structure, in which all quantities are simple monomial functions of the six extensive quantities, electrical charge, temperature, mass, length, time, and plane angle [Krantz *et al.*, 1971], all derived magnitudes stand in empirical, nomological relationships to extensive quantities. Thus, the version of the representational theory advanced by Suppes, Luce, and their associates provides an account of physical measurement superior to other versions.

However, this was peripheral to its main point. While Suppes was a philosopher[37] and Luce was trained in engineering, they both became interested in the measurement of utility and subjective probability at the interface of psychology and economics[38]. The primary concern of this group, therefore, was in providing a framework for psychological measurement. Conjoint measurement seemed applicable to psychology because its use depended only on being able to identify orders and classifications and did not require the direct identification of concatenation operations whose form mimics numerical addition. In this vein, the three volumes of the *Foundations of Measurement* [Krantz *et al.*, 1971; Suppes *et al.*, 1990; Luce *et al.*, 1990] and other publications falling into this program (many of which are published in the *Journal of Mathematical Psychology*), explore, often in a purely theoretical way, a rich array of set-theoretical structures able to sustain ratio or interval scale representations. Here is not the place to review the full sweep of this contribution to measurement theory. The interested reader is referred to Luce [1988], Narens and Luce [1986], Suck [2001] and Luce and Suppes [2002] for useful introductions and summaries.

As Norman Cliff [1992] noted, this approach to representational measurement theory has had little impact upon mainstream psychological measurement (i.e., upon psychometrics as it relates to attempts to measure intellectual abilities, personality traits, and social attitudes).[39] This approach, had psychometricians ac-

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[37]Actually, Suppes first degree was in meteorology and physics, but his PhD was in philosophy [Suppes, 1979a].

[38]See Luce [2000] for a detailed exposition of Luce's more recent research in this field.

[39]Narens & Luce [1993] responded to Cliff's comment by indicating areas where axiomatic measurement theory has had some impact. Cliff's comment is true if restricted to psychometrics, which, I suspect, was his intention. He had been president of the Psychometric Society and editor of *Psychometrika*.

cepted it, would have required that experimental research confirm the existence of suitable empirical relational structures before claiming measurement. Via this route, psychometricians stood to discover that 'not everything that one would like to measure is measurable' [Coombs, 1983, 39]. From the perspective of those wanting to employ the rhetoric of measurement, this would have been to pit a sure thing (Stevens's theory) against a risky option (the approach of Suppes and Luce) and, so, it was no contest.[40]

To the extent that psychometricians engaged with this approach, they laboured the point that many of the axiom systems considered, such as that for conjoint measurement, are 'deterministic models' (e.g., [Borsboom and Mellenbergh, 2004, 107]) and, therefore, it is alleged, ill-matched to psychological data [Cliff, 1992]. In so far as scientific theories purport to describe the structure of natural systems (usually causal structure, but in the case of measurement, non-causal structure[41]) and data against which scientists evaluate theories are fallible, such evaluations are fraught with difficulty. Psychologists 'mechanised' [Gigerenzer *et al.*, 1989] evaluations using statistical methods and seem ignorant of the fact that there are always many ways to approach any problem. The various axiom systems proposed for measurement are theories about the structure of attributes, no different in principle to any theories in science. Given the imperfections of data, it is never required that theories predict data perfectly, only that it be reasonable to interpret them as true in the light of available evidence.

Data are 'imperfect' in other ways, as well, for even were they uncontaminated by error, they would still be finite, whereas scientific theories are generally infinite in scope. Suppes (e.g., [Suppes, 1979b, 213-214]) finds this fact uncomfortable. However, representational theorists need not be uncomfortable about this,[42] for if anything is numerically represented in measurement, it is not *data* structures, but *theoretical* structures [Luce, 1979]. Furthermore, like any structure theorised about, its connection to data need not be direct. Many theories are only indirectly testable. Brent Mundy [1994] criticised Suppes on this point, arguing that what is numerically represented in measurement is 'not necessarily a directly implementable *empirical* process' [64]. If an attribute such as length is considered, then it is the attribute itself, as it is in the objects, that is presumed to be numerically representable, if anything is, and not observations of some finite set of lengths. What is required is a characterisation of the structure of length, as conceptualised within physical theories.

Such a characterisation is what mathematicians of an earlier era, such as Hölder

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[40]A small number of psychometricians have considered the approach of Suppes and Luce over the past forty years (e.g., [Keats, 1967; Perline, Wright and Wainer, 1979; Andrich, 1988; Cliff, 1993; Scheiblechner, 1999]), but the impact upon empirical research, data analysis, and mainstream practices has been negligible. (See Michell [2000], Borsboom and Mellenbergh [2004], and Michell [2004a]).

[41]What John Stuart Mill [1843] called 'uniformities of coexistence.'

[42]Discomfort on this point is not so evident in the later writings within this program (e.g., [Luce *et al.*, 1990]).

[1901][43], may be understood as offering. If the attribute of length is symbolised as Q, specific magnitudes of Q are designated by $a, b, c, \ldots$ etc., and if for any three magnitudes, a, b, and c, of Q, $a + b = c$ if and only if c is entirely composed of discrete parts a and b, then using Hölder's axioms of quantity, the structure of length is characterised by the following seven conditions:

1. Given any magnitudes, a and b, of Q, one and only one of the following is true:

    (a) a is identical to b (i.e., $a = b$ and $b = a$);

    (b) a is greater than b and b is less than a (i.e., $a > b$ & $b < a$); or

    (c) b is greater than a and a is less than b (i.e., $b > a$ & $a < b$).

2. For every magnitude, a, of Q, there exists a b in Q such that $b < a$.

3. For every pair of magnitudes, a and b, in Q, there exists a magnitude, c, in Q such that $a + b = c$.

4. For every pair of magnitudes, a and b, in Q, $a + b > a$ and $a + b > b$.

5. For every pair of magnitudes, a and b, in Q, if $a < b$, then there exists magnitudes, c and d, in Q such that $a + c = b$ and $d + a = b$.

6. For every triple of magnitudes, a, b, and c, in Q, $(a + b) + c = a + (b + c)$.

7. For every pair of classes, ϕ and ψ, of magnitudes of Q, such that

    (a) each magnitude of Q belongs to one and only one of ϕ and ψ;

    (b) neither ϕ nor ψ is empty; and

    (c) every magnitude in ϕ is less than each magnitude in ψ,

    there exists a magnitude x in Q such that for every other magnitude, x', in Q, if $x' < x$, then $x' \in \phi$ and if $x' > x$, then $x' \in \psi$ (depending on the particular case, x may belong to either class).[44]

Axiom 1 is that any two lengths are either the same or different and if different, one is less than the other; axiom 2, that there is no least length; 3, that the additive composition of any two lengths exists; 4, that all lengths are positive; 5, that the difference between any pair of lengths can be made-up by another; 6, that the additive composition of lengths is associative; and 7, that the order of lengths forms a continuous series (i.e., any set of lengths having an upper bound (i.e., a length not less than any in the set) has a least upper bound (i.e., a length not greater than any of the upper bounds)). These axioms define what it is for length to be an *unbounded continuous quantity*.

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[43]For a translation of Hölder [1901] see Michell and Ernst [1996; 1997].

[44]The statement of these seven conditions is adapted from Hölder [1901] (see Michell and Ernst [1996, 238]).

If the concatenation operation mentioned earlier is taken to provide information about the additive relation mentioned in these conditions, then some of these conditions could be tested directly by observing the outcomes of various operations upon a range of humanly manageable lengths (say, the lengths of a set of rigid, straight rods between two centimetres and two meters in size). Such tests could be interpreted as providing support for these conditions, but for the vast range of possible lengths, direct tests would not be practicable[45]. These conditions are a theory about the structure of length and they are not meant to describe the behaviour of rods or any other objects having length. Of course, because length is our paradigm of continuous quantity, this theory does not apply only to length. A theory of the same form is intended to apply to every attribute that is hypothesised to be an unbounded continuous quantity (i.e., every attribute that scientists think of by analogy with the real number line).

Hölder [1901] proved that every magnitude of an unbounded, continuous quantity is measurable relative to any magnitude of the same quantity taken as the unit. The idea is as follows. For any magnitude, a, in Q, there will be the series of magnitudes, $a, 2a, 3a, \ldots, na$, for any natural number n. The measure of any magnitude, a, of Q relative to another magnitude, b, of Q (this measure is *the ratio*[46] of a to b, i.e., $a : b$) exceeds the numerical ratio of m/n if and only if $na > mb$, otherwise $na \leq mb$. In the former case, Hölder calls m/n a *lower fraction* in relation to $a : b$, and in the latter, an *upper fraction*. Since the union of the sets of upper and lower fractions for any ratio of magnitudes is the set of all rational numbers, the complete set of lower fractions (or upper fractions) defines a unique cut in the ordered series of rational numbers and, therefore, identifies a unique positive real number, as these were defined by Richard Dedekind [1872]. That is, $a : b$ is the least upper bound of the set of lower fractions in relation to $a : b$. Furthermore, given any positive real number, r, and any arbitrary unit, b, there exists a unique magnitude, a, such that $a : b = r$. Given that any two pairs of magnitudes, a and b, and c and d, are in the same ratio if and only if $a : b = c : d$ (i.e., their ratios are identical when their respective sets of upper and lower fractions are identical), it follows that the set of ratios of magnitudes of an unbounded continuous quantity is isomorphic to the positive real numbers. That is, these two sets have identical structures. If the system of real numbers is thought to be defined by its structure (i.e., any system isomorphic to the real numbers *is* the real numbers [Waismann, 1951]), it follows that ratios of magnitudes of any unbounded continuous quantity instantiate the real number system [Michell, 1994].

That is, interpreting axiomatic, representational measurement theory as about attributes of quantitative physics, leads beyond representational theory to the ratio theory of number. The systems of magnitudes that unbounded, continuous attributes are hypothesised to be instantiate the positive real numbers. Measure-

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[45]Some axioms, such as 7, are not directly testable, which is not to say that they are not testable (e.g., see Forrest [1995]).

[46]The concept of a ratio of magnitudes of continuous quantities is more complex than indicated here. See Michell [1993b; 1994].

ment is then an attempt to *identify the real number that is the relationship between the magnitude measured and the unit employed.* The natural science definition of measurement is thereby entailed. Axiomatic measurement theory has metamorphosed into a completely[47] realist theory of measurement. Measurement is *not* numerical representation; it is numerical discovery.

The idea that empirical relational systems instantiate mathematical structure was always implicit in representational theory because of its premise that empirical and mathematical systems share similar structures. As noted, Cohen saw this and so did Nagel, writing that 'if mathematics is applicable to the natural world, the formal properties of the symbolic operations of mathematics must also be predicable of many segments of that world' [1932, 314]. Narens and Luce agreed: '*In many empirical situations considered in science . . . there is a good deal of mathematical structure* already present' [1990, 133, italics in original]. Before representational theory was launched, Hölder [1901] proved that the positive real numbers are instantiated in ratios of magnitudes of unbounded, continuous quantities, so it was inevitable that once twentieth-century scruples about ascribing this kind of structure to measurable attributes[48], and understanding numbers as empirical relations[49], were overcome, it would become a realist theory of measurement. Clinging to numbers as 'abstract entities,' existing outside of space and time, after their empirical location is identified is like continuing to believe that some event is a miracle after a naturalistic explanation is known.

What then, of the theoretical research carried out under the auspices of the axiomatic approach to the representational theory? If representational theory is rejected, was there any point to establishing that this or that kind of empirical relational system possessed a numerical representation? Under the hegemony of representational theory, the hitherto central concept of quantity was eclipsed. It is not indexed in the *Foundations of Measurement* [Krantz, *et al.*, 1971; Suppes, *et al.*, 1989; Luce, *et al.*, 1990]. It is ironic, then, that it is only possible to understand the twentieth-century development of representational theory, by considering the historical trajectory of the concept of quantity.

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[47]It is *completely* realist in the sense that both the attribute theorised about (e.g., length) and the numbers are taken to be immanent, natural structures.

[48]Both operationism and positivism contributed to the twentieth-century view that in science, theory should stray as little as possible beyond hand or eye, as if the structure of reality is somehow restricted by its perceptual relations to humans.

[49]Given our cognitive dependence upon the concept of number and the truths of arithmetic in almost every intellectual enterprise that deals with real things, it is surprising that the twentieth-century idea that the numbers are 'abstract entities' outside of space and time, was taken seriously [Michell, 1995]. The earlier view, that numbers are ratios of quantities, has been revived by philosophers such as Armstrong [1997], Bigelow [1988], Bostock [1979], and Forrest and Armstrong [1987]). It is part of the more comprehensive view that mathematics is the general science of structure (e.g., [Parsons, 1990]), of both natural structures and those that are mere inventions.

4   THE TRAJECTORY OF THE CONCEPT OF QUANTITY

As quantitative science developed from antiquity to the present, the concept of quantity was gradually better understood. Ancient Pythagoreanism, with its doctrine that 'all things are numbers' [Burnet, 1915, 52], where by *numbers* was meant *whole numbers*, testifies to a time when *count* and *measure* were thought to mean the same. The proof that the lengths of the side and diagonal of a square are incommensurable (i.e., that the ratio of these two lengths does not equal a ratio between any pair of whole numbers)[50] implies that geometric magnitudes are not structurally identical to multitudes and this, in turn, raised a question about the possibility of expressing the relationship between any given length and any arbitrary unit as a ratio of whole numbers (i.e., about the possibility of measuring magnitudes). Euclid solved this problem by defining[51] sameness of ratio between magnitudes in a way that anticipated Dedekind's [1872][52] definition of the real numbers as cuts in the series of rational numbers (see [Bostock, 1979] and [Stein, 1990]). Letting a, b, c, and d, be any magnitudes of the same quantity (say, length), Euclid's definition is that $a{:}b = c{:}d$ if and only if for all whole numbers m and n,

1. $ma < nb$ if and only if $mc < nd$;

2. $ma = nb$ if and only if $mc = nd$; and

3. $ma > nb$ if and only if $mc > nd$.

That is, although in some cases, ratios of magnitudes may not equal ratios of whole numbers, they always have unique locations in the ordered series of ratios of whole numbers, and any two ratios of magnitudes with the same location are identical. This reveals how measurement of magnitudes is always theoretically possible. From a practical viewpoint, the location of ratios is not generally determinable, but the idea of a location is coherent. Euclid's definition marks the starting point of the *Euclidean paradigm of measurement*, according to which ratios of magnitudes of a measurable quantity are isomorphic to the positive real numbers and measurement is the estimation of such ratios.

However, this concept of ratio requires the concept of multiples of a magnitude and this concept, in turn, seems to require the existence of additive concatenation operations. The ancients had instruments and procedures for the measurement of a number of attributes [Russo, 2000], for example, time, weight, and the geometric quantities of length, area, volume, and plane angle, all of which are extensive

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[50]The proof, often attributed to the Pythagoreans, was certainly known to Aristotle (*Prior Analytics*, Bk. 1, Ch. 23, 41a:26-27 see [McKeon, 1941]).

[51]Euclid's *Elements*, Bk. 5, Def. 5 [Heath, 1908]. The thirteen books of Euclid's *Elements* are said (e.g., [Artmann, 1999]) to have been composed or compiled by Euclid during the fourth century BC and Book V, which most concerns the concepts of measurement and quantity, is, according to tradition, attributed to Eudoxos of Cnidus. For convenience I will simply attribute its contents to Euclid.

[52]Dedekind acknowledged his debt to Euclid.

magnitudes and in relation to which multiples of magnitudes are constructible, within limits, via concatenating objects. As was clear from the earlier discussion of Russell, however, multiples of magnitudes and concatenations of objects are logically distinct concepts. Aristotle knew that when volumes of hot and cold liquid are combined 'both by combining destroy one another's excesses so that there exist instead a hot which (for a hot) is cold and a cold which (for a cold) is hot,' and yet, he said, the intermediate temperature 'according as it is potentially more hot than cold or *vice versa*, will in accordance with that proportion be potentially twice as hot or as cold — or three times or whatever' (*On Generation and Corruption*, Bk II, Ch. 7, 334b 8-16 (see [Barnes, 1984]). At the time, temperature was thought of as a quality (i.e., an attribute relative to which only judgments of greater or less than could be made). Yet Aristotle's thinking went beyond that and the degrees of a quality were implicitly given a structure capable of sustaining ratios, that is, one akin to quantitative structure. If only by implication, he detached the concept of multiples of a magnitude from that of operations of concatenation. This was an important leap of theoretical imagination for it meant that continuous quantities need not be extensive.

The tendency to think of qualities by analogy with quantity was a striking feature of fourteenth-century science. Degrees of qualities (or *intensive magnitudes*[53]), such as temperature, velocity, and even pain and pleasure, were thought of as, in principle, measurable [Crombie, 1994]. As Nicole Oresme, put it, 'the measure of intensities can be fittingly imagined as the measure of lines' (*De Configurationibus* I, i [Claggett, 1968, 167]), thereby attributing to them the quantitative structure of length. The idea had been explored earlier at Oxford, first, by John Duns Scotus [Cross, 1998] and then the 'Oxford Calculators' [Sylla, 1974]. While extensity and intensity were contrasted, both were thought of as quantitative, that is, as capable of sustaining ratios. However, in the case of intensive magnitudes, the additive component of quantitative structure was not thought of as something that observers have direct access to via a concatenation operation [Cross, 1998].

This implies that the distinction between extensive and intensive magnitudes is not based upon differences intrinsic to the magnitudes themselves, but has to do with how we relate to them. There is one concept of quantitative structure for measurable attributes (viz., that found in lengths), but our ways of finding out about it differ from case to case. The concept of quantitative structure is a theoretical concept, one discoverable in a variety of ways. This fourteenth century understanding of quantity was more sophisticated than much later thinking about measurement. Its immediate effect was to create an intellectual climate in which the concept of quantity was distinguished from that of extensive magnitude [Solère, 2001] and this may have contributed to the scientific revolution of the seventeenth

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[53]Degrees of qualities were also known as *latitudes of forms*. Each form or quality that admitted of degrees was said to have a latitude or range of intensities. The latitude of a form was contrasted with its extent (spatial or temporal). *Latitudo* was the Latin translation of the Greek, $\pi\lambda\alpha\tau\sigma\varsigma$ (*platos*), which 'was connected with the terms for tightening and relaxing the strings of a lyre' [Sorabji, 2002, 57]. The length of a lyre's string was analogous to the extent of a quality, and its tightness, to its intensity.

century [Grant, 1996].

This development in the concept of quantity was a two edged sword. On the one hand, it created conceptual space within which the measurement of non-extensive attributes could be conceived of as possible, and this was important in the subsequent history of physics, but on the other, it led to increasingly promiscuous applications of quantitative concepts outside of physical science. During the eighteenth and nineteenth centuries, while quantitative physics flourished, there were controversies over whether this or that non-physical attribute was really measurable, beginning with Thomas Reid's 1749[54] essay on quantity and leading up to the debate over Fechner's psychophysics.[55]

These controversies were the context for Immanuel Kant's 'impossibility claim,' in his *Metaphysical Foundations of Natural Science*, that psychology cannot be a science because quantitative concepts are inapplicable[56]. His reasoning, as reconstructed by Thomas Sturm [2001; 2004][57], rested on the premise that mental concepts could not be construed as possessing quantitative, mathematical structure. If attributes are to be measured, then it must be possible to understand them not only as ordered attributes, but also as additive, continuous structures. As perspicacious as it is, this point cannot be made with full force unless the meaning of quantitative structure is first articulated. This involves specifying quantitative structure in such a way that the concept of ratio, in Euclid's sense, emerges as a relation between magnitudes. Building upon Helmholtz's [1887] contribution, this was accomplished by Hölder [1901] with his axioms for unbounded, continuous quantities and proof that the system of ratios of magnitudes of such quantities is isomorphic to the system of positive real numbers, thereby proving the central plank of the Euclidean paradigm.[58]

This enriched the understanding of the concept of quantity enormously. However, another step required clarification prior to consideration of the issue of whether non-physical attributes are quantitative. This was the matter of whether this issue is merely logical or, empirical, as well. Kant seemed to think that it was logical and so did others. For example, Henri Bergson, in *Time and Free Will*[59], thought that, as a matter of logic, every ordinal attribute must be quantitative. Applying this to psychophysics, he argued that

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[54]See Reid [1849], which was a rebuttal of Francis Hutcheson's [1725] explorations of a quantitative, moral psychology. Hutcheson's contribution was, of course, highly speculative (see [Brooks, and Aalto, 1981]), but a model of restraint compared to extravagancies such as the 'female thermometer', a device constructed for measuring 'the exact temperature of a lady's passions' [Castle, 1995, 21]. See, also, Ramul [1960].

[55]See Titchener [1905].

[56]See Kant [2004].

[57]See also Nayak and Sotnak [1995].

[58]Neither Helmholtz nor Hölder intended to inaugurate a new paradigm of measurement, such as the representational, as is alleged (e.g., [Savage and Ehrlich, 1992]). They made explicit exactly what it means to propose of some attribute that it is quantitative within the Euclidean paradigm.

[59]The original French edition was published in 1889. Interestingly, Bergson used this as an argument against the possibility of psychophysical measurement, but others (e.g., [Bradley, 1889]) used the same kind of argument to support its possibility.

> if we grant that one sensation can be stronger than another, and that this inequality is inherent in the sensations themselves, . . . it is natural to ask by how much the first sensation exceeds the second, and to set up a quantitative relation between their intensities. . . . If. . . we distinguish two kinds of quantity, the one intensive, which admits only of a "more or less," the other extensive, which lends itself to measurement, we are not far from siding with Fechner and the psychophysicists. For, as soon as a thing is acknowledged to be capable of increase and decrease, it seems natural to ask by how much it decreases or by how much it increases. [Bergson, 1913, 71-72].

Helmholtz and Hölder took the opposite line, viz., that resolution of the issue depended upon empirical evidence. Helmholtz [1887] distinguished 'additive' from 'non-additive' magnitudes[60] and he thought that as far as the mathematical expression of the laws of physics is concerned, these kinds of magnitudes are alike. That is, the mathematical structure of these two kinds is identical. The distinction between them derives from our different ways of identifying them and, so, depends upon how we relate to them and he noted that it 'corresponds to the one which metaphysicians of an earlier period wished to state using the antithesis of *extensive* and *intensive* magnitudes' [1887, 99], thereby locating his exposition within the historical tradition of the Euclidean paradigm of measurement. His distinction was no help to psychologists. Their kind of measurement, if it was measurement, was clearly neither additive nor non-additive, in Helmholtz's sense.

Following up on Bergson's [1913] kind of argument, a substantial number of investigators[61] proposed that psychologists were actually measuring sense *distances*. Bertrand Russell had noted that

> since the differences of magnitudes are always magnitudes, there is always (theoretically, at least) an answer to the question whether the difference of one pair of magnitudes is greater than, less than, or the same as the difference of another pair of the same kind. [1903, 183].

William Brown and Godfrey Thomson then noted that even with just five ordered magnitudes, given the orders of their differences and also of the differences between those higher-order differences, and so on[62], good approximations to quantitative measures of the magnitudes follow, so that

> With an infinite number of quantities, and all the gradings of their differences, we should, it would seem, arrive at an exact solution of

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[60]Upon which distinction, Campbell [1920], as noted, based his between 'fundamental' and 'derived' measurement.

[61]Titchener reported that 'the view of mental measurement as distance measurement . . . found advocates in Delbœuf, Wundt, Boas, Stumpf, Ebbinghaus, James, Meinong, Höfler, Stout, and G. E. Müller' [1905, cxxxiii], a list that includes some of the most important names in nineteenth century psychology.

[62]In the social science literature, this kind of structure is now called an 'ordered metric scale,' following Coombs [1950].

the problem, so that grading[63] and measurement are not perhaps so different in their nature as might at first be thought. [Brown and Thomson, 1921, 12][64].

Bergson's argument that mere order entails quantity suddenly seems valid. This potent cognitive illusion is the *psychometricians' fallacy*. Hölder [1901] had already shown that whether such differences are quantitative is an empirical issue.[65]

In Part II of his paper[66], Hölder [1901] presented ten axioms for distances between points on a straight line, the only relations mentioned being those of order between points and equality between distances, from which he proved that distances satisfying his axioms are quantitative. Of course, distances between points on a straight line stand in implicit relations of addition. For example, if for any three points, A, B, and $C, A < B < C$, then *the AB-distance + the BC-distance = the AC-distance*. Hölder's axioms exploit this fact without referring directly to additivity. For example, his seventh axiom [Michell and Ernst, 1997, 346] says:

If $A < B, B < C, A' < B'$ and $B' < C'$, then it always follows that if *the AB-distance = the A'B'-distance* and *the BC-distance = the B'C'-distance*, then *the AC-distance = the A'C'-distance*.

This axiom is an indirect test of the Euclidean principle that in quantitative contexts, equals plus equals gives equals (and, so, is related to double cancellation). Its *testability* is an important feature. Some of Hölder's axioms could be interpreted as indirect tests of the hypothesis that distances are quantitative and, so, could have been adapted to test the claim that psychological measurement is a kind of distance measurement. Had the psychologists on the Ferguson Committee been aware of this (and they should have been because, within the Anglophone literature, Nagel [1932] referred to Hölder [1901] — although not to Part II[67]), they could have commenced a rebuttal of Campbell's critique. Campbell's assertion that fundamental and derived measurement are the only routes to quantification

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[63]By 'grading,' Brown and Thomson meant ordering.

[64]Campbell [1933] noticed the same fact. Given that Brown was a member of the Ferguson Committee and opposed Campbell's critique of psychophysical measurement, why was the matter not raised? It seems that Campbell thought it was a possible means of deriving measures, but did not think it was an alternative to fundamental and derived measurement.

[65]I know of no evidence to suggest that Hölder was thinking of psychological measurement, although in other publications (e.g. [Hölder, 1900]) he cited work by the founder of modern experimental psychology, Wilhelm Wundt (who, earlier, incidentally, had been Helmholtz's assistant), and Hölder was a contemporary of Wundt's at Leipzig. Heidelberger [2004] argues that Helmholtz's [1887] paper on measurement was directed against Fechner's attempts at psychological measurement, so it is possible that Hölder was aware of the debate over psychophysical measurement.

[66]See Michell and Ernst [1997].

[67]These psychologists and Campbell were not the only ones interested in measurement who overlooked Hölder's work. Virtually everyone did until the second half of the twentieth century. Of course his work was not neglected by mathematicians (e.g., Huntington [1902]).

was refuted well before it was asserted[68]. Hölder proposed an indirect way, other than derived measurement, of diagnosing whether attributes are quantitative.

In this diagnosis, evidence could go either way. If, given six sensory intensities, A, B and C, and A', B' and C' in which $A < B < C$ and $A' < B' < C'$, *the AB-distance = the $A'B'$-distance* and *the BC-distance = the $B'C'$-distance*, while *the AC-distance $\neq$ the $A'C'$-distance*, then the evidence would suggest that these 'distances' are not quantitative and, so, not really distances at all. The psychometricians' fallacy overlooks the fact that while it may be meaningful to talk about differences between levels of an ordinal attribute, the attribute is only quantitative if these differences possess the structure of distances between points on a straight line. Evidence relevant to this possibility can be acquired via tests based upon Hölder's axioms.

Hölder's work on difference systems[69] was an important extension of Helmholtz's contribution to the issue of the kinds of evidence relevant to the hypothesis that an attribute is quantitative. Hölder discussed the issue in a way that could have been helpful to psychology and social science because it considered possibilities outside those of fundamental and derived measurement and proved that in certain sorts of situations, information about ordinal and equality relations is sufficient to provide evidence for the existence of quantitative structure.

In following the trajectory of the concept of quantity from Euclid to Hölder, it is clear that the understanding of this concept has deepened considerably over this period. By the commencement of the twentieth century, it was clear that the ascription of quantitative structure to any attribute is a theoretical hypothesis, the truth of which, like any hypothesis in science, is best considered in relation to evidence. Hölder's axioms of quantity made it clear what this hypothesis amounted to and it was also clear that there is a range of different kinds of evidence that might be considered: viz., Campbell's categories of *fundamental* and *derived* measurement, and Hölder's category of *difference* measurement. Given the infinite complexity of nature and the vicissitudes of our cognitive and causal relations to various natural systems, it follows that there can be no limit upon the ways of obtaining evidence in relation to any hypothesis. One of the ways in which science progresses is through finding new ways to test old hypotheses. This was the point that the trajectory of the concept of quantity had reached early in the twentieth century.

If the picture of this trajectory becomes unclear throughout the remainder of that century, it is because discussions about the logic of measurement were rerouted by Russell's inauguration of the representational framework, Campbell's insistence that fundamental and derived measurement exhausted the relevant categories of evidence, and mainstream psychology's insistence upon quarantining the quantitative imperative within a criticism-free bubble. However, having worked

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[68]Also overlooked were the papers on psychophysical measurement by Norbert Wiener [1914; 1915; 1921], although this is more understandable, given that they used the notation of Whitehead's and Russell's *Principia Mathematica* (see Fishburn and Monjardet [1992]).

[69]To use the term favoured by Krantz, *et al.* [1971].

through the representational theory to the realist theory, it is clear that there is only one paradigm of measurement, the Euclidean paradigm. However, there are, potentially, many ways of testing whether attributes are quantitative. Thus, one of the most important achievements of the investigations undertaken in the second half of the twentieth century under the auspices of the representational approach was the exploration of the multifarious ways in which evidence for quantity is obtainable. This point may be illustrated by considering further the theory of conjoint measurement.

As presented above, this theory is not in a form optimal for testing the hypothesis that attributes are quantitative. Some of the axioms (viz., the solvability and Archimedean axioms) are not open to direct test. They assert the existence of particular things (a fourth term solving an equation, in the case of solvability; and a finite standard series, in the case of the Archimedean condition) and in neither case would failure to find the relevant thing count as evidence against the axiom involved because, given that our resources are always finite, our searches cannot be exhaustive. On the other hand, the double cancellation condition is directly testable because it is in the form of a general hypothesis: whenever certain sorts of relationships are present, another must obtain.

Conjoint measurement applies to hypotheses of the sort that one attribute, P, is related additively (or multiplicatively)[70] to two other attributes, A and X (as mass is related to volume and density). Since data relevant to such a hypothesis requires identifying a finite number of levels of the two attributes, A and X, the problem of testing the hypothesis reduces to that of specifying the conditions under which an additive representation can be given of the order relations within a finite, two-factor data matrix. Such matrices may be, in general, indefinitely large. Scott and Suppes [1958] showed that in general, a finite number of conditions necessary and sufficient for such a representation cannot be stated. However, for each specific finite case, a finite set of directly testable cancellation conditions exists [Scott, 1964]. The number of such conditions increases with the size of the matrix, which is why a general finite set, fitting all such matrices, cannot be stated. However, if for any specific finite data matrix, the set of cancellation conditions necessary for an additive representation is satisfied by the data, then the hypothesis that the relevant attributes are quantitative is supported.

These testable conditions are called 'cancellation' conditions because they include double cancellation (if the matrix is at least $3{\times}3$) and constitute a hierarchy of conditions of the same general form. The hierarchy begins with single cancellation:[71]

A weak order, $\leq$, upon a product set, $P = A \times X$, satisfies single cancellation *if and only if*

1. for any a and b in A and x in X, if $\langle a, x \rangle \leq \langle b, x \rangle$, then for all y in X, $\langle a, y \rangle \leq \langle b, y \rangle$, and

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[70]More generally, *non-interactively* [Michell, 1990].

[71]Krantz, *et al.* [1971] refer to this condition as *independence*.

2. for any a in A, and x and y in X, if $\langle a, x \rangle \leq \langle a, y \rangle$ then for all b in A, $\langle b, x \rangle \leq \langle b, y \rangle$.

Single cancellation requires that, with respect to levels of X, the order upon all pairs involving, say, a in A, be the same as the order upon all pairs involving any other particular level of A, say b; and with respect to levels of A, the order upon all pairs involving, say, x in X, be the same as the order upon all pairs involving any other particular level of X, say y. Obviously, a test of single cancellation requires at least a 2×2 data matrix. Next in the hierarchy is the double cancellation condition (defined earlier). Following double cancellation is *triple cancellation*, *fourth-order cancellation*, and so on, up to nth-order (for any finite n), depending upon the size of the data matrix. For $n > 2$, the general form of nth-order cancellation is:

> *A weak order,* $\leq$, *upon a product set,* $P = A \times X$, *satisfies* nth-*order cancellation if and only if for all permutations of* $(n + 1)$-*termed sequences of elements of* $A, a_1, a_2, \ldots, a_i, \ldots, a_{n+1}$, *and all permutations of* $(n + 1)$-*termed sequences of elements of* $X, d_1, d_2, \ldots, d_i, \ldots, d_{n+1}$ *(all the elements of A on the left sides of* $\leq$ *in the n inequalities being permuted independently of all those on the right side, and likewise for elements of X), if*

> $\quad$ 1. $\qquad \langle a_1, x_1 \rangle \leq \langle a_2, x_2 \rangle$ and
> $\quad$ 2. $\qquad \langle a_3, x_3 \rangle \leq \langle a_3, x_3 \rangle$ and
> $$\vdots$$
> $\quad$ *i.* $\quad \langle a_{i+1}, x_{i+1} \rangle \leq \langle a_{i+1}, x_{i+1} \rangle$ and
> $$\vdots$$
> $\quad$ *n.* $\quad \langle a_{n+1}, x_{n+1} \rangle \leq \langle a_{n+1}, x_{n+1} \rangle$, then
> $$\langle a_1, x_1 \rangle \leq \langle a_2, x_2 \rangle.^{72}$$

Obviously, tests of nth-order cancellation require $(n+1) \times (n+1)$ data matrices and because there are n antecedent relationships in the above hypothetical expression, each with an a-term and an x-term on both the left and the right of the $\leq$-sign, with all those on the left and all those on the right being independently permuted, the above general form resolves into $(n!)^3$ separate conditions (not all logically independent of each other, of course)[73]. In this way, testable predictions can be derived from the hypothesis that attributes, A, X, and P, are quantitative. For various permutations of the a and x terms, the above general form will resolve itself into tests equivalent to single cancellation, double cancellation, and so on for all $(m-1)$th-order cancellation conditions $(m = 1, 2, \ldots, n)$.

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[72] Adapted from Michell [1990, 80-81].

[73] As explained in Michell [1990], exactly one of these $(n)!^3$ conditions is trivially true and, so, not falsifiable in principle. Each of those remaining is falsifiable in principle.

Since the double cancellation condition was part of the axiom system for conjoint measurement given earlier in this chapter, it follows that for any finite n, the content of nth-order cancellation minus double cancellation is logically equivalent to the combined content of the Archimedean and solvability conditions relative to an $n \times n$ data matrix. Hence, these other cancellation conditions amount to indirect tests of the conjunction of the Archimedean and solvability conditions.

This illustrates the point that results produced under the auspices of the axiomatic approach to the representational theory of measurement constitute an important body of theory enabling specification of testable predictions from the hypothesis that attributes are quantitative. Any discipline, such as psychology or the various other social sciences, to the extent that they affirm the emphasis that science has traditionally placed upon empirical evidence in hypothesis testing, and to the extent that they aspire to measure their attributes, will find a significant place for such results. So far, they have shown little interest in considering the issue of evidence in relation to the hypothesis that their various attributes are quantitative.

5   THE PROSPECTS OF MEASUREMENT IN SOCIAL SCIENCE

Campbell levelled the charge against psychologists that, taking their attributes to 'have an order, they assume that they are measurable' [Ferguson *et al.*, 1940, 347] and this criticism still applies. In so far as they presume to measure their own special, non-physical attributes, psychologists still commit the *psychometricians' fallacy*. It has been a permanent feature of quantitative psychology. For example, J. F. Herbart, discussing Kant's impossibility claim, noted that

> Our thoughts are stronger or weaker, more or less clear; ... our susceptibility for perceptions, our excitability for emotions and passions is variable to a greater or lesser extent. These and innumerable other differences of quantity which obviously occur in mental states, have been reckoned among the less essential modifications, but unjustly, and this is the true reason why the lawful consistency of mental phenomena has not been discovered. [1877, 255].

Herbart considered that ordinal distinctions indicate 'differences of quantity'. Likewise, Fechner noted that

> we can speak of a greater or lesser intensity of sensation; there are drives of different strengths, and greater and lesser degrees of attention, of the vividness of images of memory or fantasy, and of clearness of consciousness in general, as well as of the intensity of separate thoughts. In the sleeper, consciousness is totally extinguished; in the deep thinker it is increased to the highest intensity. Again in the over-all clearness of consciousness separate images and thoughts rise and diminish. Higher mental activity, therefore, no less than sensory activity, the activity of

the mind as a whole no less than in detail, is subject to quantitative determination. [1860, 46-47].

He took it, without further ado, that qualitative attributes are 'subject to quantitative determination.' L. L. Thurstone reasoned that

> We may say about a man, for example, that he is more in favor of prohibition than some other, and the judgment conveys its meaning very well with the implication of a linear scale along which people or opinions might be allocated. [1928, 219]

Thurstone thought that one attitude favouring some policy more than another implies a quantitative distance between them.

As the psychometrician, Roderick McDonald [1999], emphasised, psychological measurement rests on an observational base of order relations.[74] That is, in so far as they attempt to measure their own special attributes, psychologists begin by observing, at best, mere order relations. While they might aggregate over these relations to obtain numerical data, this manoeuvre[75] does not support the hypothesis that the relevant attribute is quantitative.

This familiar manoeuvre actually obscures the issue. Testing the hypothesis that an attribute is quantitative, over and above being merely ordinal, requires observations sensitive to the distinction between order and quantity. While numerical data may be so sensitive in special circumstances [Michell, 1990; 2004], generally it is insensitive, precisely because it is numerical. This is why Stevens's definition of measurement and operational theory of scales, with its emphasis upon numerical assignments, obscures the distinction [Michell, 1996]. Once having obtained numerical data, psychologists are reluctant to treat them as anything less than quantitative measures of the relevant attributes.

The gap between mere order and quantitative structure is wide. One way to grasp it is via Euclid's [Heath, 1908] ratios. Let $a, b, c, d, \ldots$ etc. be any magnitudes of the same unbounded, continuous, quantitative attribute, Q, and let $a : b, c : d, \ldots$ etc. denote the ratios of a to b, c to d, etc. Consider the order relation between any pair of ratios, $a : b$ and $c : d$, supposing without any loss of generality that $a \geq c$. The relation between this pair of ratios, $a : b$ and $c : d$, must fall into one and only one of two discrete classes: (1) the class in which $b \leq d$; and (2) the class in which $b > d$. If it falls into (1), then $a : b \geq c : d$. In this case, the order relation between the two ratios follows simply because of the order relations between the magnitudes involved and nothing else. On the other hand, if it falls into (2), then it is not known whether $a : b > c : d$, given only the order relations between the magnitudes. In this case, the order relation between the ratios depends upon relations between the magnitudes over and above mere order.

In his Definition 7, Euclid [Heath, 1908, 114] noted that $a : b > c : d$ if and only if natural numbers, n and m, exist such that both $na > mb$ and $nc \leq md$,

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[74]Or as he calls them, following the terminology of Clyde Coombs [1964], 'dominance relations.'

[75]This manoeuvre and the method of summated rating scales are the primary means by which numerical data is obtained in psychometrics.

for then $a : b > m/n \leq c : d$. So, the extra relations, over and above order, that determine the order relations between pairs of ratios in the second class are relations of addition sustaining multiples of magnitudes. Order relations between pairs of ratios fall neatly into two disjoint classes: those in which the order relation is determined by just the order of the magnitudes involved (viz., class 1); and those in which the order is determined by the *additive structure of the attribute* (viz., class 2).

Furthermore, the complete set of order relations on the pairs of ratios exhausts what it is to be an unbounded, continuous quantity. The order upon ratios is an order upon $Q \times Q$. Thus, Q is quantitative if and only if this order satisfies the axioms for conjoint measurement (since ratios are a multiplicative function of their terms)[76]. That is, the structure defined by Hölder's [1901] axioms of quantity and the structure given by the set of all order relations on pairs of ratios satisfying the axioms of conjoint measurement are the same structure. An attribute's being quantitative is logically equivalent to an ordering upon ratios of magnitudes of the attribute.

Hence, the hypothesis that an attribute, A, is quantitative could be tested, using n of its levels, by testing whether the order between pairs of elements of $A \times A$ satisfies $(n-1)$th-order cancellation in the associated $n \times n$ matrix. In such a matrix, the proportion of order relations between pairs of ratios falling into class 2 is $\frac{1}{2} \cdot \frac{(n-1)}{(n+1)}$, which approaches .5 from below, as n approaches infinity. That is, in the limit, half the structure of a quantitative attribute is due to merely ordinal relations between magnitudes of the relevant attribute and the other half is due to additive relations. Order is but one half of quantity; additivity, the other. If an attribute is known to be ordinal, one is no more entitled to conclude that it is quantitative than one would be entitled to claim to have scaled Everest having made it only half-way. Falling for the psychometricians' fallacy is trying to be too clever by half.

Suppose that levels of A are ordered and let a, b, c, and d be any levels of A, such that $a \geq c$ and $b > d$, then the issue of whether A is quantitative is tested by considering a series of questions of the form: is the magnitude of a relative to b greater than, equal to or less than the magnitude of c relative to d? If this sort of question can be answered for some levels of A, then evidence about whether A is quantitative is obtainable. Noting this fact enables attention to be focussed on just those relations that depend upon the additive structure of the attribute without being distracted by relationships depending only upon ordinal structure.

As is well known [Krantz *et al.*, 1971], satisfaction of the single cancellation condition depends only upon the relevant attributes being ordinal, not quantitative, so whether or not attributes are quantitative may be diagnosed via the higher order cancellation tests (i.e., double cancellation and above) that are logically independent of single cancellation. At present, the precise cancellation conditions involved in testing for quantitative structure, over and above mere ordinal struc-

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[76]Since Q is continuous, the order on $Q \times Q$ is actually a *continuous* conjoint structure [Narens, 2002, 235] and not just Archimedean.

ture, have not been identified, beyond those for double cancellation [Michell, 1988] and triple cancellation [Richards, 2002][77] (see also Fishburn [2001]).

Because there is no certainty that social science attributes are quantitative, the possibility that they are merely ordered structures requires thorough investigation[78]. The theory of ordered structures is a rich body of theory, the ordinal scale (or weak order) being but one possibility amongst many [Michell, 1990]. This body of theory provides a mathematical resource far more suited to the manifest structure of social phenomena than does quantitative mathematics. If ever social scientists revert to the time-honoured scientific practice of testing claims against evidence, this body of theory may yet prove its importance.

6   CONCLUSIONS

Within social science there is an established methodological tradition committed to quantification modelled on physical science. This chapter describes some of the logical and historical consequences of that commitment. Logically, it entails the hypothesis that social science attributes, such as social attitudes, are quantitative in structure. If they are quantitative, then this is a specific empirical condition, one that need not necessarily obtain. On the one hand, in so far as social science is *science*, this hypothesis will be critically scrutinised in relation to relevant evidence.

On the other hand, the wheels of history are not turned by the hands of logic. Social scientists have been able to ignore these logical consequences because their commitment to quantitative methods is, in part, sustained by non-scientific interests[79] and these consequences are hidden by acceptance of Stevens's definition of measurement and his operational interpretation of measurement scales. This measurement framework has stranded quantitative social science above the high tide mark of the mainstream of quantitative science. This is a huge price to pay, given that the quantitative imperative was meant to advertise scientific credentials.

Quantitative physical science falls within the Euclidean paradigm of measurement and that paradigm is incompatible with Stevens's framework. Given that social science attributes, such as social attitudes, are only manifest as ordinal, the fundamental scientific issue for social scientists, is investigation of the distinction between order and quantity as it applies to these attributes. Approached in an open-minded way, unpalatable conclusions about attributes not being measurable will be accepted, if that is where evidence points. 'We must not ask nature to accommodate herself to what might seem to us the best disposition and order, but must adapt our intellect to what she has made, certain that such is best and not

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[77]While analytical procedures for numerical conjoint measurement are widely used in market research (e.g., Gustafsson, Herrmann and Huber [2000]; and Louviere [2001]), researchers in this area show no interest in testing the order/quantity distinction.

[78]See, for example, Wiley and Martin [1999]. An introduction to the theory of ordered structures is given by Suck [2001b].

[79]See Michell [1990; 1999] and Porter [1995].

something else.'[80]

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[80]Galileo, as quoted in Crombie [1994, 45].

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THE INTERSECTION OF PHILOSOPHY AND THEORY CONSTRUCTION: THE PROBLEM OF THE ORIGIN OF ELEMENTS IN A THEORY

Jerald Hage

In the short span of about five years between 1968 and 1972 some seven books [Willer, 1967; Stinchcombe, 1968; Dubin, 1969; Blalock, 1969; Reynolds, 1971; Gibbs, 1972] and [Hage, 1972] about theory construction were published.[1] Despite this, in general this movement proved to be short-lived. With the major exception of the cumulative research program in small groups at Stanford University, was built around a rigorous definition of theory (among other publications see [Cohen, 1989]) theory construction as an emphasis in doctoral programs largely disappeared.[2] At the time of their publication, none of the theory construction books confronted any of the fundamental philosophical issues except for definition of what is a theory and occasionally sometimes a discussion of positivism (see [Cohen, 1989, 43]). Most of the earlier works cited above, including my own, were concerned with connecting concepts with measures or observations and hence more in the tradition of a methods book but one for theory construction. In particular, my own effort (*Techniques and Problems of Theory Construction in Sociology*, 1972, hereafter TPTC) was designed to help individuals think theoretically more easily and hopefully more creatively, providing both strategies and techniques for doing so and thus was quite different from any of the others including Cohen's.[3]

Given this general lack of a connection between philosophy and theory constructing at least as expressed in these works poses some problems for this essay. On the one hand we have large abstract questions that philosophers raise and on the other hand the detail "nitty-gritty" of thinking techniques. Outside of the obvious connection regarding the debate about concepts and observations (see

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[1]For some of the reasons for why it was short-lived see the Hage (ed.) [1994] which contains several papers analyzing why theory construction fell out of favor during the 1970s and in particular, see the papers by Collins and Waller, Jonathan Turner, Hage, and Cohen.

[2]A less well developed emphasis on formal theory also existed at Columbia University during the 1950s and 60s where Zetterberg taught formal theory and Galtung mathematical models as well as the obvious contributions of Lazarsfeld (see [Price, 2004]).

[3]The strategies concerned the best approach for thinking about a particular element or part of a theory such as concepts or hypotheses or linkages and were discussed at the beginning of each of six chapters whereas the tactics involved specific ways in which one could think about that element.

[Cohen, 1989, Chapter 4]), and more generally logical positivism, one is left wondering what are some of the other connections to various philosophical discussions. In other words, what are the philosophical issues posed in techniques for thinking? My central insight is to say techniques for thinking are essentially arguments for where does one begin to search for ideas, whether intuitions or observations. In turn, this raises other issues including assertions about the nature of theory and what is the best overall strategy for proceeding in developing a theory.

Given this central insight and its implications, there are at minimum three connections between theory construction and philosophy that can be drawn and will be discussed in this essay. First, what does the philosophy of science have to say about the elements of theory that necessitate some thought? Second, what do the philosophical traditions of rationalism, idealism, and empiricism have to say about where one starts in thinking about these elements, the central debate about the origin of ideas being in the mind or in sensate data? Third, is there a tradition within the philosophy of science that relates the impact of the kinds of concepts and ways of thinking with them on the ability to develop scientific knowledge or what is the best way of proceeding?

To return to my central insight, theories are composed of different elements, which could have quite disparate origins or beginning points. The implicit arguments previously made about the basis of knowledge and more specifically the debate between the French rationalists and the English empiricists was also a debate about the origin of ideas. For example, I will argue that the rationalist approach is desirable for concept formation and hypothesis conjecture, an idealist approach is to be preferred for theoretical definitions and causal reasoning, and finally an empiricist approach is most appropriate for operational definitions and equations. My second insight is that one can construct a "better theory" or more robust theory, that is one that is more likely to provide both explanation *and* prediction and to endure for a longer time period by combining these different philosophical traditions within the same theory. Finally, the third insight builds upon Cassirer's [1953] central insight about content or Aristotelian categories and syllogistic reasoning or the strategy of logic except for certain limited functions in a theory.

Connecting the origin of ideas to various philosophical traditions should be approached with the recognition of three potential limitations in this essay: (1) do these philosophical traditions still have relevance; (2) my lack of philosophical expertise; and (3) the inherent limits of thinking about thinking, that is the insularity of a cognitive approach to thinking about the connection between philosophy and theory construction. Each of these limitations needs to be discussed along with my proposed remedy.

One problem with my central thesis is that some of the philosophical traditions that I discuss could be considered to be irrelevant today or largely *passé*. A good case in point is logical positivism, which is not longer discussed. Bachelard [Lecourt, 1999, 489] specifically argues that the old idealist/realist debate is no longer of any value in epistemology and yet I raise this issue again. But as will

become clear below, my "take" on these philosophical epistemological arguments is more than a recycling of old debates and instead represents a synthesis of them. The real question is this useful.

Another problem in connecting philosophy and theory construction is my lack of expertise in philosophy. Therefore, I have relied on are some standard reference works – both in English (Audi, *Cambridge Dictionary of Philosophy*, 1995) and in French (Lecourt, *Dictionnaire d'histoire et philosophie des sciences*, 1999) in order to avoid the philosophical prejudices associated with these languages and to provide me with at least a limited background.[4] But since I am not an expert in philosophy or in any of its specific traditions, I may not present each school of thought in all of its richness.

Finally, a third problem that also flows out of the unusual nature of theory construction and its concern about thinking techniques is it relies upon reflections about how the mind works. Like Descartes, I have access only to my own mind. I do not have access to the minds of other theorists or researchers. From this observation flows several obvious objections. The first one is that I must rely upon how I have thought about particular issues and discern in them specific techniques of thinking as I did in the TPTC. It is quite difficult to move beyond the limits of my own mind because seldom do people report their processes of thinking, indeed these are even *later* arrivals on the scene then the theoretical reasons for examining some empirical phenomena.[5] The second objection is the reliability of one's reflection on how their thinking processes unfold. Just as we construct our biographies, we also construct – and I mean this in all the negative connotations of the word – our history of creativity, slanting it in various ways to prove a point. A good case in point is the various *eureka* moments cited in the history of science.

My solution to these three limitations is to suggest epistemological properties that would make at minimum a *prima face* case that particular patterns of thought reflect either rationalism, idealism, or empiricism. In other words, I am attempting to make a philosophical argument for my thesis about the appropriateness of a specific philosophical tradition for the origin of ideas relative to a specific element in a complete theory. Hopefully, this provides some credibility to the various arguments.

This paper begins with the discussion of the three connections between theory construction and philosophy, specifically the philosophy of science, the various debates about epistemology and the importance of Cassirer's [1953] critiques of Aristolean concepts and syllogistic reasoning that are the basis of this paper. Then the next three sections examine how specific epistemological schools and particular parts or elements of a complete theory, again three: (1) concepts, hy-

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[4]And this choice appears to be wise because while the rationalist approach is not given much weight in the English dictionary, it is live and well in the French one. Nor should this be a surprise to anyone familiar with the history of philosophy. Indeed, this is why I felt it necessary to consider dictionaries from both sides of the channel.

[5]This of course relates to the fundamental debate about whether it is possible to have *a priori* knowledge or not see ???????

potheses and rational thinking; (2) theoretical definitions, linkages and idealistic thinking; and (3) operational definitions, linkages and empiricist thinking. Together these three levels of a knowledge structure to use Cohen's term [1989, 64] reflect what might be called a hypothetical-deductive mode of theory construction. The paper concludes with some observations about the articulation of these ideas.

1 THE INTERSECTION OF PHILOSOPHY AND THEORY CONSTRUCTION IN THE SOCIAL SCIENCES

With the clarification of various limitations that I have in being able to discuss the intersection of philosophy and theory construction, let us begin by asking what are some of the major subject areas in philosophy that can connect in at least in some way with techniques of theory construction. Typically philosophers of science have defined a theory as a set of scientific laws that explain and/or predict ([Audi, 1995, 611–614] and especially the concept of credibility and [Lecourt, 1999, 940–946] and especially the latter page; also see [Cohen, 1989]). Thus, the first connection between theory construction centers around which definition of a theory does one use and which what particular consequences.

My definition is much more complex and is justified on the basis that it synthesizes different perspectives on what is a theory both within sociology and philosophy as can be observed in the first sub-section and in Figure One. Perhaps more critically this complex definition allows for a synthesis of various epistemological schools of theory as well as way of evaluating what is missing from a theory.

This leads naturally into the second connection, the role of epistemology in theory construction. Specifically variable concepts and *multi*-variate hypotheses are linked to the philosophical school of rationalism while theoretical definitions and linkages and linked to the philosophical school of idealism. Naturally operational definitions and linkages are linked to the philosophical school of empiricism but specific arguments are made against logical positivism. Each of these schools tried to argue that the origin of all ideas came from a single source rather than observing that for some parts of a theory, a particular school of thought has a privileged position, although one with some error never the less, hence requiring a synthesis of these different traditions.

Finally, the third connection is the specific work of Ernst Cassirer [1953] on the role of syllogistic reasoning in the formation of knowledge. In my own struggles in developing sociological theory, I discovered the advantages of general variables as distinct from content categories and then searched for an explanation or justification in the philosophy of science. Cassirer [1953] provides such a justification, and I might add considerable evidence, that the type of concept and syllogistic reasoning influences the development of scientific knowledge. I would suggest that there is a larger lesson in his analysis about the properties of concepts and their impact on knowledge formation.

A well trained philosopher would probably find many more points of contact

between theory construction and philosophy. Before proceeding with the three that I have isolated – philosophy of science, epistemology, and the types of concepts – let me add several general disclaimers and several recognized omissions.

The two disclaimers focus on the problem of truth and the temporary nature of theory. The issue is not what is truth, but instead what is knowledge or perhaps more correctly various kinds of theoretical knowledge and how to discover them whether via cognitions or data collection. A complex definition of theory is an argument that there are different kinds of theoretical knowledge. But I make no claim that what is constructed is truth but instead simply that as one adds more and more elements as defined below, the theory becomes more robust because one is reducing the margin of error in thinking and increasing its scope, durability, and parsimony. But as indicated below in the first sub-section robust theories require a number of elements that go beyond the usual discussions in the philosophy of science – at least in sociology.

The second disclaimer is that theories are quite temporary as Merton [1957, 53] observed in the following quotation:

> Our little systems have their day;
> They have their day and cease to be.

Everyone is also familiar with Kuhn's thesis of scientific revolutions. Again, I make no claim that the theories constructed based on some version of my definition will necessarily have any long-term durability. The objective is more how to think about theory so that it will last *a little bit longer* before it disappears, a more modest goal. Indeed, one of the advantages of having a complex definition of a theory is that it facilitates the change process, that is the reformulation of the theory. In addition, the claim of theoretical disappearance really needs to be examined as well. Many readers of Kuhn have missed the fact that the scientific revolution consists of incorporating much of the previous scientific laws within a new framework that makes prior work a special case. Newton's laws still work within a general theory of relativity; they are simply a special case but a very fundamental one. In other words, theory construction is a continuous process in the construction of knowledge and the fact that some parts of a theory are discarded as others are added does not negate the importance of this process as some social constructionists might maintain.

Some of the major omissions are as follows. Typically philosophers of science examine various theories in a specific area such as physics or biology and then discuss epistemological problems within current theory. One can imagine that a philosopher specializing in either sociology or anthropology could have a field day pointing out all the ideas in reasoning and fallacies. I will not explore specific issues in this sense. In particular, I would not examine the philosophical debate about observables and non-observables or what are called theoretical entities even though sociology abounds with them. For example, the concepts of society or of organization and many of the other social collectives could be considered as non-observable entities. I will, however, discuss the problem of theoretical and opera-

tional definitions of the definition of organizations as a unit of analysis to illustrate several points about the problem of non-observables and their measurement. Nor will I deal with the problem of methodological individualism and various variations on this theme, a theme found elsewhere in this collection of essays. These are important topics but they are not the thrust of this present essay. There are probably other connections between philosophy and theory construction that are worth considering as well.

1.1 The Definition of a Theory and the Philosophy of Science

The obvious beginning point is philosophy of science and what it is has to say about the constituent elements in a theory. Usually the definition of a theory in the philosophy of science emphasizes the collection of laws that have been experimentally verified as we have already observed. In selecting a particular definition of a theory must ask what contribution does each element in a theory make, that is a justification for each component and perhaps more critically what is the justification for the whole. The arguments that I will make are centered on the following themes:

- Checks and balances in thinking

- Theory that is more robust

- Syntheses of various epistemological schools

I proposed a much more complex view of what I prefer to call a complete theory, recognizing that few theories in the social sciences have this characteristic. My definition of a complete theory evolved in the process of teaching theory construction as I tried to determine errors in thinking and find ways of reducing if not eliminating these errors. Gradually I arrived at the definition of a complete theory had to have six elements each of which can be further sub-divided into two categories or types:[6]

1. concepts and especially a number of general variables or continuous concepts;

2. hypotheses and especially those that postulated continuous connections;

3. concepts with both theoretical and operational definitions;

4. hypotheses with both theoretical and operational linkages;

5. the arrangement of concepts into primitive and derived terms;

6. the arrangement of hypotheses into axioms and corollaries.

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[6]In TPTC, I used six chapters for six major elements but in fact each element can easily be divided into two for a total of twelve.

The justification for such a complex definition of a theory is based on at minimum three arguments. The first argument is that each of these elements provides a specific epistemological advantage. In Figure 1 these elements are listed along with their epistemological function in the theory. This is the critical point. Eliminate one of these elements and you lose that specific attribute. Without operational linkages you can not have testability or falsification. Most critically, without theoretical definitions you can not specify meaning or else you are likely to fall into the pitfall of logical positivism. It also might be noted that these different elements also provide a way of defining alternative kinds of theoretical knowledge, and one might add disparate perspectives on what is "truth".

Figure 1

Elements of a Theory and Their Contributions

Theory Element or Property	Contribution
1. general variable concept	description
2. problematic, analytical unit	classification
3. continuous hypotheses	analysis
4. conditions, limits	specification of the generality
5. theoretical definitions	meaning
6. theoretical linkages	processes of causality, plausibility
7. operational definitions	measurement
8. operational linkages	testability
9. primitive terms	parsimony
10. derived terms	elimination of tautology
11. premises	simplicity of the theory
12. corollaries, derived equations	elimination of inconsistency

The second argument is that by adding more of these elements one constructs a more robust theory that is more likely to gain in scope, parsimony and durability. Precisely because each element it is a different way of thinking checks and balances on our cognitive processes are established that reduce but of course do not eliminate error. Theoretical definitions change the meaning of a concept and the way in which we integrate it into a hypothesis is an obvious example. In TPTC, I made a number of arguments about a multiple-fold dialectic that increases the probability that the theory will have both explanatory and predictive power. I need not repeat these arguments here.

I would submit that most sociological theories, which seldom contain more than two or three of these elements, lack robustness for this reason and this is one of the major reasons why sociology has not accumulated much knowledge despite a considerable expenditure of money over the last four decades. Without careful theoretical and operational definitions and linkages, one is less likely to discover knowledge because of the absence of various checks on either thinking or research

data. Indeed, if sociological theories contain at least the first eight elements, this would represent a significant improvement and lead to more accumulation of knowledge in the discipline. Perhaps the more important use of this list is that it provides a diagnostic for what is missing and therefore what contribution is required to make the theory more complete and thus to improve upon it. As I suggested above, the main advantage of this quite formal definition of the elements in a theory is that it encourages the continuous improvement of sociological theory furthering the quest for knowledge and I might add employment opportunities.[7]

The third argument flows from the second. Since each element reflects a different way of thinking, their integration also allows us to synthesize different schools of epistemology in philosophy. Again, the various arguments between them reflect their different strengths and weaknesses. The most striking example of this is that a very careful distinction was drawn in TCPC between theoretical and operational definitions and theoretical and operational linkages and thus the debate between the rationalists and the empiricists. In this way there was from the beginning an attempt to combine theory and research in TCPC without falling into the pitfall of logical empiricism or positivism. But what was not stated were the philosophical arguments as to why theoretical definitions and linkages are better viewed from the rationalist or mind perspective and operational definitions and linkages are best constructed empirical via trial and error. These arguments are made below.

Another justification for these twelve elements is that in fact, philosophers of science have discussed these various elements in different contexts. Cassirer [1953] discusses the role of variable concepts in the history of the development of knowledge.[8] Hempel [1952] limits logical positivism by arguing for the importance of nominal or theoretical definitions. Certainly Popper's [Audi, 1995, 631] work on the falsability doctrine is well known. Also, the various concepts of creditability that have been isolated by philosophers of science such as inductivism and simplicity refer to various elements in a theory. For example, the simplicity of a theory is frequently reducible to the arrangement of concepts into primitive and derived terms, while the arrangement of axioms and corollaries besides coherence usually has the function of simplifying the reasoning as well as eliminating inconsistencies. This definition of a theory might appear to be the same as argued in the axiomatic method as applied to geometry but again this approach does not have operational definitions and linkages. Another way of connecting these elements is to consider the interpretation of the truth condition [Audi, 1995, 235] such as correspondence or prediction, coherence which we have already discussed and pragmatic cognitive value, which can be interpreted as predictability.

But despite various philosophers of science touching upon each of these elements, I have not found anywhere in the philosophy of science the normative argument

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[7]Since economics has developed knowledge at least in the eyes of the general public and more critically many politicians, it has influenced considerably public policy and in many areas where perhaps it should not. Until sociology develops the same, there will be few positions available for sociologists outside of academic world (see [Collins and Waller, 1994]).

[8]Cassirer's term for them is function whereas his term for categorical concepts is substance.

that I made in TPTC that a complete theory requires all twelve of these elements. Instead, more of their concern has focused on what is explanation and this frequently has been answered by a formatting a theory in a syllogistic mode. But following Cassirer's critique of the mode of reasoning, one needs a better justification than this. In particular, in sociology, there is a very special reason why this normative theory is particularly appropriate. Given the nature of social reality we have to postulate the existence of a number of entities or units of analysis such as organizations, societies, networks, and world systems that some might argue are not only non-observables but non-existent.[9] Beyond this is the quite amorphous or non-material nature of the social world, e.g. consider for example the concept of social knowledge, which has become increasingly critical in a number of post-modern theories of society (see for example [Hage and Powers, 1992]). The words "social knowledge" are used to explicitly avoid the whole philosophical debate about truth and knowledge, but even so, one can not avoid the problem of a theoretical and operational definition of this concept for effective communication. Unfortunately one can peruse many books these days in which knowledge is key concept and yet is not defined, another reason to use it as an example in this essay (see below).

One cannot leave this topic of the definition of a theory without noting the critique of the social constructionists who argue that the facts that we isolate from all the various richness of social reality are in a sense constructions. While they then critique the utility of theory and thus theory construction, the wiser response is to recognize this limitation and move on to construct "better" theories by adding more variables and *multi*-variate hypotheses.

One concluding point. While the arrangement of concepts into primitive and derived terms and into higher order axioms and corollaries relies to a certain extent on logic, I am not suggesting that the explanation be represented in a formal syllogistic structure as is usually done by philosophers of science [Hempel, 1965] and some sociologists [Cohen, 1989]. Consistent with Cassirer's critique, it is far better to think about the relationships between variables as multi-variate trajectories across time rather than a binary, static one. And as we have suggested this formulation leads more easily into a search for the complex causal processes that connect three or variables [Hage and Meeker, 1988]. It also encourages a different choice of research designs, one that is cross-temporal and cross-analytical units.

1.2 The Origin of Theoretical Ideas and Rationalism, Idealism, and Empiricism

One important implication of having a complete theory defined by twelve elements is that each element represents a way of thinking and thus a potential debate about

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[9]Indeed, because most social researchers do not define carefully these social entities. Some do not "perceive" them and fall into the pitfall of methodological individualism or the fallacy of misplaced concreteness.

the origin of a specific kind of element. In TPTC, in each of the six chapters, one for each of the six basic elements of a theory (concepts, hypotheses, definitions, linkages, arrangements of concepts, and arrangements of hypotheses), I began with a discussion about different ways of thinking about finding or constructing that specific element, and then argued for a particular thinking strategy on the basis of its speed, creativity and likelihood of being substantiated with empirical research, that is its durability. But I did not make a connection to the various epistemological schools except for logical positivism, which I argued against because of the importance of the theoretical definition.

Epistemology as well known is concerned with the nature of knowledge and justification rules for "truth." I would also argue that various epistemological schools are also making claims about the origin of ideas. *The question is where is the best place to search in constructing each of these elements, in the mind or in perception?* If a theory is defined by six elements, each of which can be further divided into sub-categories, it might be said to have six different potential origins of ideas or epistemological foundations. Given this perspective, then it results in a slightly different take on rationalism, idealism, and empiricism. These reflect not only arguments about justifications for knowledge but can be seen as starting points in the construction of a theory. The reasons why are explicated in the next three sections where theoretical concepts, hypotheses and the school of rationalism is discussed, followed by theoretical definitions, linkages and idealism, and finally operational definitions, linkages and empiricism. For simplicity sake, I only concentrated on these three schools.

My argument for rationalism and idealism is essentially an argument that the beginning place in the search for ideas is better started with the mind than with empirical data. Theories are simplifications and the mind is best at reducing the varieties of data available to a manageable form. Without raising the old battle about *a priori* and *a posteriori* knowledge, I would suggest that the mind is capable of great leaps without much information and this capacity should be exploited.[10]

1.3 Types of Concepts and the Development of Knowledge

In thinking about concepts and hypotheses, we must be aware of the different kinds of concepts. Some are difficult to think with and others are much easier. Cassirer [1953] argued that only when science left Aristolean categorical concepts and syllogistic reasoning behind did scientific knowledge began to develop. Science uses variable concepts combined into what he called functions rather than substantive concepts where the issue is the question of essence.

In TPTC, I provide a number of arguments about why it is easier to think with variable concepts connected in continuous ways that I will not repeat here.

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[10]How much more appropriate would it be to ask the degree of *a priori* knowledge was involved which is another way of saying how big is the leap of the mind but this would break with the philosophical penchant for categorical concepts and thinking, which has plagued philosophy since Plato.

Essentially, the argument is that one obtains simultaneously both much more complex and much more concise sets of concepts and hypotheses. Here I would like to add some new arguments. Categorical concepts leads to an emphasis on classification and worse yet the search for Platonic essences. Obviously, we need some of these to specify the unit of analysis or the objective of our research. But what is more critical are the variable concepts employed in the analysis.

With variable concepts, which are clearly only one tiny pieces of some full description, we are lead to search for more of them to have a more complete rendering of the unit of analysis or the problem being examined. Since any variable is only a tiny aspect of what are always complex phenomena, its incompleteness is obvious. Likewise, combining variables into multi-variate hypotheses leads naturally into the issue of how do changes in the independent variables produce changes in the dependent variable and thus quite naturally into the topic of causality.

In my opinion, the single most important attribute of a theoretical statement is its form in some continuous format rather than in a syllogistic one, e.g.

> *Given some set of initial conditions, the greater the X, the greater the Y,*
>
> *e.g. Given high levels of education and of wealth, the greater the complexity in the division of labor, the greater the decentralization*

This format encourages is to think in terms of changes across time, and one might even add causal changes provided a given set of conditions. It is this kind of thinking that is much more likely to lead to the development of robust theories. Again, observe that these advantages are lost if the explanation in placed in a syllogistic format.

One might assume that if I am arguing against a syllogistic format that means that logic plays little role in the construction of a theory. This is not the case. With theoretical definitions and especially as they move towards primitive terms, then multiple deductions involving derived concepts and beyond this hypotheses are possible. By avoiding syllogistic reasoning with categorical concepts as Cassirer [1953] demonstrates, one instead knows more and more via the process of abstraction rather than less and less as is the case with Aristolean thinking. Furthermore, the logic involved in the arrangement of axioms and corollaries is more than simply arranging higher order primitive terms and lower order derived concepts because of the need sometimes to make certain assumptions before the explanation can unfold. A good example of this is the logic of functionalism, which must assume some need to be fulfilled or satisfied. This is frequently not directly tested but only some of its implications that flow from other axioms. An example of how to construct a functional theory is provided below in the discussion of idealism in the construction of theoretical linkages.

2   VARIABLE CONCEPTS *MULTI*-VARIATE HYPOTHESES AND RATIONALISM

Following from our discussion of Cassirer's type of concept and of theoretical statement, our first task in thinking about the construction of a theory is to find useful variable concepts and then combine them into *multi*-variate hypotheses. The question is where is the best place to search for these variables and hypotheses. A good starting point is the fundamental distinction between two opposing views about the acquisition of knowledge, namely rationalism (A[udi, 1995, 673–674] and [Lecourt, 1999, 794–799]) and empiricism ([Audi, 1995, 224–225] and [Lecourt, 1999, 365–368]). My position is that in recycling the debate between the English and the French schools around the origin of ideas, it is better to begin with the French school or a rational process of concept formation and hypothesis generation. This perspective is critical because if we are to discuss the *process of theory construction*, we must be concerned about the relative utility of using the mind independently of any perception or if you will sensate data. Again, it is important to observe that this question is not an either-or issue but instead one of degree. We should entertain the possibility of intuition and even *a priori* assumptions can help in the identification and creation of new concepts and new hypotheses. Indeed, I think this approach might give us a whole new way of thinking about the problem of creativity. In effect, the "leaps of the mind" beyond sensate data is precisely the exercise of a rationalist approach to theory construction.

However, I am well aware as I indicated in the introduction that my reflections about how I think are not necessarily the way in which others think or proof that rationalism is a desirable beginning point. Therefore, below I suggest some techniques of thinking where I think one can make a *prime face* case that it reflects a rationalist approach.

2.1   *Finding Variable Concepts*

But how can one think about creating variables independent of data? Perhaps a personal example might be helpful. In TPTC, I cross-classified the dimensions of rights with social structure defined as the distribution of various attributes, and created the idea of normative equality. To my knowledge this idea had never been used before; it was before the campaign for human rights by Carter. Of course, an empiricist could argue about whether I had actually in fact developed this idea independently of any perceptions. Ultimately, one has to admit to this possibility. *What I want to highlight is the way in which the concept was generated.* The concept was "found" by cross-classifying two different concepts to create a new one. This procedure for developing new concepts seems to me one that can operate quite independently of any data. Many of the new concepts generated in this way might be nonsense but then some might be quite useful and allow people not only to perceive dimensions or social objects that previous had been hidden. But more interestingly from the perspective of the debate between the rationalists and the

empiricists is when we locate objects that no one has "seen" before. The periodic table in chemistry is a case in point.

Let me suggest another way in which new concepts can be created with a more recent example from my own work. The term postmodernism became fashionable in the 1970s. The question is what do we mean by this idea? At least in architecture and in art there are some clear empirical referents as the combination of two esthetic styles. Bouncing off this definition and applying it to individuals leads to the insight that postindustrial man could be defined as having two cultural styles, which encompasses esthetics as well as normative patterns of behavior.

The usefulness of this kind of mental operation, in this instance taking a definition from one area and moving it to another, is revealed in the number of new insights that one obtains, that is in the perceptions of aspects of "reality" that were there but had not been apprehended without the concept and its definition. Indeed, some of the evidence that leaped into my field of perception, *once I had the concept*, was quite surprising and overturned a number of revered theories. Perhaps the most important one is the rejection of the thesis that gradually individuals were gaining more and more abstract notions of identity that are associated with larger collectives such as America and Europe. With the concept of post-industrial man, one can entertain the possibility of individuals having an identity of being both French and European or being both Irish and American. Suddenly, conflicts over identity are reduced and resolved. Again, empiricists might want to quibble with me about the origin of this idea but as far as I know it did not come from the data but instead taking one idea, trying to provide a clear definition for it and then applying this to other situations, which are all mental operations.

In turn this definition of post-industrial man lead to the idea of cognitive complexity, that is the capacity to think about a problem from two quite disparate angles. I then made the leap that cognitive complexity in at least the educated part of industrialized societies was changing, that is more and more individuals had this capacity to perceive both the beautiful and the ugly – to return to the original discourse about postmodernism – in many things. I then searched for evidence to support this concept of an evolution in mental processing and indeed found it, all of which is reported in the book I did with Charles Powers on *Post Industrial Lives* [1992]. Again, I do not want to suggest that these ideas are necessarily new. I think it is more the way in which they were developed and the ensuing insights relative several standard theoretical problems within sociology is what is interesting about this rationalist exercise. Indeed, this whole book is simply that, a long theoretical essay that touches upon a number of issues.

Another way of demonstrating a rationalist approach is borrowing a concept developed in one area and moving it to a completely different unit of analysis. Recently the concept of organizational learning has emerged within the context of organizational sociology, itself not a new idea that one can find in economics. Currently I am working on a paper [Hadden and Hage, 2004] that takes the idea of organizational learning and applies it to an entire different analytical unit, namely the population of organizations. Naturally when one changes the unit of analysis,

at least the operational definition must also change. Furthermore, the whole learning process may also alter. Indeed, this is what is interesting about this particular juxtaposition. One is forced to think about in what ways in which populations of organizations learn that is different from the way in which organizations learn. In other words, one does not necessarily assume that everything can be borrowed. This simple juxtaposition can open a whole new area of research and thinking in organizational ecology and again solve a number of issues in explaining and predicting the evolution of organizational populations across time. And that is, of course, the final check and one that is also made by the mind. How fruitful is the new vision?

2.2 Specifying Non-Variable Concepts: Substantive Focus and Units of Analysis in Their Spatial-Temporal Contexts

Although variable concepts allow one to view the multi-dimensional aspects of social reality, these dimensions are themselves attached to objects that require some form of classification. Usually but not always there are two different kinds of categorical concepts. The substance itself, which may or may not be the same as the theoretical problem, and the unit of analysis including the more often than not ignored spatial-temporal context of the research. I will take one illustration from my own work. For many years, I have been concerned with the topic of innovation in organizations. The theoretical problem is to explain why some organizations have more innovation than others. The substance is the topic of innovation and the unit of analysis is organizations. I, myself, ignored the spatial-temporal dimension of the research being conducted in the United States. When transposed to Europe, then it became obvious that previous findings were in some ways dependent upon the spatial context of the U.S. Since then, the comparative study of innovation has lead to the concept of the national system of innovation [Nelson, 1993] indicating how important space and time are.

Another reason why it is important to specify the spatial-temporal boundaries in either our theoretical essays or research is precisely because of the importance of the topic of evolution or of social change. One temptation, and one I must admit that I succumbed to it, is to assume that with general variables the hypotheses that contain them are valid for all space and time. As indicated above, this is not the case. The simplest place for delimiting the limits of the hypotheses and thus the theory that contains them is to indicate place and period about which they are developed, which is usually the contemporary period. This then encourages the recognition of the possibility of change. However, one advantage of general variables is that they may themselves be useful in describing this change. For example, to continue with the example of innovation, what is striking is the rising rates of innovation in many countries which can be partially explained by changes in the variables used to predict the rate of innovation such as complexity, the organic structure and high risk strategies of change (see [Hage, 1999]). But also once recognizes that there is a spatial-temporal context, one can also ask if these

higher rates of innovation are because of changes in these contexts. And the answer is yes, many governments, both developed and developing have initiated national policies designed to encourage innovation.

But just as the spatial-temporal context is ignored, usually the particular kind of substance or category that is being examined is also ignored. For example, in the study of innovation, at first it was not recognized but generally the focus was on incremental innovation rather than radical innovation. In other words, later researchers began to appreciate that innovation itself was a general variable, that there is an issue of the degree of radicalness in the innovation itself.

The same reasoning can be applied to the unit of analysis. Rather study all kinds of groups or organizations or societies, usually a particular kind of group or organization or society is being examined, such as an informal group or an industrial organization or a developed society. In other words, there is usually a failure to specify the adjective that should be placed in front of the noun that represents the analytical unit.

Generally the naming of the substance and the analytical unit is specified even if the adjective that delimits them is not. Obviously, these adjectives are easy to add. Much more difficult is the specification of the theoretical and operational definitions for the substance and for the analytical unit. But that is a topic for the next section.

2.3 Constructing Multi-Variate Hypotheses

One of the weaknesses of TPTC is that it only considered bi-variate relationships and did not suggest ways of thinking about *multi*-variate ones, which of course are more appropriate in any social science. A new book, *How to Build Social Science Theories* [Shoemaker, Tankard, jr. and Lasorsa, 2004] that builds upon my ideas of variable concepts, hypotheses, theoretical definitions and linkages, and operational definitions and linkages, provides a number of answers to this problem including the discussion of models as a sources of ideas.

Given the weakness in my prior work, I want to suggest some new rationalist strategies for developing *multi*-variate hypotheses. The main theme is the idea of looking for contingencies, that is variables that explain when the hypothesis is operative and when it is not. This adds a healthy dose of circumspection in the development of theory, which is all to the good. Furthermore, this can be accomplished in a variety of ways besides empirically, which is admittedly the most typical case.

Playing with new hypotheses is in some respects a much easier mental operation than developing new concepts. My favorite example of how this can be done is the mental operation suggested by Aristotle and which is taught to all French children, namely thesis, antithesis, and synthesis. Thinking about all the reasons why a specific hypothesis works and then all the reasons why it does not is a useful mental operation, one that would reduce the exaggerated claims in many empirical articles. The more interesting issue is finding the contingencies that explain when

one or another version of the hypothesis is correct because then we move into a multi-variate hypothetical mold, which is useful.

A very good example of thesis and antithesis is the current debate between the macro institutional schools–whether the literature is the national system of innovation [Lundvall, 1992; Nelson, 1993], varieties of capitalism [Hall and Soskice, 2001], varieties of business systems [Whitley, [1992a] 1992b], or the American school of the iron cage [DiMaggio and Powell, 1983] – all of whom argue that organizations dance to the same institutional tune. In contrast, are the various theories of organizations that either perceive organizations as having agency, that is they can choose their own strategy or at the opposite extreme are totally determined by population dynamics and selected. This latter perspective argues that almost any change is likely to lead to the demise of the organization ([Hannan and Freeman, [1984] 1989]; [Baum, 1996]). This implicit debate reflects another one of those dualities in the theory literature, in this instance between agency and structure.

We are thus left with an interesting and ready made set of questions about agency and structure or the thesis of institutional structure, the antithesis of agency, and the synthesis of each perspective. When do institutional arrangements constrain organizational forms and make them appear to be quite similar across various populations and when do organizational leaders have opportunities to exercise agency and create new organizational forms. Furthermore, the real question is to ask what would be the strategy that would reduce or eliminate the influences of the institutional setting. That is the more provocative question.

My answer to this question is to argue that the general variable, the contingency of institutional disconnectedness, helps explain when organizations can exercise agency freed from the institutional structure. This idea is obvious when stated, but it is a useful model for explaining how one analytical level can be separated from the influences of another analytical level and thus speaks to some other debates relative to reductionism and methodological individualism.

The origin of this insight was a careful study of 290 radical innovations defined as major breakthroughs in biomedical science during the period of 1880–1980 [Hollingsworth et al., forthcoming]. We found that one-fourth of these major advances were made in only 6 research organizations. In the case of the French, the Institut Pasteur, which was one of the six, accounted for about 40 percent of all the French radical scientific biomedical innovations. More generally though, France did poorly in comparison to the other three countries, Britain, Germany, and the U.S., which together accounted for most all of these discoveries during the century of time that was being examined. This statement is a strong argument for an institutional perspective (why not Canada, the Netherlands, Japan, the Soviet Union, etc.?) but also the singularity of these six research organizations and especially the Institut Pasteur in the instance of France strongly suggest the idea of organizational agency at certain historical moments. This example also illustrates the importance of being sensitive to the unit of analysis and its historical context of nation and period.

A provocative way of rephrasing the problem of institutional disconnectedness,

is to ask:. Why was the Institut Pasteur at least in its origins not French? To pose this question exemplifies some of the freshness of a rationalist approach or mental operation. Once one poses this question, then a series of mental operations on how to establish the non-Frenchness of the research institute comes to mind. In other words, one examines the standard patterns characteristic of French science and then ask whether or not they were exemplified in the case of the Institut Pasteur during the first period of 1888-1918. Among other hypotheses, it seemed recruiting non-French scientists or recruiting French scientists who do not have their diplomas or diplomas from less prestigious schools, finding non-governmental creating sources of money, promoting researchers rapidly rather than through the normal slow channels and ignoring the Ministry of Education in the process, etc were plausible ones to suggest. But many other elements were involved as well, indicating how the emphasis on *multi*-variate thinking is so critical (see [Hage, forthcoming]).

This example also illustrates how it is sometimes quite difficult to separate the origin of ideas as either empirical, that is from sensate data, and how rational thought is independent of data.   Certainly, the origin of the thought processes emerged with an empirical result, namely the unexpected singularity of these six research organizations. But it is much more difficult for me to separate out how the various manifestations of the institutional disconnectedness were indicated by various patterns that I then identified and how much I raised certain issues and then went looking for the data to support it.

A clearer example of largely mental operations in the creation of concept would be to think about the three processes of isomorphism — socialization, state regulation, and competition for funds — as discussed in the iron cage model [DiMaggio and Powell, 1983]. In this paper, the assumption is that all the processes of socialization are the same, that regulation is strict, and the competition for funds is fierce. Instead, one can imagine that these occur in various degrees and then we can ask how does variation in the extent of each of these processes create opportunities for new forms of organization within some organizational population. Or one could take their ideas and simply reverse them, which is the main point of thesis, antithesis, and synthesis, that is, there is great variability in socialization, little state regulation, and not much competition for funds. However, one must take an additional mental step and think about the contingencies that would explain these variations.

Moving back to the above example involving the Institut Pasteur, the socialization patterns were quite similar because of the control of education by the Minister of Education and in the area of medicine by the Ministry of Health. The most interesting aspect of the Institut Pasteur was the denial of the normal patterns of recruitment and of promotion by the leaders of the Institut Pasteur during the period of 1888-1918, reflecting the basic system of stratification in France, issues that are not part of the iron cage model although it does discuss the idea of new patterns of socialization. And here we see how specifying contingencies opens up new areas of thought and allows for the integration of different literatures, a most

useful way of expanding sociological knowledge as one as the theory that we are constructing.

As proof of the power of mental operations, I suggest the reader ask him or herself when is state regulation strong or where is competition for funds fierce and I think you will discover that ideas come readily to your mind. While these are probably based on some sense of data, *it also demonstrates one has to ask the right question to have access to the sensate data and this is my larger point about rationalism for the origin of ideas.*

Perhaps I am dwelling too much on the issue of multi-level analysis but I believe it is a very important case within sociology today and also a fruitful way of developing *multi*-variate hypotheses as well as a model for how to think about the agency vs. structure debate. For this reason, I want to suggest still a third way of developing a sense of contingencies and *multi*-variate hypotheses. The first example was based on an unusual research finding that led to some thought-provoking questions; the second example was based on a theoretical model where processes are turned into general variables. The third example relies on asking what is the larger question or theme in this debate.

That theme is clearly the homogeneity or isomorphism of individual and organizational behavior. But once one thinks about this *as a general variable*, then one asks is it the case that the homogeneity is at the same degree in all countries. Here we see how helpful it is to think in terms of general variables and how much they change our perception. In other words, is the degree of homogeneity the same? Within the institutional schools cited above, there is this assumption of homogeneity. This question leads naturally to a contingency, the concept of the strength of institutions, which explains why it is not the same in all countries. In turn, the mind can begin to ponder other kinds of contingencies that might explain when institutions are strong and weak and perhaps more critically in which areas, another very important kind of contingency. Again, let me suggest to the reader that they think about this and I am sure immediately various possibilities will come to mind. A little thought indicates that larger multi-cultural societies are likely to have less isomorphism and cultural homogeneity than others.

Again, one of the advantages of a rational approach to the construction of hypotheses, is that it allows one to easily move across various literatures. For example, in political sociology, there is the thesis of strong states. Certain stately strength would be another interesting contingency and this insight leads naturally into the whole question of which institutions are strong in a society and how this developed historically across time. Do market develop before or after states? Perhaps this reflects some of the importance contingencies for the differences between Europe and the U.S. or American exceptionalism. It also again means the integration of another important literature, the one on state strength within the context of this debate. We conclude with the irony that the iron cage model was created in a country where the model does not work well at all as it would in Europe because in the U.S., which is large, and multi-cultural and getting more so with time, and some would argue that the state internally is relative weak vis-à-vis various

religious groups, economic groups and other sources of pressure on the state.

A less controversial illustration of a mental operation for the development of multi-variate hypotheses that do not appear to be grounded in any empirical data is to look at the implied questions in the titles of books. Take Mead's famous book *Mind, Self, and Society*. This clearly begs the question: What kinds of minds and what kinds of selves in which kinds of society? If we have a new society, call it post-industrial or post-modern, then the following theoretical questions need to be answered. Chuck Powers and I (*Post-Industrial Lives*, [1992]) have written a very long theoretical essay that represents our mental operations relative to the answers to the implied Median questions. Therefore, there is little point nor space to delve into any detail except to note the following arguments, each of which we think are somewhat provocative but correct in post-industrial society:

- Creativity is now more important than IQ;

- Complex selves can handle the constant change in social roles;

- Symbolic communication is necessary to negotiate role change;

- Role conflict is increasing;

- Burn-out and other signs of role stress are also increasing.

In other words, we answered our theoretical question posed above with the idea of creative minds and complex selves, which we distinguished from the mind and self concepts that are the requirements of industrial society to the Median questions.

This particular book allowed us to combine structural functionalism with not only role theory, which is easy, but with symbolic interaction, which is much less easy. In other words, our central argument become that in post-industrial societies with constant changes in jobs, marriages, and other important social roles, *symbolic interaction became a functional necessity because it could help individuals perform necessary adaptation to a changing society since it allowed for the constant renegotiation of role definitions.* And this points to a major way in which one can search for contingencies and *multi*-variate hypotheses: combine theoretical perspectives.

Perhaps because I began my career with an axiomatic theory [1965], I have always had difficulty in understanding how people could be happy with only one kind of organization, or one kind of or society or one model of man and woman, etc. As soon as one has developed some distinctions about alternative kinds of social collectives, then the immediate question is which contingencies would explain the presence of one or another type. In my [1980] book, *Theories of Organizations: Form, Process, and Transformation*, I developed a complex theory with all the elements defined above. The crux of the theory was to synthesize four perspectives on organizations: structural functional theory, political theory, conflict theory and finally cybernetic theory. The core of the theory relied on the

cross-classification of the environmental dimensions of market size and technical knowledge that generated four kinds of organizations. With the primitive terms of input, structure, coordination and control processes, strategies and performance characteristics, specific variables were derived to describe and analyze these different kinds of organizations. In other words, an attempt was to create a complete description of the organizational system. These then are the basis of a number of *multi*-variate hypotheses. For those readers who feel that four organizations is too simple a typology, let me congratulate you. You are learning to think. The issue is to add other contingencies and create additional types of organizations. The same process obviously applies to any scheme of classification.

2.4   Specifying the Limits to Multi-Variate Hypotheses

As has already been mentioned, the temptation with general variables combined into continuous theoretical statements or hypotheses is to assume that they apply across space and time without qualifications and exceptions. This is obviously no the case. Therefore, one wants to engage in some mental exercises about these qualifications and exceptions or limits. A good place to begin is in the construction of **2.2**, that is the specification of the substance and analytical units including context, together they suggest the following limits:

- Kind of subject matter

- Kind of analytical unit

- Spatial context

- Historical period

As was suggested above, there the emphasis is the search for adjectives, which by definition limit the generality of the multi-variate hypotheses. They also serve a useful function in the publication of articles. The gradually relaxing of the adjectives, which is the movement towards more generality, is a useful way of constructing the conclusions of an article. Therefore, nothing is lost and perhaps much is gained because one has increased the credibility of the proposed hypotheses.

Beyond this, the use of contingencies sets limits on the hypotheses. Indeed, that is the whole advantage of emphasizing the construction of hypotheses around contingencies. They specify limits to the reasoning. It is for this reason that I have emphasized this strategy so much in the examples provided above. In addition, in the various examples provided are two generic strategies for searching for qualifications and exceptions to our hypotheses:

1. the recognition of different analytical levels;

2. the recognition of different theoretical perspectives.

Precisely because social reality comes in multiple layers such as groups, organizations, institutions, and societies to say nothing about the world system, an obvious place to begin to specify the limits of any multi-variate hypothesis to think about the next lower unit and the next higher unit. For example, if we are comparing two societies, we focus on the macro institutional level and the world system as sources of possible qualifications. As we observed, this is precisely what has been missing in both the organizational and institutional literatures.

The same principle applies to the particular theoretical perspective in which we work. Although it is admittedly difficult to raise questions about the theory that are constructing, it is useful to appreciate that it probably represents a particular mode of thinking. The question is what another mode of thinking has to say about the potential limits to our hypothesis.

Hopefully these examples indicate the usefulness of a rational approach in developing concepts and *multi*-variate hypotheses including their potential exceptions and qualifications. But theories can be made more robust in other ways as well, namely by adding theoretical definitions and linkages, our next topic.

3   THEORETICAL DEFINITIONS, LINKAGES AND IDEALISM

Probably everyone is wondering why I introduce an extreme form of rationalism, idealism, into the discussion of theory construction ([Audi, 1995, 355–357] and [Lecourt, 1999, 489–490]). The doctrine that reality reflects the workings of the mind seems to be particularly applicable with two elements in a theory, namely the theoretical definitions and the causal explanations or theoretical rationales of the hypotheses. What makes these particular elements idealist in their special epistemological qualities, which I will try and elucidate.

3.1   *Theoretical Definitions of General Variables and the Movement Towards Primitive Terms*

The theoretical definition of a concept in philosophical terms is a nominal definition that specifies the meaning of some concept. It is the character of the definition that would make it idealist. In particular, I am advocating quite broad definitions that approach the status of a primitive term, hence the reference to idealism. Then, the theoretical definition is a distinct element in a theory. Why is the use of primitive terms inherently an idealist approach? It is because by definition within the context of a theory, the primitive terms cannot be easily defined by a reference to another term. Below a quite different reason is provided for why nominal definitions are likely to be idealist in concept and that is frequently the theoretical definition is quite complex.

This approach of using primitive terms for theoretical definitions has several theoretical advantages. With primitive terms or quite general nominal definitions, we avoid the fallacy of logical positivism where meaning is reduced to the operational definition, that is the explicit measures used to quantify the variable

concepts, or as it is sometimes called instrumentalism. The second advantage is that the search for primitive terms allows one to simplify the conceptual apparatus of the theory because then multiple concepts can be derived via various operations as was suggested above in the discussion of a theory of organizations [Hage, 1980]. In other words, it helps ease into the arrangement of concepts into primitive and derived terms, an element of the theory that is not discussed in this essay.

The provocative argument that I want to make is that a broad theoretical definition, usually using a primitive term, is open to a variety of operational definitions that provides both flexibility to any specific problem and the capacity to generalize theory to other analytical units or problems. Above, I observed that one way of specifying limits to multi-variate hypotheses is to recognize the presence of multi analytical levels and different theoretical perspectives. Usually, these shifts in analytical levels and perspectives imply quite distinctive operational definitions, that is, what is actually measured. In other words, my argument is standing logical positivism on its head, arguing that to create more general and robust theories one starts with general theoretical definitions that facilitate latter the synthesis of different analytical levels and theoretical perspectives.

Furthermore, in this stage of knowledge development in sociology having a theoretical definition that admits to a variety of operational definitions is a distinct advantage. The very specific nature of social reality and the various arguments about its existence including the problem of non-observables requires a quite open and flexible strategy of theory construction. This openness is best provided in the theoretical definitions and linkages.

Examples of general variables with definitions have been provided above. The combination of a concept and its definition was necessary in the discussion because so many of the terms used in sociology have multiple meanings, connotations and denotations. Above, I suggested post-modernism is the combination of two esthetic styles and post-industrial man is the combination of two cultural styles and cognitive complexity is the combination of distinctive styles of thought as represented by disciplines or paradigms. In the case of institutional disconnectedness, I suggested that the concept is defined by the absence of a singular cultural socialization and recruitment pattern, of financial dependence, of political and intellectual control over the career, etc. In other words, the theoretical definition can also be quite complex. Many of our concepts such as post-modernism or power or status are used by the general public but not necessarily in the way in which we would like. Therefore, the discussion of a concept required the specification of the meaning since I wanted to communicate without too many connotations and denotations. It is much easier to think with variable concepts than categorical ones once the connotations and denotations have been circumscribed with the definition.

In large social collectives such as societies, the theoretical definition may frequently refer to some primitive word that is inherently impossible to define by some other small set of words. Consider the following example from TPTC:

Centralization $\equiv$ distribution of power

Power, I would submit, is a primitive term every much like Moore's discussion of the concept of honesty, which also cannot be easily reduced to a few words. The use of primitive terms in large social collective is more likely because one wants to recognize in this instance a wide variety of power processes can exist. We could in a small social collective such as an organization reduce the concept of centralization to a much smaller meaning, namely participation in decision-making. But in the process of reduction, we have obviously eliminated some other ideas about centralization and thus ways in which power might manifest itself, e.g. recruitment processes by occupation (see [Pfeffer, 1981]).

Primitive terms would appear to me to be the case par excellence for an argument about mental images and thus idealist. The major reason is that primitive terms are not easily defined in a few words and thus imagined! The primitive term reflects a basic idea or understanding of a concept and in this sense it is idealist.

But a primitive term is not the only argument for an idealist strategy in thinking about the theoretical definitions. Above, we observed that for many of the concepts in sociology, the theoretical definitions can be complex and again difficult to reduce to a few simple sentences. In effect they are constructed. This property of the definition flows from the complexity of sociological reality. To capture it requires a number of distinctions that are combined in the definition.

The importance of a complex theoretical definition is illustrated in some more recent theoretical work of mine and Chuck Powers [1992] on the whole problem of symbolic communication in the work of Mead and others of the symbolic interactionist school in sociology. The issue is what is the theoretical definition of symbolic communication? If one studies *carefully* the discussions of many of these people, they usually have a cognitive definition of communication but a cognitive definition does not enable one to define what symbolic communication is. And I might add, this has been exactly why this particular theoretical perspective has not advanced; there are many essays but they all say the same thing and never provide a good definition for what is symbolic communication. To solve this problem, Chuck Powers and I defined symbolic communication as the transmission of both cognitive and emotive messages. Emotion is another one of those primitive terms that requires whole books to explicate its various meanings so say nothing about its measures. Symbolic communication is the comparing the two messages, the cognitive and the emotional and ascertaining their agreement.

With this two-fold theoretical definition, we can begin to define some very interesting kinds of symbolic messages that are sent in each face-to-fact interaction:

- the importance of the message to the speak or ego;

- the sincerity of ego or its relative truth value;

- specific feelings;

- importance of the listener or alter ego to ego.

Here it is important to reiterate that it is the individual's judgment of the symbolic messages and not whether there is some absolute standard by which truth is ascertained. One can fool all the people some of the time and some of the people all the time but basically in trying to understand when people discern the insincerity of some president, they are comparing the cognitive and emotional messages. In the book, *Post Industrial Lives* [1992], Chuck Powers and I wrote about a number of interesting ways in which symbolic messages are sent and also about those existential moments, when ego's masks drop and we discover the "truth".

One advantage of this complex definition is that these four messages that can be discerned related to a number of major issues in the social psychology of relationships and groups such as motivation, self-affirmation, affirmation of the other, and trustworthiness. These themes provide the social glue and the connection between two person relationships and larger social aggregates. In addition, these same messages also elucidate some interesting problems in existential philosophy such as the issue of authenticity and in Habermas' work on communication where he ignored this problem of making judgments about the truth of the message. And this is both a text of the quality of the definition, what might it call it its theoretical scope, and also another example of how the theoretical definitions whether primitive and/or complex establish the foundation for synthesis.

The major reason why a theoretical definition is an idealist construction flows from its relationship to the operational measures or definitions that are discussed below. By specifying a relatively broad meaning, a conceptual property space has been marked out. Two important conclusions stem from this. First, there are obviously multiple operational definitions that can "fit" into the meaning space. As I suggested in my book, I can explore alternative operational definitions of power, reflecting different ideologies, and observe for which problems each has the most predictive power. In other words, with the meaning theoretically specified, we keep a "fresh" and paradoxically more "open" mind about alternative measures and their utilities.

Second, and equally important, the specification of a broad meaning, as is the case with a primitive term, means that even with a number of measures, not all of the intellectual property space has been filled. This not only eschews logical positivism but, as I argued in my book, creates a useful dialectic between the meaning and the measures, resulting in a constant refinement of the latter and only occasionally an abandonment of the former. This encourages a steady theoretical progress in the development of sociological knowledge.

There is of course a potential weakness in a theory containing many concepts with broad theoretical definitions, the problem of tautology. This issue is handled by the ordering of primitive and derived concepts, a subject that I do not touch upon in this essay.

An appreciation of why we need theoretical definitions is perhaps best understood if we observe what happens to a theory that does not have them. As a contemporary example of how meaning can be incorrectly interpreted from mea-

sures is to be found in much of the work in organizational ecology where inferences a bout meaning are abstracted from concrete measures of populations of organizations. Hannan and Freeman [1989] argue that the survival curve associated with organizational populations across time could be interpreted as having two phases, one in which the number of organizations grows faster than the demand and one in which the number of organizations declines more rapidly than the demand. The former period, they explain, is a period in which legitimacy is being established while the latter period occurs when too many organizations occupy the resource space. One of their more celebrated cases is the American brewing industry. First, to argue that there was growing legitimacy for beer drinking the first part of the 19th century is more than just a stretch of history. But, second, to ignore that a fundamental technological shift had occurred with the invention of a mass technology–pasteurization of beer — *and it is this that caused the overcrowding* — that led to a number of mergers and hence a rapid decline in the number of organizations is to miss the major dynamic that explains the evolution in the population size, their major interest. I submit that this is the more typical pattern to describe the evolution of organizational populations across time, at least those with mass technologies and consumer acceptance of a standardized product, a most important qualification since obviously some cultures have not exhibited this pattern in their brewing industries.

It is quite easy to criticize empiricism after the fact. But would the specification of a theoretical definition or beyond this a theory about how organizations evolve across time prevented this kind of error? This is less certain. What is clear is that the theory of how many organizations one needs relative to any population is a function of the existence of a mass technology, an idea established by Chandler [1977] if not Marx as well as the bulk of contingency theory in organizations.

3.2 Theoretical Definitions of Categories such as the Problematic Substance and Analytical Unit

One advantage of always searching for more basic theoretical definitions and thus the movement towards primitive terms is the recognition of how various literatures can be combined. Over time, people began to recognize that innovation itself involved the creation of new knowledge. I chose to use as an example of a definition of what is knowledge as a substance for two reasons. This idea is an elusive one, difficult to define and about which there is much debate both among philosophers and social scientists. Part of the elusiveness stems from the considerable variety of scientific disciplines, their methods of research and what they accept as evidence of what constitutes an established fact.

The second reason is that knowledge has become an extremely important part of a number of new literatures such as tacit knowledge and even new paradigms in the social sciences, e.g. in the study of organizations. The confusion about its definition is illustrated by the fact that many scientists do not think that they are creating or building knowledge with their research. At a broader level, even greater

are the distinctions in the kinds of hands-on experiences associated with craft or artisan knowledge as opposed to those associated with the professional training of a physician or lawyer. Again, we see here the use of adjectives demarking different kinds of knowledge and therefore its increasing centrality.

An important aspect of knowledge is its transmission via the process of collective learning or capacity building. What makes this aspect interesting is that it frequently involves *re*learning. It is this aspect that makes the transmission of knowledge somewhat "sticky" or difficult. Another reason for this difficulty is the importance of tacit knowledge that is uncodified knowledge. There is a growing appreciation that to create radical innovations one must learn the tacit knowledge of others within various research teams. Therefore, one theme in the study of knowledge is the successful transmission as represented by relearning not only at the level of the individual but for the study of innovation more importantly at the level of the collective.

Another problematic in defining knowledge are the philosophical debates between a realist and an idealist position (among others), which in the social sciences have centered on the concept of the social construction of knowledge, that is the importance of power in the designation of what is taught, learned and even what is accepted as a research finding.   All of these divergences and debates make the settling on a common definition difficult, some would say impossible.

If knowledge is so difficult to define, why do I attempt to provide a definition? Most books that discuss innovation and knowledge slide over this definitional problem. To me, this does not seem appropriate. It appears better to attempt to provide a solution even if it proves to be inadequate because the concept of knowledge and kindred ideas such as tacit knowledge, knowledge base, organizational learning, etc. are so important and could be derived from it. Furthermore, the theory about the determinants of innovation whether product, process/ technological or scientific discoveries starts with some measure of the availability of a diverse set of capacities, which implies some kind of knowledge and tacit knowledge. Furthermore innovation, which means something new has been added, implies the idea of some new knowledge, which in turn has feedback consequences on these capacities as well as changes the larger institutional context. In other words, the concept of knowledge links many literatures and important theoretical problems. If so, some attempt must be made to define what we mean by it.

Generally when defining the concept of a substance, one uses the angle of vision of the particular problematic. This is a critical qualification because with different angles one might select quite different theoretical definitions. Again, this is another argument as to why theoretical definitions are idealistic. As indicated above, my problematic is product and process innovation, on scientific discoveries that are the essence of new knowledge, on learning and especially collective learning, or capacity building at both the individual and collective level, and institutional change. Therefore, I propose the following definition.

> Knowledge = the capacity to reproduce or to replicate findings, products and processes.

This definition may appear to be quite static but *changes* in knowledge are defined by innovation in goods and services, scientific discoveries, and relearning. These changes add the dynamic quality because they result in additions to the total stock of knowledge. It is worth repeating that this knowledge stock exists at multiple levels ranging from the individual to the national state. The flows of knowledge reflect another aspect of change as they build knowledge communities or what are sometimes called communities of practice.

The static definition above calls attention to the importance of being able to replicate research results, which lies at the heart of what is accepted as established fact. In this context, Popper's idea of the ability to falsify an idea is an important contribution. Organizational ecologists describe organizations as having the capacity to reproduce the same product or provide the same service across time. In the work of Nelson and Winters [1982] this is referred to as having routines. The whole issue of quality control illustrates how important the idea of reproducibility is. And the economists' idea of economies of scale is built on a similar notion. In health services, professionals attempt to replicate healing processes whether by drug or by surgery or some other kind of intervention. In educational services, professors attempt to reproduce a certain level of understanding or ability to reason critically in their students. As is apparent, this definition can be applied to individuals as well as collectives. But again, it is worth repeating that what interests me are changes in the stock of knowledge as reflected in innovations, which means changes in routines or learning new ways of doing things.

With this definition of knowledge I can construct a fundamental equation of definitions:

Knowledge stock + collective learning = New knowledge or innovation

Furthermore, there are feedbacks from the creation of knowledge resulting in innovation on the knowledge base as reflected in various education programs and in various forms of collective learning, altering them in various ways.

A critical component in the equation above is the issue of collective learning or *rel*earning. Innovation requires the ability of a firm to recognize the value of new, external information, assimilate it, and translate it into the procurement and allocation of facilities, materials, components and knowledge. For the firm, we want to understand what facilitates learning internally. But internal learning is not the only source of innovation. The interaction with its environment determines a firm's access to a diversity of resources, whereas the learning enables the firm to transform these resources into innovations.

Learning is conceived as a process in which all kinds of knowledge are (re)combined to form something new. In our equation above, I suggested that the knowledge base plus collective learning leads to new knowledge or innovation. What makes it collective is the interaction of this learning with some form of communication between people or organisations that possess different types of required knowledge. Basically Lundvall's [1992] account of interactive learning clarifies: 1) how technological and market dynamics pressurise firms to innovate their processes and

products, 2) and how this innovation process impels firms to interact forward and backward in the production chain.

As can be observed one advantage of this extended discussion of the substance of knowledge and its broad definition is that is indicates a variety of ways in which hypotheses can be developed and various aspects of disciplines synthesized.

Units of analysis are equally difficult to define. A good example of the complications of the definition of what is an organization is to be found in Hage [1980]. The problem is to recognize that many organizations are really multi-organizations, that is they are composed of separate products as in a chemical company such as DuPont, e.g. nylon, rayon, gortex, etc. or services as in an hospital such as Johns Hopkins, e.g. cancer service, cardiology service, eye clinic, etc. The critical defining characteristics are similar technologies and markets.

3.3   Theoretical Linkages and the Concept of Social Causality

I would suggest that an even stronger case for idealism can be made for the concept of causality, especially social causality. Originally in TPTC, I avoided the concept of causality because of the controversy within the philosophy of science. Much of this debate would probably disappear if we just accepted causality as an idealist concept that is being employed because it provides a useful mental way of thinking about theoretical rationales or the reasons why we assume some multi-variate hypothesis is worth examining. In TPTC, I argued that the theoretical linkage was a rationale as to why the connection between two or more concepts was plausible. Much later, Barbara Meeker and I [1988] decided to tackle the problem of social causality and wrote a book about this idea building upon the monumental work of Bunge.

The framework of causality assumes that we can specify some initial conditions and then a series of processes that connect causes to effects. It entertains the possibility of multiple processes that can connect different or even the same causes with a singular effect (or multiple effects) or multi-finality. What is useful about the concept of social causality is that it requires us to move on to specify processes in our research, something that is sorely needed today in sociology.

Although functionalism is much criticized, it is a useful place to begin to construct some argument about cause because of its strong assertions about needs. As an example of such a causal explanation, Cathy Alter and I [1993] constructed the following three premises in a functional form of explanation:

1. Inter-organizations networks that provide services to clients need to be coordinated for effectiveness of care.

2. Coordination of client care can be accomplished via sequential care, reciprocal care, and team care.

3. As the scope (multi-problem) and duration (chronic care) of client care increases, then the coordination must shift from sequential care to team care to maintain effectiveness.

The initial conditions are that it applies to only a certain kind of an inter-organizational network, one that is a delivery system. The first premise is a functionalist one about the *need* for coordination. Here we have a perfect example of an idealistic form of reasoning. We postulate needs although obviously we can not empirically measure their existence. The second premise indicates that coordination can be achieved in a variety of ways, which is the typical problem in social explanations and why there are multiple processes towards the same end or effect. We have already observed this in the iron cage model and it is also implicit in our discussion of complex theoretical definitions. Finally, the third premise specifies under what conditions one or another method of coordination is most appropriate. Bergman [1957], who was of the Vienna School, argued that theories needed an action premise. This third premise is my example of what an action premise would look like in sociology. A test of this line of reasoning indicated that as the difference score between the scope and duration of the client and the emphasis on team care increased, then the effectiveness of the delivery system declined. What is left out of this explanation are all the social processes that help explain the connection between team care for multi-problem clients In other words, the causal processes are under-specified. This example also illustrates how the placement of a theoretical rational in the form of a major and minor premises or a syllogistic format is of little use.

In sum, primitive terms and social causality are essentially mental operations that we impose on some social reality. It certain does mean that either our choice of terms or of causal explanation are in any sense correct or have some foundation in reality. Instead, we find these mental operations helpful because they provide considerable flexibility in thinking about the problems that we want to explain.

3.4   Theoretical Definitions of the Limits

Since some time was spent on the example of the definition of the substance of knowledge, little time need be spent on the problem of defining the limits of our hypotheses. As indicated, the limits involved specifying adjectives and levels of analysis, are akin to how one defines knowledge or an organization. The same mental operations are involved.

4   OPERATIONAL DEFINITIONS, LINKAGES AND EMPIRICISM

Anyone who accepts a hypothetic-deductive model of science also accepts the idea of the need for empiricism. But rather than perceive all research as empiricist, I have suggested that a theory has different elements, some of which not empirical and others are. After all what is a fact? It is something that becomes relevant in the context of a theory, that is its connection to other concepts or ideas. Otherwise, it is part of the haze in perception. In TPTC, I argued that were two empirical elements in a theory: operational definitions and operational linkages. In philosophical terms, the operational definition can be called a real definition. However,

as I have suggested above, given a broad theoretical definition that verges on a primitive term, then there are multiple operational definitions that can be logically derived from the theoretical one. This is almost turning logical positivism on its head. But the presence of multiple operational definitions is a desirable property given this stage of theory development in sociology where there is little consensus on either definitions or measures of concepts. Furthermore, even if a broad operational definition that covers most of the property space of the theoretical definition is the goal, this is not likely to be achieved even in the next few decades and especially for important ideas such as centralization of society. Indeed, what is interesting is that there are new processes of power being created such as the role of lobbyists in the U.S. case. If one accepts this idea than the theoretical definition remains stable but not the operational ones.

The operational linkages are the equations that connect the operational definitions. Since my writing TPTC, there has been some improvement in the kinds of equations that are considered in sociology. Log-linear has been added to regression analysis and perhaps more importantly, regardless of its raw empiricism, organizational ecology does explore a number of interesting issues in the specification of the functional form of the equations that test its ideas.

The philosophical issue that is underpinning this entire intellectual exercise is: What is the relative weight of rationalism and empiricism in the development of operational definitions and linkages? Above, I have made an argument as to why idealism seemed to be most appropriate for theoretical definitions and linkages. Here, I want to make an opposite argument as to why operational definitions and linkages are best constructed via observational approaches rather than mental operations.

4.1   Specifying Operational Definitions for Continuous Concepts

The fundamental reason why an empiricist approach is necessary is that it is very difficult to decide *a priori* on the best measures and the best equations for connecting them. There are just too many different possibilities. At the theoretical level, the many possibilities are limited because of broad definitions and rationales such as causal reasoning. But at the empirical level, one needs to explore the variety of possibilities to find out what works. The theoretical level sets limits on this search so that not everything goes. The classic example of the differences between the theoretical definition and its various multiple indicates is the relationship between status or prestige in society and its various operational indicators such as income or education [Leik and Meeker, 1975, 29].

One of the major theoretical errors in the social sciences is that the consequences of some variables are used to indicate the presence of the variable itself. Take the concept of organizational learning. Economists observe rising levels of productivity and then assume that it is a measure of learning. But it is not; it is a *consequence* of learning and could be caused by many other things.

For example, suppose we define organizational learning as the adoption of new

work routines, something that is quite consistent with current organizational theory. Then the operational definition could be changes in either strategies or tactics and the number of these can be counted. Recently I measured this in a study of changes in women's health behavior in developing countries. This is a quite interesting performance measure of NGO effectiveness. It also allows sociologists to escape the pitfall of treating productivity as the only legitimate measure of effectiveness. In this instance, the numbers of strategic and tactical changes in five separate intervention arenas in each of 14 NGOs were measured and this was then correlated with changes in women's health behavior over a two year period of work in a variety of communities in Nicaragua. The amount of learning was significantly correlated with a number but not with all of the 13 specific indicators of safe motherhood and child survival. Furthermore, there was experimentation with different ways of counting the number of strategies and tactics, including the weighting of the strategies. However, in the end, from a strictly empirical perspective, the sheer count of the number of tactical changes was the best operationalization of organizational learning in this instance. This illustrates how, even though the basic theoretical definition remains the same, the specific operational indicators and definition can shift. Indeed, it would have been difficult to think *a priori* about what might work and what might not work. In fact, clearly I made a mistake in my first thinking about this. But via trial and error, empirically, I was able to locate empirical indicators that "work". And that is of course exactly why one needs to be an empiricist. *But note that that is also the reason why one cannot be a logical positivist.* What "works" in one situation (unit of analysis, sector, country, perspective) does not apply in another and therefore the theory requires a broad and/or complicated theoretical definition for diverse operational definitions appropriate for different analytical units and for disparate theoretical perspectives.

As an example of how complex the operational definition can be, consider the following and rather important case of developing an operational definition for the radicalness of process innovation. The theoretical definition is the amount of change in the throughput of the manufacturing processes. The real issue is how to measure this. With Paul Collins and Frank Hull [1988], we took the Amber scale, which measures the kind of a machine on a four point scale that varies from stand alone machines to assembly-line, to process flow to automated machines. In a survey that we conducted in a number of New Jersey plants building upon earlier work of [Blau *et al.*, 1976], we measured how much change had occurred in the proportion of machines in the manufacturing processes of the close to 50 plants that were still open. Thus for each plant we have measured over a 14 year period how much change in the proportion of machines at each level of the Amber scale.

But how to quantify the idea of radicalness? We decided that the appropriate method was to treat each level on the Amber scale as a hyper-geometric function, that is the proportion of stand alone machines would get a weight of one, the assembly-line proportion would get a weight of three, the process flow proportion

a weight of nine, and the automated machine proportion a weight of twenty-seven. Obviously, and reflecting the fact that this is an empiricist approach, we tried different weighting schemes and found that a hyper-geometric function performed better than a simple square. For those that find this operational definition complicated, remember that physics is complicated. So why should sociology be less so?

4.2   Specifying Operational Definitions for the Problematic Substance and for Analytical Units

The reader might find it strange that operational definitions might be used for substance and especially analytical units. Yet, the latter issue goes to the heart of the question of observables. In the theoretical definition of knowledge, I suggested that it was the replication of tasks at the individual level or of activities at the organizational level. One can measure this quite precisely by examining the margin of error obtained at either the individual or the organizational level. At the latter level, quality control measures indicate the precise error level. Furthermore, this illustrates how helpful theoretical definitions are for finding various operational definitions.

One of the major limitations of sociology has been that social reality is not easily perceived. We can "see" individuals but we do not see organizations or societies except for some of their physical attributes such as plants, machines, national parks and tanks. Much more difficult is to perceive the workings of either an organization or a society. Quite elaborate measurement procedures have to be developed (for organizations see the discussion of the measurement of structure in [Hage, 1980]). Buried in these various measurement procedures is the importance of isolating from individuals those parts of them that belong to organizations or to societies from those parts that reflect their individual characteristics. This is difficult and many sociologists do not understand the importance of this kind of methodology and frequently confuse the different levels of analysis. Indeed, this is the origin of methodological individualism.

What is more difficult for me to understand is why social scientists have problems in perceiving the collective performances of various social entities. Innovation in products and services is a collective property, a performance of organizations that is not reducible to individual acts because the collective sum or performance is greater than the individual parts or performances. Societies or at least governments go to war as has been recently demonstrated in Iraq. This is a collective act with profound and enormous consequences. Furthermore, while many attempt to reduce the explanation of this act to a single individual such as President Bush or a small group of presidential advisors, the fact remains that a large number of decision-makers were involved in this decision process including the importance of public opinion that wanted some action after 9/11. And this is the great danger of not having an operational definition for large social collectives such as societies or organizations. We reduce our understanding to the acts of single individuals

and thus deny not only social reality but more critically its complexity.

4.3   Fitting Operational Linkages

Perhaps the best argument as why operational linkages are most appropriately specified by empirical methods and thus belong in the empiricist tradition is provided in several of the mathematic books that have been written for sociology ([Coleman, 1964]; [Leik and Meeker, 1975]). In [Leik and Meeker, 1975, 29], they observe the different amounts of variance explained (R^2) with different equation forms including exponential, log, and power function in the relationship between status and income or education. Another good illustration of the need to consider alternative forms of equations is found in the many studies of diffusion. Among other possibilities are waxing, exponential, waning exponential, linear logistic, gompertz and cohort (see [Leik and Meeker, 1975, chapter 7]). An even more complicated equation form is the Poisson distribution, which assumes rare events [Coleman, 1964]. The important point is that there are different theoretical implications to these various equation forms, and lead one to rethink their assumptions about limits and conditions, a topic that we touched upon above.

One of the more minor issues in the intersection between philosophy and sociology is the assumption that one makes about the nature of the social world and how best it should be described. Unfortunately, in much of sociology the implicit assumption is that the social world is linear, very much like in the pre-Columbian days when Europeans assumed the physical world was flat. If the simplest equation discovered in the 17th century is non-linear why should social reality be any simpler? Yet, the bulk of the training about operational linkages is build upon regression analysis. While log-linear is an improvement it is still far from the kind of sophisticated operational linkages that one needs to describe the social world.

The vision of a flat social world is aided by two important limitations in most research designs: (1) an absence of data collective across time; and (2) an absence of data collected in different contexts, especially national or cultural ones. Cross-comparative, cross-temporal research designs are more likely to allow one to appreciate the non-linear character of the social world. It is not accident that many of the examples used in both Coleman [1964] and Leik and Meeker [1975] involved time series data.

Organizational ecology [Hannan and Freeman, 1989] has raised the bar on the mathematical sophistication of operational linkages. In various books, they have discussed the pros and cons of particular fits. But it is obvious that these are all empirically driven. Perhaps the extreme case of this is the current practice of estimating the curves for organizational age. In a number of articles, researchers employ a split-wise function, which simply means that they observe the actual distribution and then fit the curve to this. Clearly this means that there will be a different function for each study as a consequence. While operational linkages can be this specific, one should strive for a more general approach that at least applies to a number of different organizational populations. Hadden [Hadden and

Hage, 2004] in working on this problem discovered that the age inverse fitted well one population of organizations, namely shoe companies. It remains to be seen whether it can be generalized to others.

As should be readily apparent in these examples, operational definitions and linkages require considerable manipulations of the data, looking for one that works. It is obvious that the operational linkage is connecting the operational definitions of three or more variables so that one is playing with all these empirical problems simultaneously. It should also be apparent that this is much more difficult to think about mentally then the broad theoretical definitions and the causal explanations.

5   CONCLUSIONS

In my discussion of the intersection between theory construction and philosophy, I have tried to indicate not only how can sociology learn from philosophy but also how philosophy might learn or least sense new opportunities in sociological theory and especially the special problems associated with theory construction. My definition of a theory appears to move substantially beyond what is normally discussed in the philosophy of science. I have suggested that we can recycle the famous debate between the English empiricists and the continental school of rationalists by recognizing that different parts of a theory are best constructed by different approaches, sometimes mental operations and sometimes data manipulations. Indeed, in a footnote, I have suggested the real question is what degree of *a priori* creativity is involved in our concept formation and hypothesis generation. We should encourage leaps of imagination.

Given this approach of specifying elements in a theory and then suggesting which school is most relevant, it is apparent that my formula for the construction of knowledge is two-thirds mind and one-third data or to put it other terms, in thinking about new ideas, I side more with the continental school than the British one. Because of the stronger emphasis on empirical approaches in at least American sociology, there has been a tendency to readily accept a linear view of social reality.

One very important qualification in the various examples provided above, which I am sure that empiricists would immediately argue, is that the sequence of ideas as I am reporting them may not be the sequence in which they occurred. It is entirely possible that I observed certain patterns, conceptualized them with a name and then proceeded from there. But while I admit this, I have also tired to indicate some mental processes or patterns of thought were this would appear to be less the case. Regardless, the importance of playing with ideas seems to me to be the heart of the insight of rationalists and not to be discounted.

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CAUSAL MODELS IN THE SOCIAL SCIENCES

James Woodward

1 INTRODUCTION

Causal modeling is an umbrella term for a variety of techniques that are used to make causal inferences from statistical data. These techniques take many different forms and have a number of names: they include, for example, regression, simultaneous or structural equations, factor analysis, the use of path models, and much else as well. Such techniques are widely used in the social sciences, particularly in disciplines such as sociology and political science that lack powerful and generally agreed upon formal theories. They are also widely employed in certain areas of psychology and in some bio-medical contexts, such as epidemiology. Causal modeling techniques are used for the causal analysis of experimental data, but much of their interest stems from the fact that they are also used to make causal inferences from non-experimental or "observational" data.

Causal modeling techniques raise a number of philosophical and methodological issues that should be of interest both to philosophers of science and to users of those techniques. Some of these cluster around the notion of causation itself. The techniques claim to (sometimes) reliably deliver causal conclusions from statistical information, but in what sense of (or according to what account of) "cause" are such conclusions causal? Do any of the standard philosophical theories of causation capture or illuminate the sorts of causal relationships that causal modeling techniques (supposedly) discover? What is the difference, if any, between the use of such techniques to reach causal conclusions and their use for other purposes such as mere "description"? Closely associated with these are issues that are more epistemological or methodological in nature, having to do with confirmation, evidence, and model selection. Under what conditions, if any, (and for what sorts of data and background assumptions) do causal modeling techniques lead reliably to causal conclusions? What exactly is the logic of such inferences? Can causal conclusions be derived just from statistical information and if not, what sorts of additional background assumptions are required?

2 REGRESSION

2.1 Bivariate Regression

Let us begin with a simple linear regression model involving just one independent variable. Suppose that we have a population P of n individuals, and that we

are able to measure the values taken by two variables, X and Y, for each of these individuals. Let $(x_1, y_1), (x_2, y_2), \ldots (x_n, y_n)$ be these measured values. In a bivariate regression model, it is assumed that a linear relationship holds between the values of Y, X and a so-called "error" or "disturbance" term U for *each* of these n individuals:

(2.1.1) $y_i = a + bx_i + u_i$ for $i = 1 \ldots n$.

Or, more compactly:

(2.1.2) $Y = a + bX + U$

Here a and b are fixed coefficients and u_i is the value of U for the ith observation. For example, Y might represent the measured heights of plants in a certain population and X the amount of fertilizer they have received. Alternatively, X might represent a subject's years of schooling and Y his annual income at age forty. The natural interpretation of (2.1.1)–(2.1.2) is that the coefficient b tells us what sort of change in the value of Y will, on average, be "associated" with a given change in the value of X within this population. (Here "associated" means simply that for a non-zero value of b, the values of X and Y are correlated, and not that any particular causal interpretation of this relationship is warranted.)

We will address issues about the status of U in more detail below, but for the moment we may think of U as having several possible interpretations: U may reflect either the presence of "measurement error" in Y, the influence of various other variables on Y besides X, or, more controversially (see below— Section 2.6.4), the presence of some stochasticity or indeterminacy in the relationship between X and Y, or some combination of all these. In any case, in contrast to X and Y, the individual values of U are assumed *not* to be directly measured; instead they are an unknown source of variation in the values taken by Y. It also is assumed that U is a random variable with a well-defined probability distribution and hence that Y is a random variable as well. The presence of U in equation (2.1.2) thus results in a spread of values for Y for fixed values of X. The regression equation for Y on X is then defined as the equation which gives the mean or expected value of Y for the various values of X. In the specific case of (2.1.2) the assumed relationship between X and Y is linear, but, if desired, one may use other functional forms to capture the relationship between X and Y, with the same general understanding of the resulting regression equation as giving the expected value of Y for the various values of X.

Suppose that we wish to estimate the values of the coefficients a and b in (2.1.2) from measurements of the values (x_i, y_i) in a sample consisting of just some of the individuals in the population P. In other words, we wish to infer to the values of these coefficients for the entire population from this sample. The usual practice is to choose estimators a^* and b^* for a and b, according to the method of least squares; that is to choose a and b such that the quantity

(2.1.3) $Q = \Sigma(y_i - a^* - b^* x_i)^2$

is minimized. Geometrically this amounts to choosing the regression equation in such a way that it "best fits" the points (x_i, y_i) where the criterion for best fit is the minimization of the sum of the squared vertical distance of these points (expressed by (2.1.3)) from the regression line. A standard result is that if the error term U satisfies certain distributional assumptions — to be described in a moment — the estimators a^* and b^* will have various desirable properties (in particular they will be best linear unbiased estimators or BLUE) and will be given by

$$(2.1.4) \qquad\qquad b^* = S_{xy}/S_x^2$$

$$(2.1.5) \qquad\qquad a^* = \bar{Y} - B^* \bar{X}$$

where $S_{xy}, S_x^2, \bar{X}, \bar{Y}$ are respectively the sample covariance of X and Y, the sample variance of X, and the means of X and Y.

The distributional assumptions about U that are sufficient for a^* and b^* to be best linear unbiased estimators are as follows:

> (D) (a) $E(U_i) = 0$, (b) homoscedasticity or constancy of the variance of U across different values of X: $V(U_i) = \sigma^2$, (c) statistical independence or absence of auto-correlation in the error term, (d) statistical independence of the error term and the independent variable — that is, X and U are independent.

The general picture is thus that if we are willing to make certain assumptions about the existence and characteristics of a functional relationship between X and Y and if we are also willing to make certain assumptions about the error term U, we may derive reliable estimators for the coefficients a and b from facts about observed or measured values of the variables X and Y. Thus, relative to these assumptions, regression gives us a data-driven, or bottom-up discovery procedure for estimating the values of these coefficients from statistical information (in the form of information about the covariance of X and Y and the variance of X) about the observed distribution of values of X and Y .

2.2   Multiple Regression

So far our focus has been on the relationship between a dependent variable and a single independent variable. Multiple regression involves a generalization of the ideas just described to the relationship between a dependent variable Y and a number of independent variables $X_1 \ldots X_n$. Focusing again on the linear case, the assumed model is now

$$(2.2.1) \qquad y_i = B_0 + B_1 x_{1i} + B_2 x_{2i} + \ldots B_k x_{ki} + u_i, i = 1, 2, ..., n$$

where y_i is the observation for the value of Y for the ith individual and $x_{1i}, x_{2i}, \ldots, x_{ni}$ are the observed values of X_1, X_2, X_n for that individual and u_i the value of

the error term. As before, B_o, B_1, B_n are assumed to be fixed coefficients. We may write (2.2.1) in matrix notation as

(2.2.2) $$Y = XB' + U$$

where

$$\begin{vmatrix} Y \\ U_2 \\ \bullet \\ \bullet \\ Y_n \end{vmatrix} \quad B = [B_0, B_1, \dots, B_k]$$

$$U = \begin{vmatrix} U_1 \\ \bullet \\ \bullet \\ \bullet \\ U_n \end{vmatrix} \quad \text{and } X = \begin{vmatrix} 1x_{11}X_{12} \bullet X_{1k} \\ 1 \bullet \bullet \bullet \bullet \\ \bullet \bullet \bullet \bullet \bullet \\ 1X_{n1} \bullet \bullet X_{nk} \end{vmatrix}$$

As before, if one makes assumptions like D regarding the distribution of the error term, the best linear unbiased estimator for the coefficients B_i will be (in close analogy with)

(2.2.3) $$B^* = (X'X)^{-1}X'Y$$

where X' is the transpose of X.

One may think of the observations on $Y, X_1, \dots, X_k$ as points in a $k+1$ dimensional space and the choice of the regression equation as a matter of finding a k-dimensional hyperplane that best fits this scatter of points. The natural interpretation of the equations is now that each of the coefficients B_i describes the average change in the value of Y that is associated with a unit change in the value of X_i, under conditions in which the values of the other independent variables remain constant

2.3   Data Summary, Prediction, and Causal Inference

So far we have merely described an algorithm for fitting a linear equation to a body of data, with the resulting equation representing an "association" between the variables figuring in it, but with no suggestion that this association is necessarily "causal". Nonetheless regression equations are often assumed to represent (or are interpreted as representing) causal relationships. In fact, when interpreted causally, such equations have (what we might call) a "natural" causal interpretation, according to which changes in the values of the variables on the right hand side (r.h.s.) of the equation cause changes in the values of the dependent variable,

but in which there are no causal relationships among the r.h.s variables themselves and no "reciprocal" or feedback causation running back from the dependent variables to the r.h.s variables.

The use of regression equations to represent causal relationships raises a number of issues. First, what does "cause" mean or commit us to when used in such a context? When should we regard a regression equation as a "correct" representation of a causal relationship as opposed to an "incorrect" representation of a causal relationship or, alternatively, as not a representation of a causal relationship at all? Second, what sort of evidence and/or additional background assumptions are required if one is to reach distinctively causal conclusions on the basis of regression techniques, given that one may fit a regression equation to a body of data in circumstances in which no causal interpretation is intended?

A useful point of entry into these questions is provided by David Freedman [1997]. Freedman distinguishes [116] three possible uses or interpretations of regression equations:

(i)   **Data Summary**. To summarize or represent a body of data consisting of measurements of the values of the variables Y, $X_1,.. ,X_n$

(ii) **Prediction**. To predict the value of a dependent variable Y from a set of independent variables $X_1,.. ,X_n$

(iii) **Causal Inference**. To predict the value of Y from an *intervention* that changes the value of one of the independent variables $X_1, \ldots, X_n$

Data Summary. As we have seen, a regression of Y on $X_1 \ldots X_n$ by definition describes the average or expected value of Y associated with different combinations of the values of the variables $X_1 \ldots X_n$. Given an arbitrary body of data we can always use the estimators given by (2.2.3) to find such an equation and in this sense the equation will automatically represent or summarize a pattern in the data. Obviously, however, it does not follow from this possibility that the resulting equation describes a causal relationship between Y and $X_1, \ldots X_n$ or even that a similar pattern will hold among the values of these variables when measured on some other body of data. For example, according to the *New York Times*, there is a correlation between the recent history of payouts P of certain Nevada casinos and the phases of the moon M (Cf. [Woodward, 1998]). Hence if one regresses P on M, the resulting coefficient will be non-zero. However, there are good reasons both to doubt that this relationship is causal and to doubt that a similar relationship would hold for other casinos or time periods.

Prediction. As indicated by (ii) above, a second possible use of a regression equation is to predict the value of Y from the values of the independent variables $X_1 \ldots X_n$ for other sets of data besides the data set on which the equation was originally estimated. As Freedman observes, successful prediction does *not* require that $X_1 \ldots X_n$ cause Y. Instead what it requires is that the process that generates the data on which the regression equation is estimated be *stable* across space

and time — that is, that a relevantly similar process also generate the values of $Y, X_1 \ldots X_n$ for the new data set for which we wish to make predictions. Suppose, for example, that Y and X are both effects of a common cause Z with no direct causal connections between X and Y. If we regress Y on X (omitting the causally relevant Z as a regressor and ignoring the consequent violation of the distributional assumptions D), we may use the resulting equation to successfully predict the average value of Y from the observed value of X on new data provided that the process that generates this new data behaves in a similar way to the process that generated the original data on which the regression equation was estimated — that is, assuming that the new data is generated by the common cause Z with similar functional relationships between Z and X and Z and Y. In this case, we successfully predict the values of Y from the values of X via regression even though, *ex hypothesi*, X does not cause Y. As this example illustrates, whether a relationship is exploitable for purposes of successful prediction on a new body of data from that on which it was originally estimated (whether the relationship is *projectable* in the sense of [Goodman, 1955]) and whether it represents a causal relationship are two different matters.

Causal Inference. Freedman identifies causal inference with the prediction of the value of the Y given an *intervention* that changes the value of one of the independent variables X_i. His thought seems to be roughly this: a regression equation like (2.2.2) will represent a causal relationship if and only if were one to intervene on one of the independent variables X_i (that is, manipulate it in the right way — see below), the value of Y would change in just the way claimed by (2.2.2) — that is, a hypothetical experiment that changes X_i by amount dX_i will on average change Y by amount $b_i dX_i$. In this sense, if the equation correctly describes a causal relationship, it will provide information that is potentially relevant to the prediction and control of Y. Following standard terminology[1], we may say that when a regression equation correctly describes how a dependent variable will respond to an intervention on or manipulation of one of the independent variables in the equation then the equation is *invariant* under this intervention; thus an equation that correctly describes a causal relationship will be invariant under at least some interventions (on the independent variables occurring in the equation). A model having this feature is sometimes said to be *structural* — the idea being that it represents a causal structure, as opposed to a mere pattern of correlations. It is important to note that a regression equation may correctly describe how a dependent variable will respond to some range of interventions on values of the independent variables in the equation (for some population of interest) even if the equation does not correctly describe the response of that variable to interventions outside of this range. Invariance under interventions is thus a relative matter: a generalization may be invariant under some interventions (and thus qualify as causal) but not under others (see [Woodward, 2003, Ch. 6] for additional discussion).

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[1] See, e.g., [Woodward, 1999; 2003] for additional discussion and references.

2.4   Connection to Manipulability Theories of Causation

Within the philosophical literature, the idea that causal claims have to do with the successful prediction of what will happen under interventions is obviously related to manipulability theories of causation, such as those developed by Gasking [1955], Collingwood [1940], and von Wright [1971] This is also an idea with a long history in the econometrics and causal modeling literature and among theorists of experimental design. For example, in their influential text *Quasi-Experimentation*, Cook and Campbell [1979] write, "The paradigmatic assertion in causal relationships is that manipulation of a cause will result in manipulation of an effect". Similarly, the econometrician Gary Orcutt [1952] writes, "we see that the statement that Z_1 is a cause of Z_2 is just a convenient way of saying that if you pick an action that controls Z_1 you will also have an action that controls Z_2" [307]. Broadly similar sentiments are also endorsed by Pearl [2000] and Woodward [2003].

The intuitive idea behind this interventionist construal of causal claims is that a regression equation may provide an accurate summary of a pattern of correlation in the data or may be used to predict the values of some dependent variable in new data (i.e., may fall into categories (i) and (ii) above), and yet fail to correctly describe what change in a dependent variable would result under a hypothetical experiment in which the independent variables are manipulated. To take an example from Cartwright, [1983], suppose there is a correlation between the amount A of life insurance a subject purchases and longevity L. Regressing L on A will thus yield a positive coefficient. Obviously, however, it does not follow from this observation that a subject can manipulate how long he or she lives by altering the amount of life insurance he or she purchases. Instead, it seems much more likely that the correlation between L and A arises because both are effects of some common cause or set of such causes — for example, it may be that people of higher social economic status (*SES*) both tend to purchase more life insurance and to live longer. If so, and if there is no causal link from A to L, one would expect that if a substantial sample of people (of varying *SES*s) were to be randomly assigned different amounts of insurance, the correlation between A and L in this sample would disappear, reflecting its non-causal status.

It is important to appreciate that the interventionist interpretation is intended as an account of what researchers are committed to in making causal claims and not as an account of how the truth of such claims must be established. In particular, the interventionist account does *not* claim that the only way of establishing the truth of causal claims is by actually performing interventions. Instead, the idea is that when we infer to causal conclusions on the basis of non-experimental or "purely observational" data, we should think of ourselves as trying to infer, on the basis of non-experimental data, what the outcome would be if (contrary to actual fact) we did carry out certain interventions. For example, if we were to decide, on basis of purely observational data, and using regression or some other causal modeling technique, that there was no causal link from A to L in the example above, but there was a causal link from SES to L, then we are committing ourselves

to the claim that if we were to intervene to change A there would be no change in L and that for some range of interventions on subject's SES, this would be associated with changes in their longevity. Put somewhat differently, when we infer to a causal conclusion on the basis of non-experimental data, we are claiming to be able to predict or simulate what the results of a hypothetical experiment would be by engaging in some appropriate procedure of statistical control or adjustment rather than by actually performing a physical manipulation. That is, to the extent that they deliver causal information, the statistical procedures for calculating the partial regression coefficient B_i are to be understood as predicting what would happen to Y in an experiment in which one is able to physically manipulate X_i. The key question then becomes: under what circumstances is such an inference reliable?

Although intuitively appealing, this interventionist idea faces a number of difficulties. To begin with, it inherits various problems that surround traditional manipulationist theories of causation. These include the worry that any such theory must be "circular" in an unilluminating way: since the notion of manipulation/intervention is obviously a causal notion, how can we use this notion to elucidate what it is for a relationship to be causal? There is also the worry that the notion of an intervention must inevitably be a highly anthropomorphic notion — unavoidably tied to the notion of *human* manipulation — and as such unsuitable for understanding the notion of causation, since causal relationships are (it is commonly supposed) "objective" relationships that hold independently of the possibility of human intervention. Finally, there is a more specific worry concerning exactly how the notion of an intervention should be characterized. For example, in the case of the $A \leftarrow SES \rightarrow L$ system considered above, with SES a common cause of A and of L, if we manipulate A by manipulating SES, then there will be a corresponding change in L, even though, by hypothesis, there is no causal relationship between A and L. We need a way of characterizing the notion of an intervention that excludes this sort of possibility. We will return to these issues in sections 3.4.1–2 below, where we will see that the last issue, at any event, can be given a precise resolution. For the present, it will be useful to work with an intuitive idea of an intervention on X with respect to a second variable Y, understood as an idealized manipulation of X of the sort that would be appropriate in a well-designed experiment to test whether X causes Y. This characterization suggests that, among other requirements, the manipulation I must not affect or be correlated with other causes of Y besides those that lie on the causal route (if any) from I to X to Y — that is, an intervention on X with respect to Y should be of such a character that Y is changed (if at all) only through the change in X produced by the intervention and not in some other way. This requirement is violated in the example considered above, since the manipulation of A occurs through the manipulation of SES, which is a cause of A that affects L via a route that does not go through A. Applied to an equation like (2.2.2), an intervention on, e.g., X_i should affect only X_i among the independent variables and should not alter the values of either the other independent variables or the error term. This

seems to capture Freedman's idea about the connection between intervention and causation in the passage quoted above.

2.5   The Role of the Error Term

Even in advance of a more precise statement of the interventionist idea just adumbrated, we can usefully explore some of its implications for issues having to do with the causal interpretation of regression equations. The first has to do with the status of the distributional assumptions D concerning the error term. A number of writers, including both philosophers and statisticians/social scientists, have claimed that the satisfaction of some or all of these assumptions is either necessary or sufficient or both for a regression equation to represent a causal relationship or to have a coherent causal interpretation. Assumption (d) which asserts that the error term U must be independent of or uncorrelated with the independent variables in the equation — hereafter the *uncorrelatedness assumption* — has been taken to be particularly crucial in this regard. For example, Gurol Irzik [1996, 255] writes

> A crucial assumption of causal modeling is that an error term is uncorrelated ... for each equation with the causes [i.e. independent vararibles] in that equation. All [causal] models ... endorse this assumption one way or another.

Similarly, the social scientists James, Mulaik, and Brett [1982, 45], quoted in Pearl, [2000, 135]) hold that unless the uncorrelatedness assumption is satisfied, "neither the equation nor the functional relationship represents a causal relation". Similar claims are made by Papineau [1989, 401] and Cartwright [1989, 24].

In assessing these claims, it is crucial to separate issues having to do with the conditions under which a regression equation may be taken to represent a causal relationship or to make a correct causal claim (and how "causal" in this context is to be understood) — issues of *causal interpretation* as I have been calling them — from epistemological issues concerning the conditions that are relevant to the reliable estimation of the coefficients in the equation. It is arguable (cf. [Pearl, 2000; Woodward, 1999]) that the conditions (D) are relevant to the latter problem but that their satisfaction is neither necessary nor sufficient for a regression equation to have its natural causal interpretation. In other words, conditions like D are relevant to how we find out what the coefficients in a regression equation (or more generally a causal model) are. They are conditions that are sufficient, although as noted below not necessary, for reliable estimation, but their satisfaction is neither necessary nor sufficient for a regression equation to be interpretable as making a causal claim.

One way of seeing that satisfaction of D is not sufficient for a regression equation to correctly describe a causal relationship is via the following consideration. Suppose that we are given a body of data consisting of a number of measured values for the variables $X_1 \ldots X_k$. We can always arbitrarily pick one of these variables

— say X_j — and regress it on the remaining variables, taking the coefficients b_i in the resulting regression equation to be given by their OLS estimators. If we then define the error term U as the "residual" $U = X_j - \Sigma_{i \ne j} X_i$, it follows just as a matter of mathematics that U will be uncorrelated with the independent variables in the resulting equation. This is easiest to see in the simplest possible case — bivariate regression. Suppose that

(2.5.1) $Y = aX + U$

Let us put aside issues having to do with statistical inference from sample to population and suppose that we know the relevant population covariances, variances, $E(XY)$, $E(XX)$ etc. Define a by its OLS estimator — i.e., $a = E(YX)/E(XX)$.
    Then
$$U = Y - aX$$
$$XU = XY - aXX$$
$$E(XU) = E(XY) - aE(XX) = 0.$$

Thus, when X is so defined, it must be uncorrelated with Y. Parallel reasoning shows that if we had instead regressed X on Y with error term V and define b as $E(XY)/E(YY)$, then Y and V would be uncorrelated in

(2.5.2) $X = bY + V$

It cannot be true that both (2.5.1) and (2.5.2) are causally correct.
    One implication of this is that the condition that the error term must be uncorrelated with the independent variables will only be a substantive, non-trivial constraint if we don't just define the error term as the residual, but instead have some independent understanding of what that term means. The natural characterization is to take the error term to represent the net effect of other *causes* of the dependent variable besides those explicitly included in the r.h.s. of the equation. It then becomes a substantive empirical question whether in any given case the error term, so understood, is uncorrelated with those r.h.s. variables.
    Even with this "causal" understanding of the error term, however, it is *not* true that satisfaction of the uncorrelatedness assumption is necessary for a regression equation to have a causal interpretation. To begin with, it is certainly possible to reliably estimate the coefficients in a regression equation if the uncorrelatedness assumption is not satisfied, provided the right sort of other information is available. To do this one uses other estimating techniques besides OLS. Indeed, as we shall see (Section 3.2), there are systems of equations in which, because of their structure, the uncorrelatedness assumption *must* be violated, but the coefficients in such equations can sometimes be reliably estimated by means of non-OLS techniques.
    Second, there is the more fundamental objection to the idea that the uncorrelatedness assumption is crucial for causal interpretability. This objection is that, as suggested above, satisfaction of the uncorrelatedness assumption has to do with the epistemological problem of determining what the values of the regression coefficients are, and *not* with the problem of whether the equation itself represents

a causal relationship. One advantage of an interventionist account of the causal content of a regression equation is that it brings this point out very clearly. Recall that on the interventionist account, a regression equation like (2.2.2) will correctly describe a causal relationship if, for some interventions on each of the independent variables, the value of Y will change in just the way represented by (2.2.2). Suppose that this is the case. Suppose also that the processes that generates the values of the error term U in the population on which we are estimating (2.2.2) is such that U is uncorrelated with the independent variables in (2.2.2).Then one may reliably estimate the coefficients in that equation by OLS.

Suppose next that the data generating process shifts so that U is now correlated with one or more of the independent variables. According to the interventionist interpretation of the causal content of (2.2.2), it will continue to make a correct causal claim as long as it remains true that (2.2.2) correctly describes the change in Y under interventions on each of the X_i. Since an intervention, by definition, changes only the variable intervened on, and none of the other independent variables in (2.2.2), including the value of the error term, it is entirely possible that (2.2.2) will continue to correctly describe the response of Y to (some) interventions on the variables X_i under this change in the distribution of U, although OLS estimates of the coefficients in (2.2.2) will of course now be biased. Indeed, as noted above, if (2.2.2) is a causally correct equation, we should expect that it will be invariant under some range of changes in the value and distribution of U — including some shifts from a situation in which U is uncorrelated with the independent variables in (2.2.2) to a situation in which it is correlated — just as we should expect a similar sort of invariance for (2.2.2) under changes in the values of or distribution of the values of the other independent variables in (2.2.2).[2]

Although satisfaction of the uncorrelatedness condition does not play a role in explaining what it *is* for a relationship to be causal, there is an obvious argument, explicitly formulated by Herbert Simon [1977], showing that, if we are willing to

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[2]A natural line of thought, among those who defend the claim that the uncorrelatedness assumption is essential for causal interpretation is that violation of the assumption indicates that some sort of mistake about causal structure has been made. Thus Cartwright [1989, 24] writes, "If the independent variables and the error term were correlated, this would mean that the model was missing some essential variables, common causes which could account for the correlation, and this omission might affect causal structure in significant ways". It is true enough that if U is correlated with one of the independent variables in a regression equation, this indicates that there is some omitted causal structure, which accounts for the correlation. It is also true, as Cartwright claims, that this *might* reflect the fact that the postulated regression structure is mistaken. For example, it might be the case, in a bivariate regression of Y on X, that X does not cause Y but that U and X have a common cause C which produces a correlation between U and X and between Y and X. In such a case, regressing Y on X and adopting the uncorrelatedness assumption will lead to a mistaken causal conclusion. However, violation of the uncorrelatedness assumption for some postulated regression structure does not automatically indicate that the structure is mistaken, rather than incomplete. In the above example, it might alternatively be true that both X and U are causes of Y, with $Y=aX+U$ correctly describing how Y will respond to an intervention on X even though X and U are correlated because of C and this fact is omitted from the model. Of course in this case OLS will lead to biased estimate for a, but, as argued above we need to separate issues about the conditions under which OLS estimates are unbiased from issues about the conditions under which an equation has a valid causal interpretation.

make certain other assumptions, the condition can play a crucial role in causal *inference*. Consider the regression equation

(2.5.1) $$Y = aX + U$$

Suppose that the error term, now interpreted causally (and not merely correlationally) to reflect the net effect of omitted causes of Y, is uncorrelated with X, and that X and Y are correlated — when we regress Y on X, the coefficient $a \neq 0$. Let us also assume the following general principle, sometimes called Reichenbach's principle or the principle of the common cause:

> (R) If X and Y are correlated, then (i) either X causes Y or (ii) Y causes X, or (iii) there is some common cause or set of common causes C for both X and Y.

Let us take these possibilities in reverse order. (iii) If the correlation between Y and X is due to some common cause or set of such causes C, then there will be a cause of Y — namely, the common cause C which is omitted from (2.5.1) and which is correlated with X (because C causes X). Since C contributes to U, U will be correlated with X in violation of the uncorrelatedness assumption. In other words, the uncorrelatedness assumption rules out possibility (iii). (ii) Suppose that Y causes X. Then again the other causes of Y (distinct from X) that are represented by U will be correlated with X, in violation of the uncorrelatedness assumption. The only remaining possibility is (i). It thus follows from (R), the assumption that U represents the other causes of Y besides X and that it is uncorrelated with X, and that X and Y are correlated, that X causes Y. Note also that it may be possible to rule out possibility (ii) on other grounds — for example, if X temporally precedes Y or if we have subject matter specific knowledge that Y cannot cause X, as when we reason that the condition of the wheat crop cannot cause the monthly rainfall, even if the two are correlated. Following essentially this line of reasoning, Simon concludes

> ... correlation is proof of causation in the two variable case if we are willing to make the assumptions of time precedence and non-correlation of the error terms. [1977, 98]

(R) is just one example of a general principle connecting causal claims and statistical information (about correlations, independence relations, etc.) that has been proposed by philosophers and social scientists; we will encounter other examples below (Section 4). Especially when combined with more domain-specific background knowledge, such principles can play a powerful role in causal inference. We should also be able to see from the above reasoning, however, that the uncorrelatedness condition (when the error term is interpreted causally) is far from innocuous and that, although this practice is common among social scientists, it is extremely naïve to adopt this condition as a sort of default — that is, to assume

that it holds in the absence of specific evidence to the contrary. In assuming this condition in a regression context, one is assuming, among other things, that there are no omitted common causes of X and Y in (2.5.1) and that the causal direction does not run from Y to X. Such an assumption should not be made in the absence of specific positive evidence to support it.

2.6 Comparison with other accounts of causation

2.6.1 Granger Causation

We can further deepen our understanding of what is distinctive about the interventionist treatment of causation, as applied to regression analysis, by contrasting it with some other well-known accounts of causation. The econometrician Clive Granger has proposed the following characterization of causation in the context of time series: Y_t causes X_{t+1} "if we are better able to predict X_{t+1} using all available information than if the information apart from Y_t had been used" [1969, 376]. Slightly more precisely, Granger proposes the following "causality definition:" Y_t causes X_{t+1} if $Pr(X_{t+1} \in A/\Omega_t) \neq Pr(X_{t+1} \in A/\Omega_t - Y_t)$ for some set A where Ω_t, is the set of all observable events available up to time t and $\Omega_t - Y_t$ is this set minus information exclusively in the sequence $Y_{t-j}, \geqslant O$ [1988, 10].

As it stands, this definition (with its reliance on "all observable events" and so on) is obviously not operational or useful in practice. Granger therefore replaces it with a characterization that refers to a more manageable subset of observational information, and also replaces the reference to the complete conditional probability distribution of X_{t+1}. This leads to the following proposal: if the variance of the forecast error of an unbiased linear predictor of X based on all past information is less than the variance of the corresponding prediction based on all past information except for past values of Y, then Y causes X. This in turn suggests a simple test for causality, which, somewhat imprecisely expressed, is this: regress X on a number of lags of itself and a number of lags of Y. If the coefficients on the lagged values of Y are zero, Y does not Granger cause X.

The basic idea behind Granger's theory is that causes should carry information about their effects: information about the cause should improve one's ability to predict the effect. The idea is very similar to Patrick Suppes' well-known probabilistic theory of causation [Suppes, 1970] — indeed, Granger's theory is in many respects just a quantitative version of Suppes' theory, and faces difficulties that are similar to those that have been lodged against Suppes' theory.

It seems clear that Granger's conception of causality and the manipulability conception are very different notions. To begin with, Granger causation is (at least if is to be a practically useful notion) relativized to an information set. X might Granger cause Y with respect to some information set I (because information about X improves one's ability to predict Y relative to I) and yet not Granger cause Y with respect to some larger information set I'.[3] The interventionist ac-

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[3]This relativity has a close parallel in Suppes' theory: Causal claims are relativized to a choice

count of causation does not exhibit this sort of relativization to an information set. More fundamentally, it appears possible to use X to manipulate Y (so that X qualifies as a cause of Y according to the interventionist account) even though X does not Granger cause Y. Suppose that the relation

$$Y_t = X_{t-1} + U$$

describes a causal relation in the manipulability sense (i.e., changing X_{t-1} is a way of changing Y_t) and that X is steadily increased by the same amount (e.g., by 1 unit) in every time period. Then knowledge of the value of X will not allow us to predict the mean value of Y any more accurately than we would be able to predict it given just past values of Y and thus X will not Granger cause Y. Nonetheless, by hypothesis, X can be used to control Y.

The difference between Granger causation and the notion of causation associated with the manipulability theory is widely acknowledged in the econometrics literature. Both Cooley and Leroy [1985] and Hoover [1988] give examples involving economic theory in which X is used to control Y even though X does not Granger cause Y. Hoover also notes that distinct but observationally equivalent causal models (see Section 3.4 below) may yield exactly the same claims about Granger causality but different claims about the results of various possible manipulations — further evidence for the distinctness of Granger causality and the manipulability conception. Hoover comments that "clearly, Granger causality and causality as it is normally analyzed [the manipulability conception] are not closely related concepts" [1988, 174] and that "Granger-causality and controllability may run in opposite directions" [1988, 200]. Granger [1990] agrees: he distinguishes his account of causation from the controllability or manipulability conception and explicitly rejects the latter. He writes [1990, 46], "The equivalence of causation and controllability is not generally accepted, the latter being perhaps a deeper relationship. If a causal link were found and was not previously used for control, the action of attempting to control with it may destroy the causal link." The contrary view embodied in the manipulationist conception is that an alleged causal link between C and E which is such that *all* attempts to control E by controlling C destroy the link is not a causal relationship at all. The difference between Granger causation and the manipulationist conception illustrates one respect in which a manipulationist conception can be contentful and non-trivial even if it is "circular" in the sense of failing to be a reductive theory: the manipulationist account is inconsistent with a widely accepted alternative account of causation.

Does it follow from the fact that Granger's account of causation and the manipulationist account yield different causal judgments that one of them must be "wrong"? Some will hold that in severing the connection between causation and the notion of a relationship that is at least potentially exploitable for manipulation, Granger has abandoned something so central to the way we ordinarily think about

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of partition, so that C may Suppes-cause E relative to partition P but not relative to some other partition P'. Moreover, Suppes regards the choice of a partition as a matter of pragmatics: there are no objective criteria for this choice.

"causation" that what we are left with is not really worthy of that appellation. Regardless of what one thinks about this contention, it seems uncontroversial that care must be taken not to move illicitly from one of these notions to the other. For example, it would be illegitimate to move from a test that shows that the size of the money supply Granger causes the inflation rate to the conclusion that the latter can be manipulated by manipulating the former, even though this last claim (about manipulation) is generally of most interest for policy purposes.

2.6.2 Causation, Regression, and Laws

Causation as incremental predictability (along the lines favored by Granger) and claims about the role of the uncorrelatedness assumption in causal interpretation are among the most prominent alternatives to the manipulationist conception in the social science literature. Within the philosophical literature, alternative accounts of causation that may be usefully compared with the manipulability account include law-based accounts, probabilistic theories of causation, and counterfactual theories.

A very substantial philosophical tradition connects the notion of cause and the justification of causal claims to *laws of nature.* Laws are variously said to be essential for distinguishing between true causal claims and true claims about the existence of accidental correlations (it is claimed that the former but not the latter "instantiate" laws), for constructing causal explanations (laws are said to play an essential role in explanations), and for providing "truth conditions" or for elucidating the meaning of causal claims. Some social scientists explicitly connect regression equations to laws. For example, Blalock [1964, 51] writes that, "It is the regression equations which give us the laws of science" and that regression equations, when interpreted causally, "involv[e] causal laws" [1964, 44]. Zellner [1984, 38] claims that the notion of cause in econometrics is the notion of "predictability according to a law or set of laws." Of course in assessing these claims much will depend on what is meant by "law." If "law" means something like a generalization that closely resembles paradigms of physical laws such as Maxwell's equations, the Schrödinger equation, and the field equations of general relativity, then it seems implausible that regression equations used to make true causal claims should be regarded as laws of nature. Even when not fully exceptionless, paradigmatic laws at least hold over a very large range of circumstances and background conditions and have very great scope — they apply to a wide range of different kinds of systems. It is generally agreed that in typical social science applications, regression equations that describe causal relationships are not like this — instead they describe (at best) much more fragile and circumscribed causal truths, which virtually always break down under a very wide range of conditions that are not explicitly specified in the equations themselves. Typically, they describe only local or relatively population-specific causal truths.

2.6.3 Regression Equations as Local Causal Truths

As an illustration of what this means, suppose that we estimate a regression of form (2.2.2) relating plant height to amount of water and fertilizer for a specific population of plants in certain background conditions. Even if the resulting equation correctly describes how the average height of the plants would change in response to some range of manipulations of the amount of water and fertilizer that they receive, so that the equation is "causally correct" in the interventionist sense of accurately specifying what will happen to the average height under these manipulations *for these plants and under these background conditions*, the equation will almost certainly fail to hold if we vary the background conditions in a substantial way — e.g. average temperature, soil conditions not reflected in the fertilizer variable, amount of sunlight and so on. Even if water and fertilizer remain causally relevant in these new circumstances (as reflected in non-zero regression coefficients) the numerical magnitude of these coefficients will likely vary considerably. In addition, the functional relationship between height and water and fertilizer postulated in (2.2.2) will hold at best only for some range of values of these variables when set by manipulations and not for all values of these variables. Thus while it may be true that changing the amount of water plants receive from, say, two liters to three liters may have, on average, the same effect on height as an increase from three to four liters, no one supposes that one may make a plant grow arbitrarily tall by giving it an arbitrarily large amount of water.

A similar point holds for most if not all social science applications of regression techniques. Consider, by way of illustration, the results of a series of experiments conducted in various U. S. states in the 1970s to explore the effects of a negative income tax on labor market participation, described in Stafford, 1985. Although the qualitative pattern was broadly similar across different states, with income maintenance having a relatively small effect on primary wage earners and a more substantial one on secondary earners, the coefficients representing the quantitative impact of these effects varied considerably across different populations. For example, the estimates of uncompensated wage elasticity for adult males vary from − 0.19 to − 0.07 and estimates of income elasticity vary from − 0.29 to 0.17. A similar result is found by Thomas Mayer [1980] who describes a number of empirical investigations of consumption functions, investment functions, demand functions, etc., most of which show considerable coefficient instability across different populations and circumstances. While this may derive in part from correctable methodological problems, Mayer's conclusion is that this instability also illustrates the "more general problem that behavioral parameters are not as stable as those in the natural sciences". In a similar vein, Johnston writes in an overview of recent empirical work in econometrics:

> One impression that surfaces repeatedly in any perusal of applied work is the fragility of estimated relationships. By and large these relationships appear to be time specific, data specific and country or region specific. [1992, 53]

A related line of thought is expressed by Achen, who writes that the researcher's purpose in using regression equations to describe causal relationships is to describe, for example,

> ... the effect of the Catholic vote on the Nazi rise to power or the impact of a pre-school cultural enrichment program like Head Start on poor children's success in school. Whatever the truth in such cases, one would not characterize it as a law. Neither Catholics nor impoverished youngsters would behave the same way in other times or places. [1982, 12]

An interventionist conception of causation and the associated idea that the sorts of causal relationships discovered and described by causal modeling techniques are typically relationships that are invariant only over a limited range of interventions, and for a specific set of background conditions or populations, provides a natural way of making sense of the ideas expressed by Johnston, Mayer, and Achen. There is nothing paradoxical or mysterious about the idea of a relationship that holds in a particular population or set of background conditions and that can be exploited for purposes of manipulation within that population or those background conditions but which may not be so exploitable for other populations or conditions — the examples given above furnish illustrations. By way of contrast, when causal relationships are associated with laws, this immediately suggests that those relationships must hold either universally or at least have considerable generality. And for better or worse, the causal relationships discovered by causal modeling techniques, at least in social science contexts, just don't have this feature.

The standard response of those who (influenced by philosophical preconceptions about the nomological character of causation) wish to retain some connection between the relationships that are the outputs of causal modeling techniques and laws is to suggest that these relationships should be viewed as "laws" of a special sort — so-called *"ceteris paribus"* or qualified laws, as opposed to "strict" laws. (See, e.g. [Kincaid, 1989; Pietroski and Rey, 1995]). *Ceteris paribus* laws are laws that hold "other things being equal" or only under special, restricted conditions. There are at least two problems with this response. First, despite many attempts, we still lack a successful account of *ceteris paribus* laws and their semantics (cf. [Earman, Roberts, and Smith, 2002; Woodward, 2002]). All of the extant accounts fail in a very fundamental way: they fail to distinguish between genuine causal relationships (which presumably reflect the holding of *ceteris paribus* laws) and relationships of accidental association. So despite the apparent naturalness of the idea that genuine causal relationships reflect or are "grounded in" laws (although perhaps of the qualified or *ceteris paribus* variety) while merely correlational relationships are not, it has turned out to be very difficult to spell this idea out in a convincing way. Second, there is a basic motivational problem with the appeal to *ceteris paribus* laws. Presumably those who invoke the connection between causal claims and laws do so because they think that the connection provides some sort of independent explication of what it is for a relationship to be causal. How-

ever, precisely because of the local and relatively fragile character of the causal relationships typically discovered by the use of causal modeling techniques in the social sciences, this explication can only work if many of the central features ordinarily associated with the notion of a law are somehow cancelled or withdrawn from *ceteris paribus* laws: in contrast to paradigmatic laws, *ceteris paribus* laws are non-universal, invariant only under a very limited range of interventions and background conditions, highly local, and so on. The upshot thus puts one in mind of a story about a special kind of dog that isn't a mammal, does not have four legs and a tail, and isn't man's best friend. In the absence of some independent account of what a *ceteris paribus* law is, the notion becomes a stand-in for "general causal claim", which is just the notion that we were originally trying to understand.

By contrast, appealing to the notions of manipulability and invariance does provide such an independent explication of what it is for a regression equation to describe a causal claim and also has the great advantage of not automatically building strong assumptions about universality and great generality into the content of all causal claims, associations which then somehow have to be removed if we are to capture the content of the sorts of causal claims discovered by regression techniques. On this approach, it is sufficient for a relationship to count as causal if it correctly describes what will happen under some range of interventions in some particular population and set of background conditions — in other words, if it is (at least) locally invariant. It then becomes a further empirical question to what extent this relationship continues to hold in other circumstances and how general and universal it turns out to be. Of course, this construal of the content of the causal conclusions warranted by causal models also highlights an important limitation of those conclusions: even if we are prepared to believe the claims that the model delivers about what would happen under interventions in some specific set of background circumstances, we will often have little idea about the extent to which those claims generalize to other populations or background circumstances.

At this point an additional distinction will be helpful in clarifying the nature of the causal claims made by (at least many) regression equations. Philosophers commonly distinguish between two kinds of causal claims: *type causal claims* to the effect that one type of causal factor causes or can cause another (e.g. smoking causes lung cancer) and *token causal claims* to the effect that some particular event or outcome was caused by some other event (e.g. Jones' smoking caused his lung cancer.) It is noteworthy that causal claims issuing from causal models do not fit naturally into either of these categories. Unlike type causal claims, the claims resulting from causal modeling techniques are typically population specific. What one tries to establish is not just that causal factor C can cause or sometimes causes effects of type E, but rather (roughly) that in some specific population causal processes of type C are at work at a certain rate in producing effects of type E. But unlike token causal claims, the researcher's interest is not in the causation of specific individual outcomes but something more generic; not why Jones developed lung cancer but rather why, e.g., the incidence of lung cancer among American women increased from 1940 to 1960 at such and such a rate, where the candidate

causal factor is the increase in women's cigarette consumption during this period. Similarly, when a researcher uses regression to investigate the deterrent effect of the death penalty, her interest is not (just) in establishing that the death penalty has on occasion deterred potential murderers (this claim is probably uncontroversial) and certainly not in establishing what role the death penalty played in the decision by some particular person to murder, but rather what the quantitative effect of the death penalty is on the murder rate in the U.S. population during a certain time period — how many murders are prevented (or not) by the imposition of the penalty.

2.6.4   Regression and Probabilistic Theories of Causation.

Theories that associate causal claims with laws represent one of the principal approaches to causation found in the philosophical literature. A second distinct approach connects causal claims to claims involving conditional probabilities. Although details vary, the core idea of such approaches (developed in related but somewhat different ways by Suppes [1970], Cartwright [1983], and Eells [1991]) is that a cause C must raise the probability of its effect E with respect to some suitably specified set of background conditions B_i. Typically such theories impose some version of a so-called unanimity requirement, according to which this probability raising (or at least the absence of probability lowering) must occur across *all* background conditions in the set of interest: that is, $Pr(E/C.B_i) > Pr(E/-C.B_i)$ for all B_i. Theories of this sort can either be reductive or non-reductive, according to whether the factors B_i are specified in non-causal or causal terms. In the former, reductive case the idea is to translate or reduce causal claims into claims about relationships among conditional probabilities (or more broadly conditional independence and dependence relationships), where these rely on some notion of probability that is understood as "objective" (as opposed to "subjective" — objective in the sense that different varieties of frequentism or propensity theories are objective interpretations of probability) but not as carrying causal commitments. In non-reductive versions, the background factors B_i to be conditioned on are taken to be other causes of E in addition to C; according to the simplest version, C is a cause of E iff C raises the probability of E when we condition on all combinations of values of these other causes.

Defenders of probabilistic accounts of causation commonly attempt to motivate this approach by appealing to the widespread use of causal modeling techniques like regression in the social and biomedical sciences. The assumption is that because statistical information is used in such contexts to make causal inferences, causal modeling techniques are committed to a notion of causation that is stochastic or probabilistic in something like the sense captured by probabilistic theories. This motivation is explicit in, for example, Suppes [1970], Eells [1991], and in Salmon's [1971] closely related SR model of statistical explanation.

In fact, however, the connection between causal modeling techniques and probabilistic theories of causation is far from straightforward. It is by no means clear

that the latter are appropriately viewed as reconstructions of an account of causation implicit in the former. Indeed the assumption that causal modeling techniques are committed to a probabilistic conception of causation in the sense described in the philosophical literature has led to some serious misunderstandings of such techniques and of the causal information they provide. To begin with, there are technical issues that stand in the way of any straightforward connection. Probabilistic theories are typically constructed around the assumption that variables are dichotomous or at least measurable only on a so-called nominal scale — that is, that they take just two or at most a discrete set of values. By contrast, regression equations include relations among variables that are measurable on ratio or interval scales — that is, variables that take continuous values, at least within some range, like height or IQ. Although one can include dichotomous variables among the regressors in a linear regression equation, the dependent variable *cannot* be dichotomous, for the obvious reason that the resulting relationship will no longer be even approximately linear. While there are ways of representing relationships involving dichotomous dependent variables, models in which the dependent variable is interpretable as a probability of some outcome occurring, and techniques for estimating the parameters that characterize such relationships, these represent cases with very special features — the more general and typical case of functional relationships among quantitative variables does not fit naturally into such a framework. For example, if one regresses plant height on amount of water and obtains a non-zero regression coefficient b, it is unclear how to interpret this in terms of the idea that amount of water "raises the probability" of plant height and even more unclear what the motivation is for trying to provide such an interpretation, given that, if it is causally correct, the regression equation gives us the functional form and linear coefficient relating these quantities and is thus far more precise and informative than any information provided by the "raises the probability of" locution. From this perspective, probabilistic theories focus on a very special case — one in which the variables involved are dichotomous or nominal — a case which does not generalize naturally to the (very common) situation in which variables have a more quantitative structure, the latter being more typical of the cases in which regression and other causal modeling techniques are applied.

There are other differences between the assumptions about causation made within probabilistic theories and the assumptions made when causal modeling techniques are used that are even more fundamental. Probabilistic theories assume that the causal relation itself (at least at the level of description at which the analyst is working) is chancy or stochastic rather than deterministic. By contrast, if regression equations are taken literally, they posit causal relationships that are deterministic. In a regression equation like (2.2.2) the stochastic element enters the picture in the assumptions made about the distribution of the error term U; the relationship between X and Y is not itself modeled as stochastic. Taken literally, (2.2.2) says that if we could intervene on a particular occasion to change the value of X in a way that did not disturb the value taken by U on that occasion, Y would exhibit a deterministic response. Any spread or stochasticity in the response of Y

to repeated interventions that set the value of X for different individuals occurs because U varies randomly for different individuals. Of course it might be argued that equations like (2.2.2) should not be taken literally — that the way in which the error term figures in (2.2.2) is merely a misleading way of trying to capture or represent the fact that the relationship between X and Y itself is stochastic. One problem with this construal is that it deprives both the assumption that U enters into (2.2.2) in an additive way and the assumptions D about the distribution of U of their most obvious motivation: If the presence of the error term is just a way of representing the fact that the relation between X and Y is stochastic, why assume additivity and distributional assumptions like D, given that there are many possible stochastic links between X and Y that are not captured by these assumptions? Moreover, what becomes of the alternative interpretation of the error term as representing other causes of Y besides X? Does the error term now represent both such causes *and* a stochastic element in the relation between X and Y? If only the former, how then should we interpret the uncorrelatedness assumption?

It is presumably considerations like these that have led those who wish to use causal models to represent the idea that the relationship between cause and effect is itself stochastic to avoid using the error term for this purpose and instead to introduce a new representational apparatus. For example, in order to represent stochastic causation in causal modeling contexts, Nancy Cartwright [1989] introduces, in addition to the usual regression representation, a random indicator variable b^*, taking the values 1 and 0, that represents whether a cause "fires" or not. A stochastic relationship between X and Y is then modeled as $Y = b^*aX + U$, where a is a conventional regression coefficient and b^* is defined as above. This yields a perspicuous representation of the idea that the relationship between X and Y is itself stochastic, over and above whatever stochastic element is introduced by the error term, but it is a major departure from the traditional regression representation, which, to repeat, views the connection between dependent and independent variables as deterministic.

Finally, there is an even more fundamental difference between probabilistic accounts of causation at least in their reductivist versions and the picture of causation that appears to be implicit in regression and other causal modeling techniques. Causal modeling techniques distinguish between two different kinds of information which are represented in quite different ways and have different roles. First, there is a body of statistical information having to do with the variances and covariances of various measured variables, as well as the distribution of the error term, all of which might be represented by a joint probability distribution. Second, there is a distinct mathematical object — the regression equation itself, whose role, when the equation is interpreted causally, is to represent a causal relationship. There is thus no attempt to directly capture or represent causal relationships just in terms of facts about the probability distribution. Instead, the role of the probability distribution/statistical information is to serve as evidence (in conjunction with other assumptions) for the claims about causal structure made by the regression

equation itself. It is this distinction that allows us to consistently describe the regression equations representing causal relationships as deterministic even though the evidence which is used to fix the values of the regression coefficients is "statistical". The separation of these two kinds of information in turn allows us to raise questions about their relationship: under what conditions and given what additional assumptions can one infer facts about causal structure from probabilistic information about the joint distribution of the independent and dependent variables? To what extent is information about probability relationships among variables by itself sufficient to uniquely determine the causal relationships among those variables? By way of contrast, reductive versions of probabilistic accounts of causation attempt to define or capture causal relationships simply in terms of facts about probability relationships, so there is no room for the additional questions just described.[4]

2.7   The Underdetermination of Causal Relationships by Statistical Information

The observations in the preceding paragraph might be of little significance if purely statistical information were by itself sufficient to fix the causal structures postulated in causal models, but this is very far from being the case: serious underdetermination problems exist even if we know that the correct structure is a regression structure. These problems become even more acute if the true causal structure can be more complex (See Section 3 below). To illustrate one of the simplest possibilities, consider the problem of which variables to include in a regression equation. As the expression (2.2.3) for the OLS estimator for the regression coefficients makes clear, the estimated value for each coefficient b_i will be a function not just of the covariance between X_i itself and the dependent variable Y, but also a function of the covariance between X_i and all of the other independent variables in the equation. It follows that one can always alter the coefficient of any variable within a regression equation by the inclusion or deletion of other independent variables as long as these variables exhibit a non-zero covariance with the other independent variables and with Y. For example, in the two variable multiple regression equation

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[4]To what extent do these remarks carry over to non-reductive versions of probabilistic theories? Such theories make use of an additional primitive (or representational element) such as "X is causally relevant to Y" and thus do not attempt to capture causal facts just in terms of facts about probability distributions. However, this additional representational element turns out to be inadequate to capture complex causal structures of the sort described in Section 3 below in which one variable affects another by distinct routes. The guiding idea of non-reductive probabilistic theories of causation — that C causes E if and only if C raises the probability of E conditional on other causes B_i of E — is plausible only if one is dealing with a regression structure and fails for more complex casual structures (see Woodward [2003, Ch. 2] for details).

$$Y = b_1 X_1 + b_2 X_2 + U$$

the OLS estimator for the coefficient b_1 is

(7.7.3) $b_1* = [S_{x2}^2 . S_{yx1} - S_{yx2} S_{x1x2}]/[S_{x1}^2 S_{x2}^2 - (S_{x1x2})^2]$

where S_{x2}^2 is the sample variance of X_2, S_{yx1} the sample covariance between Y and X_1 and so on.

Hence, the estimated value of the coefficient b_1 depends on the correlation between Y and X_1, the correlation between X_1 and X_2, and the correlation between X_2 and Y. As long as X_2 exhibits a non-zero correlation with X_1 and Y, we can change the estimated value of b_1 by dropping X_2 from the equation or by substituting for X_2 a new variable X_3 which has a different correlation with X_2 and Y.

Suppose, for example, that the "true" regression equation is

(2.8.1) $Y = b_1 X_1 + b_2 X_2 + U$

but that one mistakenly omits the relevant variable X_2 and instead estimates

(2.8.2) $Y = b_1 X_1 + V$

Then one can show that
$$E(b_1) = b_1 + b_2 b_{12}$$

where b_{12} is the regression coefficient of the omitted variable X_2 on the included variable X_1. That is, the bias in the estimated value of b_1 resulting from the omission of X_2 will be the true coefficient of X_2 multiplied by the regression coefficient of X_2 on X_1.

When X_2 is omitted, the coefficient b_1 on X_1 will thus fail to accurately describe the effect on Y of an intervention that changes X_1 alone. A similar difficulty will arise if we include a variable X_3 that is not causally relevant to Y but is correlated with Y and with X_1.[5] As a practical matter, it will be virtually always possible to find such variables in realistic cases. Thus, without additional constraints of some sort about which variables may legitimately be entered into the equation, many different causal conclusions will be consistent with the observed statistical information.

A striking illustration of this problem (which is known all too well to practitioners in the social sciences) is furnished by Leamer [1983]. He considers the problem of determining whether capital punishment in the present day U.S. has

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[5]For this reason, the common methodological assumption that it is somehow preferable or more reliable to employ a very large set of candidate independent variables as regressors is indefensible. If such candidate variables are not direct causes of the dependent variable Y but are correlated with it, the result will be to produce biased estimates even for those variables that are causes of Y.

a deterrent effect — whether it lowers the murder rate. He begins by considering a long list of variables which, as he puts it, have been hypothesized to influence the murder rate [1983, 41]. These include, among others, variables that purport to measure the extent of deterrence — e.g., the conditional probability of execution, given conviction for murder, and the conditional probability of conviction for murder, given commission, variables measuring economic conditions such as the unemployment rate and the median family income, and variables reflecting other social and demographic factors, such as the racial composition of the population. All variables are measured at a state-wide level. Leamer shows that if one includes all of these variables in a multiple regression equation, one obtains the result that each additional execution deters, on the average, 13 additional murders. However, as Leamer points out, people with different ideological or theoretical expectations about which factors are likely to influence the murder rate will of course regard different subsets of these variables as plausible candidates for inclusion or exclusion from the regression equation. For example, someone with the set of expectations Leamer labels "right-wing" will regard the deterrent variables as almost certainly relevant and will want to include these in the regression equation, but will regard the social and economic variables as doubtful and will favor omitting some or all of these. Leamer shows that if the right winger includes all of the deterrent variables and a suitably chosen linear combination of the other variables he can obtain a deterrent effect of 22.56 lives saved per execution. Alternatively, by including the deterrent variables and another set of non-deterrent variables, one can obtain the result that 0.86 lives are saved per execution. Someone who includes and excludes still other variables — who, say, focuses largely on social and economic variables as causes of crime (a "bleeding heart," as Leamer calls him) and who regards the inclusion of some of the deterrent variables as doubtful, will find with the right choice of variables, that each execution *causes*, on the average about 12 additional murders. This example thus shows how, by beginning with different premises about which variables should be included in the regression equation we are estimating, one can reach quite different conclusions about the causal effect of capital punishment on the murder rate, given the same body of statistical data.

Leamer's own recommendation for dealing with the problem is to carry out what he calls a sensitivity analysis. Researchers should estimate a variety of different regression equations with different combinations of variables that, on the basis of the prior beliefs, they regard as plausible or possible candidates for inclusion in the equation. They should then report how sensitive the coefficients on the various variables in the equation are to decisions about which other variables to include. If the regression coefficient for some variable remains relatively stable as one varies the plausible candidates for other variables to be included in the equation, then one can draw a causal conclusion about the role of this variable with some confidence. Thus, for example, if it happened that the apparent deterrent effect of the death penalty remained relatively constant regardless of which other variables (i.e. ones that the researcher is willing to regard as possibly causally relevant) are included in the regression equation, then one would be justified in drawing a rough conclusion

about the size of that effect. But since, as we have seen, this is not the case, the most reasonable conclusion, according to Leamer is that "any inference from these data about the deterrent effect of capital punishment is simply too fragile to be believed" [1983, 42].

Leamer's overall framework is a form of subjectivist Bayesianism. Different researchers will have different prior beliefs about which variables should be included in a regression equation, but there is no "objective" basis for determining which of these beliefs are "right" or "wrong". According to Leamer, the list of potential explanatory variables in any particular case is "never ending," and "the exact point at which the list is terminated is whimsical" and has to do with which variables the researcher feels "comfortable" including [1983, 39]. In principle, an investigator might adopt almost any belief about these matters. As Leamer puts it at one point, "If the level of money might affect GNP, then why not the number of presidential sneezes or the size of the polar ice cap?" [1983, 35]. Given that this is the case, all the researcher can do is to report how different prior opinions will, given the same body of statistical data, map into different posterior causal beliefs.

2.8   The Role of Extra-Statistical Assumptions in Regression

Most thoughtful researchers would agree that the considerations just described show that if regression and other causal modeling techniques are to be used to draw causal conclusions, additional "extra-statistical" assumptions (that is, assumptions that go beyond the statistical data) of various sorts are required, but many would disagree that these assumptions will always be as "subjective" as Leamer suggests. For purposes of comparison consider Tufte's [1974] analysis of the causes of variations in automobile fatality rates across states. Although his treatment relies mainly on simple scatter plots and tabular comparisons, we can easily translate it into a multiple regression context. Thinking about the problem in this way, what Tufte in effect does is to regress a variable representing death rates for the United States against a number of other independent variables including a variable representing presence or absence of automobile inspections, a variable representing population density, and variables representing whether or not a state was one of the original 13 states and has seven or less letters in its name. He obtains a non-zero regression coefficient in each case — death rate is correlated with each of these variables and, as it happens, each variable is correlated with the others. Like Leamer, Tufte in effect notes that the value for the regression coefficient for each of these variables will vary depending on which other variables are included in the regression.

Tufte takes it to be obvious, however, that if the regression equation is to be interpreted causally, inspections and population density are appropriate variables to include on the r.h.s. of the equation while the other variables described above are not. Obviously, this conclusion cannot be based on the pattern of statistical association among these variables and the death rate but must instead be based

on other considerations, which have to do with what Tufte calls "substantive judgment." He writes

> While we observe many different associations between the death rate
> and other characteristics of the state, it is our substantive judgment,
> and not merely the observed association that tells us density and in-
> spections might have something to do with the death rate and that the
> number of letters in the name of the state has nothing to do with it.
> [1974, 9]

The suggestion that the presence of automobile inspections "might have something to do with" fatality rates seems unmysterious. While we might well wonder, prior to empirical investigation, whether a particular program of inspections has caused a substantial lowering of deaths and injuries, it is an uncontroversial part of our causal background knowledge that mechanical malfunctions of various sorts can cause automobile accidents. If inspections succeed in detecting such malfunctions and reducing their incidence — and after all this is what they were designed to do — they will cause a reduction in fatality rates.

The correlation between population density and fatality rates, by contrast, is somewhat less transparent, but Tufte is able to show that this association is just what we would expect, given other widely shared causal assumptions.

> Thinly populated states have higher fatality rates compared to thickly
> populated states because drivers go for longer distances at higher speeds
> in the less dense states. Accidents in states like Nevada and Arizona
> are probably typically more severe since they occur at a higher speed.
> It is not, however, just a matter of the number of miles driven, be-
> cause there is also a fairly strong negative relationship between den-
> sity and the *deaths per 100 million miles driven* in the state. Victims
> of accidents in the more thinly populated states, in addition to being
> involved in more severe accidents, are also less likely to be discovered
> and treated immediately, since both Good Samaritans and hospitals
> are more scattered in thinly populated states compared to the denser
> states. [1974, 20-21]

By contrast, according to Tufte our background causal knowledge suggests that the number of letters in a state's name is simply not the sort of thing that could causally influence the automobile fatality rate (it would not occur to anyone to suggest shorter state names as a highway safety measure). We are thus led to regard the correlation between these two variables as spurious or at least not reflective of a causal connection running from names to fatalities.

Readers familiar with the philosophical literature on probabilistic causality will recognize that it contains very similar observations, which are used to motivate non-reductive accounts of causation. Consider a well-known example, originally described by Bickel *et al.*[1977], but introduced into philosophical discussion by Nancy Cartwright [1983]. Being accepted into graduate school at UC-Berkeley is

correlated with gender — men have a higher acceptance rate than women — thus raising a prima facie case that Berkeley discriminates against women. However, a more careful look at the acceptance data shows that, department by department, women are admitted at about the same rate as men, and that the lower overall acceptance rate for women arises because women were more likely than men to apply to departments with higher than average rejection rates for all candidates.

Suppose that we represent acceptance or rejection, gender (male, female) and whether one is applying to a department with a low or high rejection rate by means of dichotomous variables Y, X_1, and X_2, the former being the dependent variable and the latter two candidates for independent or cause variables. As noted above, linear regression is not appropriate for various technical reasons in this sort of case, but the underlying logic is much the same as in the examples discussed by Leamer and Tufte. Y and X_1 are statistically dependent. However, Y and X_2 are also statistically dependent, as are X_2 and X_1. When we control for (that is, condition on) X_2 we find that Y and X_1 are independent, but that X_2 and Y are dependent when we control for X_1. Switching representations so that $Y, -Y, X_1, -X_1$ and $X_2, -X_2$ now represent *values* of variables so as to allow for translation into the format adopted in the probabilistic theory of causation literature, we have

$$P(Y/X_1) > P(Y/-X_1)$$
$$P(Y/X_2) > P(Y/-X_2)$$
$$P(X_1/X_2) > P(X_1/-X_2)$$
$$\text{but } P(Y/X_1.X_2) = P(Y/-X_1.X_2), P(Y/X_1.-X_2) = P(Y/-X_1.-X_2).$$

If we are willing to think of X_2 as the causally relevant variable in this situation, then it looks as though X_1 is causally irrelevant to Y and the correlation between Y and X_1 arises because of the way in which X_1 happens to be correlated with the genuinely causally relevant variable X_2.

As Cartwright points out, it is obviously not the pattern of statistical association by itself among these variables (or any other variables we might have measured) which generates this conclusion:

> If, by contrast, the authors had pointed out that the associations reversed themselves when the applicants were partitioned according to their roller skating ability that would count as no defense.

> The difference between the two situations lies in our antecendent causal knowledge. We know that applying to a popular department (one with considerably more applicants than positions) is just the kind of thing that causes rejection. But without a good deal more detail, we are not prepared to accept the principle that being a good roller skater causes a person to be rejected by the Berkeley graduate school and we make causal judgments accordingly. [1983, 37–8]

Cartwright's point is that the causal conclusions drawn from this example only follow if we are willing to make certain prior causal assumptions — in this case,

the assumption that decisions about admission are made primarily at the level of individual departments, that individual departments have a limited, usually fixed number of admissions slots and cannot accept all applicants, that roller-skating ability is causally irrelevant to decisions to admit since individual departments do not care about this feature of their applicants and in all probability lack information about it, and so on.

The judgment that X_2 is an appropriate variable to partition on and that roller-skating ability is not is based on causal considerations that parallel Tufte's judgment that density is an appropriate variable to enter into a regression equation while the number of letters in a state's name is not. As in the previous examples, the mere fact that there is *some* variable X_i such that including it makes the correlation between Y and X_1 disappear isn't what justifies the conclusion that Berkeley is innocent of sex discrimination, for it will often, perhaps always, be possible to find such a variable, whether or not there is a causal relation between Y and X_1 or between Y and X_i.

We can see this point in a more abstract way as follows. Suppose that A is positively statistically relevant to (and raises the probability of) $B : P(A/B) > P(A/-B)$. Then provided one can find a third variable C which is statistically dependent in the appropriate way on A and B, one can, by conditioning on C, either maintain the direction of this inequality, turn it into an equality, or reverse its direction. To see this write:

(2.9.1) $$P(A/B) = P(A/B.C)P(C/B) + P(A/B.-C)P(-C/B)$$

(2.9.2) $$P(A/-B) = P(A/-B.C)P(C/-B) + P(A/-B.-C)P(-C/-B)$$

Comparing the first terms in the two products on the r.h.s. of (2.9.2) we can have (2.9.1) > (2.9.2) with any one of

$$P(A/B.C) > P(A/-B.C)$$
$$P(A/B.-C) > P(A/-B.-C)$$

or

$$P(A/B.C) = P(A/-B.C)$$
$$P(A/B.-C) = P(A/-B.-C)$$

or

$$P(A/B.C) < P(A/-B.C)$$
$$P(A/B.-C) < P(A/-B.-C)$$

provided $P(C/B), P(C/-B)$ etc have the appropriate values — that is, depending on how B and C are correlated.

Our focus so far has been primarily on the need for additional "extra-statistical" assumptions to motivate the inclusion or exclusion of variables from a regression equation. In fact, however, extra-statistical assumptions are required at a number of other points as well if regression techniques are to be used to produce reliable

causal conclusions. One such point concerns the role of the error term. As already noted, in the context of causal inference the error term (and the uncorrelatedness assumption associated with it) must be interpreted causally — as the net effect of those causes of the dependent variable that are omitted from the equation. Obviously, when so understood, the uncorrelatedness assumption is not a "purely statistical" assumption — its validation requires substantive knowledge of which omitted variables are causes of the dependent variable and how these are distributed.

In addition to this, the assumption that the system being modeled has a regression structure as well as the assumption that we have the causal order right — that is, that the dependent variable in the equation we employ is an effect and not a cause of any of the independent variables in the equation — are themselves substantive additional assumptions and their violation will also lead to mistaken causal conclusions. These include not just the obvious mistake of concluding that, e.g., Y is an effect of X when it is really a cause from the fact that regression of Y on X yields a non-zero coefficient, but more subtle errors as well. For example, if Y is caused by both X_1 and X_2 with no causal connection between X_1 and X_2 (indeed with X_1 and X_2 statistically independent), then regressing X_1 on Y and X_2 will yield a non-zero regression coefficient between X_1 and X_2, essentially because the independent causes of a common effect are dependent conditional on the value of that effect. (Cf. Section 4)

Stepping back from these various examples, we can see the following overall pattern. On the one hand we have (what we might describe as) purely statistical or correlational information about the covariances and variances among some set of variables. However, this information is not by itself sufficient to warrant causal conclusions (that is, to warrant a particular regression equation, interpreted causally rather than some alternative). To reach such conclusions, additional assumptions of some sort are required: **Additional Assumptions + Statistical Information → Causal Conclusions**.

Within the framework of an interventionist account of causation, all of this should seem unsurprising. Statistical information about variances, covariances, etc. describes what actually happens — the actual pattern of co-occurrence among the variables of interest in some relevant population. By contrast, causal claims are claims about what would happen to certain variables if interventions *were* to be performed on others — as such, they have a modal or counterfactual character that goes beyond claims about what actually happens, at least in cases like the one presently under discussion, in which the intervention in question is not actually performed. As we see in Tufte's example, both inspections and the number of letters in a state's name are correlated with auto fatality rates, but it is a further question whether, if an intervention were to be performed on the first two variables, the fatality rate would change. Additional assumptions besides the statistical information are needed to answer such questions.

As noted above, such assumptions are commonly described as "a priori" or "extra-statistical". This does not mean that they are non-empirical or incapable

of being tested, but rather that their justification has some other source or rationale than just the statistical information at hand. In the examples discussed above, the needed additional assumptions look themselves to be causal in character and domain (or subject matter) specific in the sense that they embody specific empirical claims about, e.g., the ability or inability of one specific causal factor (e.g. inspections, number of letters in a state's name) to causally influence another (automobile fatality rates). We lack a generally accepted philosophical account of the character and status of such claims but, intuitively it often seems natural to think of them as claims about causal capacities in roughly the sense described in Cartwright [1989] or Woodward [1993; 1995] or about possible causes in the sense of Scriven [1959]. That is, they are qualitative claims to the effect that C has the capacity (or not) to cause a specific sort of effect E.

The use of such causal assumptions as inputs to regression and other causal modeling techniques does not of course mean that such techniques are circular, or uninformative, or cannot be used to discover new causal knowledge. Suppose that we are unsure whether X_1 causally influences Y. If we can identify other causes of Y besides X_1 and/or also know that those other causes of Y we are unable to identify make a net contribution to Y that satisfies the distributional assumptions (D), then by regressing Y on X_1 and the other known causes of Y, we can determine whether X_1 influences Y by ascertaining whether the regression coefficient on X_1 is non-zero. We thus use *other* causal information (that is, causal information that does not have to do with the existence or non-existence of a causal relationship between X_i and Y), in conjunction with statistical information, to determine whether X_1 causes Y.

An alternative possibility is that we already know some qualitative facts about the causal relationship between X_i and Y (e.g., we regard it as uncontroversial that inspections will have some influence, although perhaps a very small one, on fatalities and that it will reduce fatalities rather than increase them) but we want more specific quantitative knowledge about exactly how much change in Y will be produced by various changes in X_1 (how much will fatalities be reduced if we introduce a certain level of inspections). At least in principle, regression can provide such information.

Our focus so far has largely been on the use of subject matter specific background causal knowledge as inputs to causal modeling. However, there is another possibility: it may be possible to formulate other general and relatively domain independent principles that can be combined with statistical information to reach causal conclusions. The use of such general principles in the discovery and validation of causal models has been systematically explored by Peter Spirtes, Clark Glymour and Richard Scheines in an extensive series of papers and books (e.g. [Spirtes, Glymour, and Scheines, 2000]). Their program will be discussed in Section 4

2.9   Testing versus Estimation

As explained above, OLS techniques, as well as other more complex techniques that can sometimes be employed when the conditions for the reliable use of OLS estimators are not met, are simply techniques for estimating coefficients, given a specification of a regression equation — that is, given the assumptions about causal direction, causally relevant variables etc. embodied in the equation. It is important to understand that such estimation does *not* test the specification itself against alternative possible specifications. To the extent that, as in Leamer's example, different regression equations that incorporate different independent variables (or perhaps even make different assumptions about which are the independent and dependent variables) are observationally indistinguishable, given the observed statistical data, estimating the coefficients in any one of these equations (and doing the usual statistical tests yielding confidence intervals, standard errors, etc. for these coefficients) does nothing to establish that this particular equation rather than one of these alternatives is causally correct. To put the point slightly differently, testing a regression equation (or any other causal model) in the sense of providing reasons for believing it, has to do with showing it is superior to alternative, competing models. This kind of testing needs to be sharply distinguished from estimating the coefficients in the equation. When a researcher does the latter, and makes a causal claim based on the result, the relevant questions to ask are (i) whether there are plausible alternative models (there usually are) and (ii) whether there is evidence that discriminates among these in a way that favors the researcher's model. Users of causal modeling techniques often fail to address these questions systematically, contenting themselves instead with estimating a single model.

3   CAUSAL INFERENCE IN SYSTEMS OF EQUATIONS

3.1   Introduction

A single regression equation represents a particularly simple causal structure in which a single dependent variable is represented as caused by one or more independent variables but in which no causal relationships are represented as holding among the independent variables themselves and no cyclic or reciprocal causal links running back from the dependent variable to the independent variables are represented. Often, however, social scientists and other users of causal modeling techniques want to represent more complex causal structures in which variables that are effects of some causes are represented as causing other variables and in which there may be reciprocal or feedback causal relationships between variables. Structures of this sort are represented by systems of equations. The conventions governing the equations in such a system parallel those for individual regression equations: each individual equation represents the claim that the variables on its r.h.s. are direct causes of the variable on its l.h.s. ("Direct" here means simply

that the causal relationship is not mediated by any other variables in the system of interest — see section 3.4 for additional discussion.) To represent causal relationships among the r.h.s. variables, one simply adds additional equations in which these additional relationships are represented, with a separate error term for each equation. To illustrate, suppose that voting behavior V is directly influenced both by voters' evaluations E of candidates and by their party identification P. Moreover, P exerts an independent influence on E. Assuming that these relationships are linear, we may represent them as follows

$$(3.1.1) \qquad\qquad E = a_1 P + U_1$$

$$(3.1.2) \qquad\qquad V = a_2 P + a_3 E + U_2$$

where U_1 and U_2 are error terms.

As suggested above, the coefficients a_1, a_2, and a_3 are intended to represent the "direct effects" of P on E, P on V, and E on V. We should note for future reference that these direct effects need not be the same as the "total effects" of these variables. The total effect of P on V is the sum of its influence on V along two different routes: a direct route, represented by the coefficient a_2, and an indirect route, along which P influences E which in turn influences V. We may obtain the total effect of P on V by substituting the expression for E given by (3.1.1) into (3.1.2):

$$(3.1.3) \qquad \begin{aligned} V &= a_2 P + a_3(a_1 P + U_1) + U_2 \\ V &= (a_2 + a_3 a_1)P + a_3 U_1 + U_2 \end{aligned}$$

The total effect of a change in P on V is thus represented by the coefficient $a_2 + a_3 a_1$. Note that the total effect of P on V can be interpreted along the same lines as the interpretation of the coefficients in individual regression equation advocated in Section 2.3.3: The total effect of P on V is just the change in V that would be associated with a single intervention on P (with no other interventions on other variables in the system). By contrast, it is less obvious how to interpret the individual coefficients such as a_2 in a system of equations like (3.1.1–3.1.2) since an intervention on P will produce a change in V corresponding to the total effect given by the sum $a_2 + a_3 a_1$ rather than to the coefficient a_2. We will return below to the question of what the individual coefficients and the direct effects associated with them mean.

As a second illustration, consider the following model which says that the quantity (Y_1) of some good demanded by a household depends on its price (Y_2), household income (X_1), and an error term, while its price in turn depends on quantity demanded and the wage rate X_2 and an error term

$$(3.1.4) \qquad\qquad Y_1 = a_1 Y_2 + a_2 X_1 + U_1$$

(3.1.5) $$Y_2 = a_3 Y_1 + a_4 X_2 + U_2$$

Here, in contrast to (3.1.1–3.1.2), we have a case of reciprocal causation between Y_1 and Y_2.

It will be useful for our subsequent discussion to put this in more abstract form. Assume that we have a "complete" system of m equations with k "exogenous" variables $X_1 \ldots X_k$ and m "endogenous" variables $Y_1 \ldots Y_m$. We can think of the former as variables, values of which are determined by factors outside the model. The model does not represent the causes of these variables, although these variables are represented as causes of other ("endogenous") variables, whose values are determined by variables within the model. We can write this system in matrix form as

(3.1.6) $$YA + XB + U = 0$$

where A is a $m \times m$ matrix of coefficients for the endogenous variables and B is a non-singular $m \times k$ matrix of coefficients for the exogenous variables, respectively, and U represents the error matrix. In the case of an ordinary regression equation, we saw that we could estimate the values of the structural regression coefficients from information about the variances and covariances of the dependent variable Y and the exogenous variables X_i, provided certain assumptions governing the distribution of the error term were satisfied. In a simultaneous equation context we have a similar evidential base — information about the variances and covariances of the variables $X_1 \ldots X_k$ and $Y_1 \ldots Y_m$ — but the problem of estimating the coefficients from this base is considerably more complicated.

3.2 Recursive and Non-Recursive Models

To explore these complexities, it is useful to begin by distinguishing between two different sorts of simultaneous equation models: *recursive* and *non-recursive* models. For our purposes, we shall regard a model as recursive if it has two features: first, it must be *hierarchical*. Intuitively, this means that the causal relationships in the model are unidirectional in the sense that there are no direct reciprocal relations as when X_1 causes X_2 which in turn causes X_1 and no causal loops as when X_1 causes X_2 which causes X_3 which in turn causes X_1. A bit more precisely, a model will be hierarchical if the endogenous variables are capable of being causally ordered in such a way that the first endogenous variable in the ordering is determined only by the exogenous variables, the second endogenous variable only by (at most) the first endogenous variable and the exogenous variables, and so on — i.e., no endogenous variable causes a variable before it in the ordering. Second, a recursive model must satisfy some rather strong assumptions concerning the distribution of the error terms — in particular error terms in each equation in the model must be uncorrelated both with the other r.h.s. variables in that

equation *and* with all the other error terms in the other equations (that is, the error terms must also be uncorrelated *across* equations.)

In matrix representation the requirement that a model be hierarchical means that the coefficient matrix B containing the coefficients relating the endogenous variables to each other must be triangular or such that it can be made triangular by a reordering of the endogenous variables — i.e., such that it consists of all zeroes above the diagonal. The following model exhibits this sort of triangular structure

$$
\begin{aligned}
X_2 &= b_{21}X_1 + U_2 \\
X_3 &= b_{32}X_2 + b_{31}X_1 + U_3 \\
X_4 &= b_{43}X_3 + b_{42}X_2 + b_{41}X_1 + U_4
\end{aligned}
$$

(3.2.1)

The requirement that the error terms be uncorrelated across equations implies that all of the off-diagonal elements in the covariance matrix U for the error terms are 0.

It follows (cf. [Berry 1984, 12]) from these assumptions that one can use OLS to obtain unbiased estimates of the coefficients in such a system. In effect, one can treat such systems as a collection of regression equations and estimate them accordingly.

Suppose, however, that the model with which we are dealing is not recursive, either because it is not hierarchical or because the error terms are correlated across equations. Recall that the error terms in a given equation represent variables that influence the dependent variable in that equation but have been omitted from the equation. In many cases it will be plausible that some of the same variables have been omitted from two or more equations and, if so, the error terms will be correlated across equations. For example, in the system (3.1.1–3.1.2) it may be that some of the omitted factors U_1 which, in addition to party identification, influence voters' evaluations, are also among the omitted factors U_2 which (in addition to P and E) influence voting behavior. If so, U_1 and U_2 will be correlated. There is general agreement among those who use causal modeling techniques that this sort of possibility — in which the error terms are correlated across equations because the same variables have been omitted from those equations — is probably extremely common in contexts in which such techniques are used.

What will happen if such a correlation occurs? Consider the two equation model

(3.2.2) $Y_1 = aX_1 + U$

(3.2.3) $Y_2 = bY_1 + V$

If U and V are correlated, Y_1 and V will also be correlated (because U is correlated with Y_1). Hence, the independent variable (Y_1) in (3.2.3) will not be independent of the error term in that equation. In this case, as we have seen, OLS estimators will be biased. It is also easy to show that if a model is non-hierarchical then at

least some of the error terms occurring in some of the equations *must* be correlated with the independent variables in that equation (regardless of whether the error terms are correlated across equations). As an illustration, return to the system (3.1.4–3.1.5). From (3.1.4) it follows that U_1 and Y_1 must be correlated. Since Y_1 appears on the r.h.s of (3.1.5) Y_2 and Y_1 (and hence Y_2 and U_1) must be correlated, although both appear on the r.h.s of (3.1.4). In such cases, OLS estimators again will be biased (and inconsistent). Thus, such estimators will be biased in all non-recursive models if either the error terms are correlated or if the system is non-hierarchical.

In the case of some non-recursive models it may be possible to estimate the coefficients by means of other more complex techniques (indirect least squares or instrumental variables), but it is also possible that the available statistical information and other constraints do not allow for the estimation of some of the coefficients. Instead, one or more of the coefficients may be *unidentified.*

3.3   Identification

A model or set of equations is said to be identified if, given the full joint distribution of the variables in the equation and available empirical extra-statistical constraints (hereafter, the observational information), the values of all of the coefficients in the equations can be uniquely determined from this information. A model is not identified if alternative models, with different coefficients, are compatible with the observational information. When a model is not identified, there thus will be a number of distinct candidates for the correct structural model all of which are "observationally equivalent" in the sense that we cannot discriminate among them purely on the basis of available observational information.

3.4   Reduced Form Equations

One natural way of seeing how the problem of identifying the parameters in a model arises (and of understanding the conditions under which it may be solved) is by reference to the idea of reduced form equations associated with a model. Intuitively, the reduced form equations describe the total effects associated with each exogenous variable in a causal model. Given a system of equations which correctly represents some causal structure, we can always find the associated reduced form equations by substituting into the original structural system for the values of the endogenous variables in such a way that we are left with a system of equations in which each endogenous variable appears in only one equation and only on the left hand side of that equation, and only exogenous variables appear on the right hand side of each equation.

As an illustration, consider again the system (3.1.1–3.1.2). In this system, there is one exogenous variable, P, and two endogenous variables, E and V. The reduced form will thus consist of two equations, one with the endogenous variable E on the left hand side and the exogenous variable P on the right hand side, and the other

with the endogenous variable V on the left hand side and P on the right hand side. Since (3.1.1) is already in this form, it is its own reduced form equation. The reduced form equation associated with (3.1.2) is just the equation

$$(3.1.3) \qquad V = (a_2 + a_3 a_1)P + a_3 U_1 + U_2 = bP + W$$

where $b = (a_2 + a_3 a_1)$ and $W = a_3 U_1 + U_2$.

Two features of the reduced form equations for a structural model are of particular interest. First, a structural model and its associated reduced form equations will always be, as far as the available statistical information goes, "observationally equivalent" in the sense described above. Intuitively, this is because, as the above examples illustrated, we obtain the reduced form from the original structural model by a series of algebraic manipulations (substituting equals for equals, adding equal quantities to both sides of an equation, and so on) that leave us with a new set of equations having exactly the same set of solutions for the values of exogenous and endogenous variables X and Y as before. Since the same sets of values for these variables will satisfy both systems of equation, observations of how X and Y are associated — i.e., observation of their joint probability distribution — cannot distinguish between the original structural model and its reduced form. Of course, it may be the case that extra-statistical assumptions may be used to discriminate between these models — see this section below.

Second, by construction, the exogenous variables on the right hand side of each reduced form equation will be uncorrelated with the error term in that equation. Hence, we can always use OLS to yield unbiased estimates of the reduced form coefficients (e.g, b in 3.1.3). Thus, even if a structural model is not identified, the reduced equations associated with the model will always be identified — we can always use information about the variances and covariances among the measured variables to uniquely fix the values of the reduced form coefficients. We can thus think of the reduced form equations as embodying all of the statistical information (that is, the information about variances and covariances of the variables figuring in the model) associated with a given structural model. The question of whether the original structural model is identified then comes down to this: suppose one were given the reduced form equations associated with a structural model. Only if one can infer the coefficients in a particular structural equation from the reduced form coefficients and other available background information will the structural equation be identified. If, on the contrary, several different sets of structural equations will yield the same reduced form equations, the structural model will not be identified.

Thus in the case of the system (3.1.1–3.1.2), the coefficient a_1 is just the coefficient in (3.1.1) and is thus identifiable from the data by OLS, assuming that the usual distributional assumptions are satisfied. By way of contrast, the reduced form coefficient a_1 and b are *not* sufficient to fix a unique value for structural coefficients a_2 and a_3 since an infinite number of different possible pairs of values of a_2 and a_3 are compatible with the relationship $b = a_2 + a_3 a_1$.

How, if at all, can this underdetermination problem be solved? The conventional approach involves combining the (statistical part of the) "observational" information contained in the reduced form coefficients with additional information about the coefficient matrix B and about the error matrix U in (3.1.6) — these are commonly called "identifying restrictions." For example, one might know that certain elements in the coefficient matrices are zero or have some other specific value or that certain relationships hold among these elements (e.g., that certain coefficients must be equal) or that certain terms in the error matrix are zero (i.e., that the error terms in two distinct equations are uncorrelated).

As an illustration, suppose that one somehow knew that the value of the coefficient a_3 in (3.1.3) was equal to zero. This amounts to the information that P does not directly cause V, but only indirectly, via its influence on E. Then, from (3.1.3) and the fact that a_1 can be estimated from (3.1.1) it follows that we can obtain the value of a_2 from this information and from b, the reduced form coefficient, which can be estimated by OLS. Obviously such identifying restrictions again will be "extra-statistical" in the sense that they have some other source besides the statistical relationships among the variables we are able to measure. Again, just as in the case of regression, it will be natural to think of this additional information as often based on domain-specific causal information of some sort — e.g. information to the effect that certain variables do not causally influence others or, in the case of the assumption that the error terms in certain equations are not correlated, claims to the effect that the causal structures that might generate such a correlation (e.g. an omitted and unmeasured common cause) are absent.

3.5 Directed Graphs

So far we have been using systems of equations to represent causal relationships. It will be helpful at this point to introduce a second device for representing causal relationships: directed graphs. The representational convention governing the use of such graphs is very simple: an arrow (or directed edge) is drawn from X to Y if and only if X is a direct cause (also called a *parent*) of Y. For example, the causal structure that is represented by the system of equations (3.1.1–3.1.2) is also represented by the following directed graph:
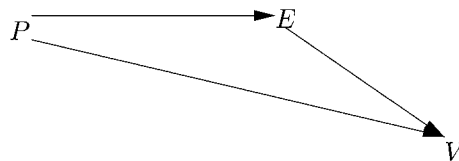
There is thus a very close correspondence between the use of equations to represent causal relationships and the use of directed graphs. The major difference is that while systems of equations explicitly represent the functional form of the relationship between causes and their effects, directed graphs do not represent this

information. In effect, they represent only the fact that effect variables are some function or other of their direct causes, without telling us what this function is. In this respect, directed graphs are less informative than systems of equations. In other respects, however, they may have certain representational advantages. In the case of complex causal systems, it is often easier to parse or comprehend the structure of the system when it is represented by a directed graph than when it is represented by a system of equations. Moreover, as we shall see (Section 4), there are important systematic relationships between graphical representations of causal structure and independence relationships that can serve as a basis for causal inference.

3.6   Causal Interpretation in Systems of Equations

Our attention so far has been focused on the question of how one might estimate the values of the coefficients in the system of equations that correctly represents some causal structure, given that there are other systems of equations that are in some sense "observationally equivalent". There is, however, an obvious prior question: what does it even mean to say that one such system correctly represents causal structure (is "causally correct" or "structural"), given that observationally equivalent systems are available? Unless we can provide an answer to this question, the estimation problem and the associated problem of identifying the correct structure from an equivalent class of structures makes little sense. We have already noted that the system (3.1.1–3.1.2) can be rewritten as the observationally equivalent (3.1.1–3.1.3). Since the second set of equations is obtained from the first by a series of equality preserving algebraic transformations both have exactly the same solutions in P, E and V. Yet, by the interpretive rules given earlier, (3.1.1–3.1.2) and (3.1.1–3.1.3) make (apparently) different causal claims. ((3.1.1–3.1.2)) say that P directly causes E and that E and P directly cause V. By contrast, (3.1.1–3.1.3) says that P directly causes E and that P directly causes V but says nothing about a direct causal connection between E and V. As yet another possibility, we could rewrite (3.1.1–3.1.2) as

(3.4.1) $$P = cE + W, \text{ where } c = 1/a_1, W = -1/a_1 U$$

(3.4.2) $$V = dE + R \text{ where } d = a_2/a_1 + a_3, R = -a_2/a_1 U_1 + U_2$$

These equations represent E as a direct cause of P and of V.

In view of these facts, it is natural to wonder in what sense (if any) these various systems really represent alternative causal possibilities. Herbert Simon expresses this concern very clearly when he writes:

> An important objection [to the idea that pairs of equations like those above represent different causal possibilities is] that it is essentially artificial, since the same set of observations could be represented by

different structures with causal orderings of the variables. If [pairs of equations like those above] are to be regarded as operationally equivalent, because each can be obtained from either of the others by algebraic manipulation without altering the solution, then causal ordering has no operational meaning. [1977, 8]

One way of responding to Simon's worry is to pursue the same general interpretive strategy employed above, associating different systems of equations with different claims about what would happen under various hypothetical interventions and taking these differences to capture the operational meaning of a causal model. In other words, if different structural models make different claims about what would happen under interventions, we may think of them as representing distinct ways the world might be.

3.6.1 Representation of Interventions in Systems of Equations

To explore this idea, it will be useful to describe more explicitly how the notion of an intervention may be represented in the context of systems of equations.[6] Consider the correlation between purchase of life insurance P and longevity L discussed above and suppose that this correlation is due entirely to the operation of a common cause SES (S) which causes both P and L. We may represent this structure by means of the following two equations

(3.4.3) $$P = aS + U$$

(3.4.4) $$L = bS + V$$

Suppose now that an intervention is performed that manipulates P. As suggested above, this might be accomplished by randomly assigning different levels of insurance to subjects in a way that is independent of their SES. The effect of this random assignment will be that the value of P is now entirely set by the intervention, rather than by the SES of the subject, as previously. Furthermore, if the intervention is genuinely ideal and the causal structure is as represented by (3.4.3–3.4.4), the intervention should not disturb the relationship between S and L — that is, S should continue to have whatever effect it has on L as represented by (3.4.4), just as before the intervention occurred. This leads us to what is sometimes called the "equation wipe-out" conception of interventions: to represent an intervention on a variable X occurring in a system of equations, we replace the equation in which X occurs as dependent variable with a new equation $X = k$, specifying that the value of X has been set to a new value by the intervention and is no longer influenced by whatever variables determined its value in the original system. All other equations in the system remain undisturbed but we assume that

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[6]This basic idea for the representation of interventions goes back to at least Strotz and Wald [1960], and is discussed in detail in Spirtes, Glymour, and Scheines [2000], and Pearl [2000].

X has the value to which it has been set by the intervention in whatever equations it occurs as an independent variable and that this value makes whatever contribution is specified by the remaining equations to the other variables in the system — in other words, the new value of X propagates through the system in whatever way is indicated by the system of equations. For example, the effect of an intervention on P is to replace (3.4.3) with $P = k$ while (3.4.4) remains undisturbed. Similarly, the effect of an intervention which sets $E = k$ in the system (3.1.1–3.1.2) is to replace equation (3.1.1) with $E = k$ while leaving (3.1.2) undisturbed. Given this intervention, the value of V is determined by the new value of $E(= k)$ and by whatever value P takes as a result of the intervention. (Note that the value of P is not affected by the intervention on E.) Thus, an intervention that changes E by amount dE should change V by amount $a_3 dE$.

With this way of thinking about the effect of interventions, we can see the sense in which the observationally equivalent systems nonetheless represent distinct causal possibilities. In contrast to the prediction made by (3.1.1–3.1.2) about the effect of an intervention on E, if (3.1.1–3.1.3) is the causally correct representation, then an intervention on E should not change V at all, since according to this representation there is no causal link from E to V.

Let us consider this idea in more detail. Why (or under what circumstances) is it justifiable to assume that an intervention on, e.g., P in the common cause structure $P \leftarrow S \rightarrow L$ will leave the relationship (3.4.4) undisturbed? A natural line of thought (more or less implicit in some of the causal modeling and econometrics literature) is this: We suppose that when a set of equations is fully causally correct (or provides a fully perspicuous representation of some causal structure) each separate equation represents a distinct causal mechanism or relationship. Then, if such mechanisms/relationships are genuinely distinct, it should be possible in principle (if not in actual practice) to interfere with each separately while leaving the others undisturbed.[7] For example, if (3.4.3–3.4.4) is causally correct, then what it claims is that the mechanism by which SES affects longevity is distinct from the mechanism by which SES affects the decision to purchase insurance. If so, it should be possible (again, in principle — that is, it should be a sensible "thought experiment") to do something that alters the latter relationship (e.g. by assigning experimental subjects different levels of insurance independently of their SES) and SES will continue to affect longevity just as before. If, on the contrary, we have no way of altering (even in principle) the former relationship without at the same time altering the latter, this would be an indication that those relationships are not really distinct and that the representation (3.4.3–3.4.4) does not really capture or correspond to whatever mechanisms or relationships generate the pattern of covariation among P, S, and L that we actually observe.

From this perspective, a set of equations that is not fully causally correct provides a representation that entangles, collapses, or otherwise misrepresents distinct causal relationships. For example, if (3.1.1–3.1.2) is the correct representation of

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[7]For additional discussion of this idea, see [Woodward, 1999; 2003], where this ideal of independent disruptability is called "modularity".

the causal relationships among P, E and V, then this system consists of two distinct mechanisms — a mechanism connecting P to E and a distinct mechanism linking both P and E to V. By intervening on E we disrupt the former mechanism but not the latter. Assuming that (3.1.1–3.1.2) is the correct representation, then if instead we adopted the representation (3.1.1–3.1.3) we would collapse the two distinct mechanisms by which P influences V (the direct route represented by 3.1.2 and the indirect route by which P influences E which in turn influences V) into a single mechanism represented by 3.1.3 alone.

Suppose that we adopt the representation (3.1.1–3.1.3) and that we intervene in such a way as to alter the value of the coefficient a_1 in (3.1.1) — for example, we intervene on E, which amounts to setting $a_1 = 0$. Since the coefficient b is a function of the value of a_1, among other arguments, b will also change under this intervention. Thus from the perspective of someone employing the representation (3.1.1–3.1.3), the relationship between V and P (and hence the value of V associated with a fixed value of P) represented by (3.1.3) will seem to change under this intervention on E. This is the sort of behavior that we would ordinarily (within the framework of a manipulationist theory) take to be evidence that there is a causal relationship between E and V, but (3.1.1–3.1.3) does not represent any such relationship.

The representation (3.1.1–3.1.3) has the feature that its coefficients (and the functional relationships it represents) are "entangled" or not independently changeable, in the sense that both coefficients a_1, b are functions of the same true coefficient (a_1). By way of contrast, when a system of equations is causally correct, the equations themselves (and in the linear case the coefficients in those equations) will be independently changeable. Such systems will correctly and completely describe the results of interventions on all of the endogenous variables in the representation. A moment's thought will show that at most one representation among all those that are observationally equivalent will satisfy the requirement of independent changeability of equations (or of correct representation of the results of interventions on all endogenous variables). These requirements single out in, Alderich's [1989] words, a "privileged parameterization" from the class of observationally equivalent representations.

An important disadvantage of a system of equations that are not independently changeable (the equations of which are characterized by parameters that are combinations of the parameters that characterize the causally correct system) is that it does not allow us to track local changes in the system. For example, if (3.1.1–3.1.2) is the causally correct representation but we are unaware of this and instead employ the representation (3.1.1–3.1.3), then if a change occurs in the relationship between P and E in (3.1.1) — that is a change occurs in the value of a_1 — we will also see a change in the value of b in (3.1.3) but the representation (3.1.1–3.1.3) gives us no way of predicting what this change will be. From the point of view of (3.1.1–3.1.3) this local change in a_1 ramifies through the whole system of representation in an unpredictable and holistic way. By way of contrast, if we possess the causally correct representation (3.1.1–3.1.2), then if we have good

reason to believe that some change has affected only a_1 in 3.1.1, we can retain
the rest of the representation (3.1.2) intact and use it to track the effects of the
change. Thus, on this understanding of what a causally correct representation is,
such representations will have a kind of "modularity" property that is not shared
by other observationally equivalent representations.

These remarks may also help to clarify what is meant when we say that differ-
ent representations are observationally equivalent. (3.1.1–3.1.2) and (3.4.1–3.4.2)
are observationally equivalent in the sense that they will both correctly represent
the (so far) observed pattern of covariation among the measured variables. What
they disagree about is the predictions they make about what would happen un-
der certain counterfactual possibilities, that is, about what would happen were
various interventions (or other kinds of changes — see below) were to occur. If
the relevant interventions do occur in the future, there is of course an obvious
practical utility to knowing which among the observationally equivalent models
is causally correct, but even if the relevant interventions do not in fact occur, so
that different observationally equivalent models continue to adequately represent
what actually happens, we take there to be a fact of the matter about what would
happen should these interventions occur and it is this that competing claims about
causal structure attempt to capture.

Finally, let us return to the question of what the individual coefficients (and
the associated notion of "direct cause") mean in a system of structural equations.
A natural way of explicating these notions is to appeal to *combinations* of inter-
ventions. In particular, consider an equation of form $X_i = a_{ij}X_j + \ldots$ occurring
in a system of equations. If this equation is causally correct, then there is some
set of values for all other variables besides X_i and X_j in the system such that if
we were to fix those variables at those values by interventions and independently
intervene to change the value of X_j, the value of X_i would change in just the way
represented by the coefficent a_{ij}. For example, the coefficent a_2 relating P and V
in the system (3.1.1–3.1.2) tells us (assuming that the system is causally correct)
that if were to intervene to fix the value of the remaining variable V, then (for at
least some values of V), an intervention that changes P by amount dP will change
V by $a_2 dP$.

3.6.2   *Graphical Representations of Interventions*

The "equation wipe out" way of representing the effect of interventions within a
system of equations has a close counterpart within graphical representations of
causal structure. An intervention I on a variable X in a directed graph may be
represented by "breaking" or removing the arrows previously directed into X and
replacing them with a single arrow from I to X. All other arrows in the graph
remain undisturbed. (cf. [Pearl, 2000; Spirtes, Glymour, and Scheines, 2000])
Thus an intervention on P in the (3.4.2–3.4.3) may be represented as an operation
in which the directed graph
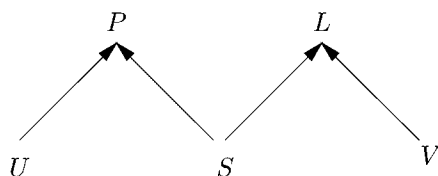
is replaced by
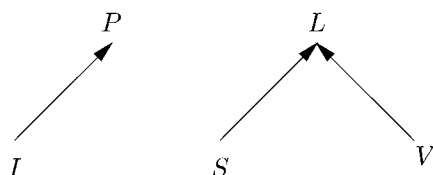
Similarly an intervention on E in the structure associated with (3.1.1–3.1.2) replaces
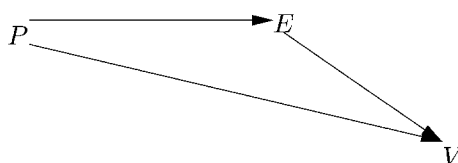
with
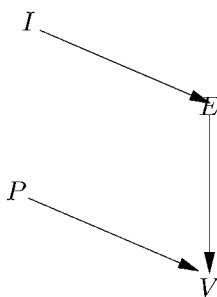
As before, the underlying idea is that an intervention amounts to a sort of local surgery in which the variable intervened on is brought entirely under the control of the intervention, severing the causal links that previously determined its value, but everything else in the graph is left undisturbed.

3.7   Uncorrelatedness and OLS Estimation Again

Let us now return, in the context of systems of equations, to some of the issues about the role of the uncorrelatedness assumption and the conditions for OLS estimation discussed in Section 2.5. We have seen that in systems with causal loops

like (3.1.4–3.1.5), the demand that the errors for each equation be uncorrelated with the independent variables in that equation can not be satisfied. Nonetheless we have argued that such equations can be given a coherent causal interpretation. Indeed, although one cannot use OLS to arrive at unbiased estimates of the coefficients in such systems, other techniques (two stage least squares, use of various exclusion restrictions, as well as information about the variance–covariance matrix of the errors) may, under the right circumstances permit estimation of the coefficients. This reinforces the conclusion, reached in Section 2.5, that satisfaction of the uncorrelatedness condition is *not* a necessary condition for a system to have a causal interpretation. Instead, the role of the uncorrelatedness condition is better conceptualized as a necessary condition for one particular sort of estimation procedure among many possibilities — OLS — to be unbiased.

Reflection on reduced form equations suggests a similar conclusion. As we have seen, it is always possible to associate a system of equations, regardless of its structure, with a set of reduced form equations. In the reduced form system, the assumption that the error term in each equation is uncorrelated with the r.h.s variables in that equation must be satisfied and thus the coefficients in such equations can always be estimated by OLS — the system will always be identified. Thus, if satisfaction of the uncorrelatedness assumption (or identifiability) is a *sine qua non* for causal interpretation, it is not clear why researchers should not content themselves with estimating reduced forms. The fact that they often do not shows that they do not regard the estimation of reduced form equations as all there is to the discovery of causal structure.

4   CAUSAL INFERENCE WITHOUT DOMAIN SPECIFIC BACKGROUND KNOWLEDGE: THE SGS PROGRAM

4.1   Introduction

Our focus so far has been largely on conventional procedures for causal modeling of a sort that are discussed in standard textbooks. As we have seen, these assign a large role to domain specific causal background knowledge in causal inference. However, as we have also noted, often when causal modeling techniques are used in social sciences, such background knowledge is not sufficiently precise or detailed to allow researchers to distinguish between competing, observationally equivalent models. For example, available social science background knowledge does not tell us which of the many different sets of possible regressors are the appropriate ones to use in the investigation of the deterrent effects of the death penalty conducted by Leamer, and, as we have seen, different choices of regressors lead to quite different estimates of this effect. Moreover, if Leamer had considered more complex causal structures that could not be represented by a single regression equation, this underdetermination problem would have been even worse. In practice, social science researchers often consider some very small set **M** of possible causal models from the much larger class of models that are observationally equivalent relative to

the observed statistical data. Sometimes only a single favored model is estimated and alternatives are not even considered. Typically, to the extent that a case is made that some particular model is superior (according to some favored criterion) to competitors, only a few competitors from the full set observationally equivalent alternatives are considered.

It thus would be very desirable to have more systematic procedures for generating the full set of alternative models that are consistent with a body of statistical data and for searching among these for those that (according to some criterion) are best supported by this data. Ideally, such procedures should not require highly specific causal background assumptions. A very interesting and ambitious research program designed to accomplish this has been developed by a number of researchers, including in particular a group at Carnegie-Mellon consisting of Peter Spirtes, Clark Glymour and Richard Scheines, (hereafter SGS).

4.2   The Causal Markov Condition and Faithfulness

In the SGS framework, systems of causal relationships are represented by directed graphs, along the lines described above. Associated with each directed (or "causal") graph G is a probability distribution P over the vertices V in G. SGS make two fundamental assumptions connecting the structure of G to the probability distribution P. The first is the so-called Causal Markov Condition (**CM**):

> G and P satisfy **CM** if and only if for every subset **W** of the variables in V, **W** is independent of every other subset in **V** that does not contain the parents of **W** or descendants of **W**, conditional on the parents of **W**.

The parents of a variable X are just its direct causes — that is, those variables in **V** which are connected to X by means of a directed arrow into X. The descendants of X are just its effects, either immediate or indirect — that is, those variables Y for which there is a directed path from X to Y. Thus **CM** says that conditional on its direct causes, any subset **W** of variables in **V** is independent of all other such subsets, except for subsets of variables that are effects of **W**.

CM is a generalization of the familiar screening off or conditional independence relationships that a number of philosophers, (beginning with [Reichenbach, 1956] and including, for example, [Salmon, 1971], but see, in contrast, [Salmon, 1984]) have taken to characterize the relationship between causes and probabilities. For example, **CM** implies that if X and Y are joint effects of a direct common cause C and X and Y have no other direct causes, then conditional on C, X and Y are independent: C screens X off from Y. Similarly **CM** implies that if X is a cause of Z which is in turn the only direct cause of Y, then Z will screen X off from Y. **CM** also implies that if X does not cause Y, Y does not cause X, and X and Y share no common cause, then X and Y will be unconditionally independent.

Under what conditions is it plausible to assume **CM**? As SGS [2000, 32] note, if a system is deterministic or (what they call) "pseudo-indeterministic" (very

roughly, this means that there is an unknown underlying system D which is de-
terministic and representable by a directed acyclic graph but the representation R
with which we are working contains only some of the variables in D so that R looks
"indeterministic" because of omitted variables) and the exogenous variables are
independently distributed, **CM** must hold. In the non-deterministic case, there
are a number of conditions under which **CM** may fail or in which its application
may be at least unclear. These include (but are not limited to) cases in which
the variables in **V** are drawn from a mixture of different subpopulations with dif-
ferent probability distributions, cases in which those variables are measured in an
inappropriately coarse-grained way which collapses what from a more fine-grained
perspective are causally relevant differences for some effect variable in **V**, cases in
which reciprocal causation or causal loops are present, and cases in which the ob-
served values of the variables in **V** are the result of some kind of "selection bias".
(For more detailed discussion, see [SGS, 2000 pp 32ff; Hausman and Woodward,
1999]) Whether it is reasonable to expect that **CM** will hold for indeterministic
systems that do not fall into these well-known categories of exceptions is a mat-
ter of current debate , with some (e.g., [Cartwright, 2002]) contending that **CM**
regularly fails under indeterminism, even apart from the exceptions just enumer-
ated, and others (e.g., [Hausman and Woodward, 1999]) arguing that **CM** is a
reasonable default assumption in such cases.

The second general assumption linking causal structure and probabilistic infor-
mation that SGS employ is the assumption of

> **Faithfulness**: A graph G and associated probability distribution P
> satisfy the faithfulness condition if and only if every conditional inde-
> pendence relation true in P is entailed by the application of the Causal
> Markov condition to G [2000, 31].

As an example of what faithfulness rules out, return to the structure represented
by the equations (3.1.1–3.1.2) and suppose that $a_2 = -a_1 a_3$. Under this condition,
there will be exact cancellation of the influence of P on V along the direct path
from P to V and the indirect path that goes through E. So assuming that the
errors are independent, P and V will be independent. Nonetheless, if one considers
just the graphical structure associated with this example:
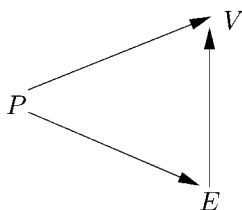
the Causal Markov condition applied to this graph does not entail that P and V
are independent. There is thus a violation of faithfulness: the independence of P

and V is the result of the special values that happen to be taken by parameters a_1, a_2, and a_3 rather than any facts about the structure of the graph.

Why (or when) is the faithfulness condition a reasonable assumption? SGS show [2000, 41] that in the linear case, and for certain other classes of functions, the set of cases in which faithfulness is violated has measure zero, under some natural measure over the range of possible parameter values. Applied to the example above, the intuition is that if one supposes that the parameters a_1, a_2, a_3 can take any one of a substantial range of possible values, cases in which there is exact cancellation in accord with $a_2 = -a_1 a_3$ will be rare; for most possible combinations of values of these parameters, this equality will fail to hold. Of course, it is natural to wonder exactly what this shows: some will think that unless the measure zero result translates into some result to the effect that independence relations resulting from special parameter values have objective probability of zero or nearly zero, we have been given no reason to adopt faithfulness as a principle of casual inference. Similar issues arise in other contexts in which measure zero results are available and are claimed to show that certain possibilities are "improbable"; rather than considering such issues further, we will instead explore what can be accomplished if faithfulness (and **CM**) are assumed[8].

To set the stage for this, some further observations and terminology will be helpful. Consider a causal structure in which X and Y are both causes of a third variable Z, with no other causal relationships present among these variables.
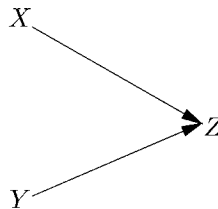
If this graph satisfies the Causal Markov condition, X and Y will be independent. It is also true (although considerably less intuitive to many people) that X and Y are *dependent* conditional on Z. This is easy to see in the deterministic case: if Z is a function of X and Y, then, given the value of Z, the value of X conveys information about what the value of Y has to be. A similar conclusion follows in the probabilistic case: if there are changes in the value of X and Y that make a difference to the probability distribution of Z then, given the value of Z, the values of X and Y convey information about each other.

A structure like (4.3.1) is called an *unshielded collider* by SGS — it is a *collider* because the arrows from X and Y collide at Z and it is unshielded because there is no arrow from X to Y or vice-versa. By contrast

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[8]For example, similar issues arise in connection with the "improbability" of the initial conditions governing the early universe — see [Earman, 1995], for a penetrating discussion.
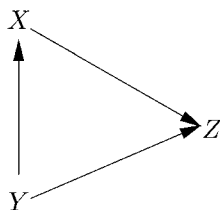
is a *shielded* collider. Consider now a three variable structure in which X and Z are dependent, both unconditionally and conditionally on Y, Y and Z are dependent, unconditionally and conditionally on X, and X and Y are unconditionally independent but dependent conditional on Z. It is an important fact that, assuming the causal Markov and faithfulness conditions, the only causal structure compatible with these probability relationships is 4.3.1 — that is, the structure in which Z is an unshielded collider for X and Y.

SGS say that two directed acyclic graphs, G and G' are *faithfully indistinguishable* if and only if every probability distribution faithful to G is faithful to G' and vice-versa. Intuitively, if two graphs are faithfully indistinguishable, they are indistinguishable given the statistical data, assuming both the Causal Markov and faithfulness condition. Thus any inference algorithm based on these conditions will not be able to distinguish faithfully indistinguishable graphs. It is an important fact (cf. [SGS, 2000, 61, Theorem 4.2]) that two directed graphs G and G' are faithfully indistinguishable if and only if (i) they have the same vertex set, (ii) any two vertices are adjacent in G if and only if they are adjacent in G' and (iii) any three vertices X, Y and Z such that X is adjacent to Y and Y is adjacent to Z but X is not adjacent to Z in G and G' are oriented as $X \rightarrow Y \leftarrow Z$ in G if and only if they are so oriented in G'. In other words, G and G' have the same unshielded colliders. (In a directed graph, two vertices X and Y are adjacent if and only if there is a directed edge from X to Y or from Y to X. If we form an undirected graph by removing the arrowheads from a directed graph, then adjacent vertices will be connected by an undirected edge.)

4.3   The SGS Algorithm

These observations motivate the algorithms SGS have devised for inferring causal structure. I will follow SGS's own exposition in describing an algorithm (which they call the SGS algorithm) which is computationally infeasible but which illustrates clearly the basic inferential strategy they employ. (They go on to describe other algorithms, such as the PC algorithm which has the same input/output relations as the SGS algorithm but which is more computationally efficient.)

SGS Algorithm: [2000, 82]

    1. Form the complete undirected graph H for the vertex set **V**. A complete graph is a graph in which every vertex in **V** is connected

to every other vertex. Recall that an undirected graph is a graph which contains only undirected edges — that is, only adjacency relationships are represented.

2. For each pair of vertices A and B test whether A and B are independent conditional on all other subsets of variables in **V**. If so, remove the undirected edge between A and B

3. Step 2 will result in a new undirected graph which may be sparser (i.e. have fewer undirected edges) than H. Call this graph K. Consider each triple of vertices A, B, and C in K, such that A and B are adjacent, B and C are adjacent, but A and C are not adjacent. Orient such triples as $A \rightarrow B \leftarrow C$ if and only if there is no subset **S** of variables in **V** that contains B but not A and C such that A and C are independent conditional on **S**. Here we are making use of the fact about conditioning on an unshielded collider mentioned above.

4. If $A \rightarrow B$, B and C are adjacent, A and C are not adjacent, and there is no arrowhead at B, orient B–C as $B \rightarrow C$. If there is a directed path from A to B and an edge between A and B, orient A–B as $A \rightarrow B$.

5. Repeat until no more edges can be oriented.

The output of this algorithm is a class of directed graphs which are faithfully indistinguishable. SGS argue, on the grounds described above, that it is a very desirable feature of an inference procedure that it output the entire class of structures which are (relative to the evidence and the assumptions built into the procedure) indistinguishable or equivalent rather than a single structure.

SGS observe [2000, 80] that the SGS algorithm as well as a number of other algorithms they described will provably recover graphs faithful to the population distribution if certain assumptions are met: (4.3.1 i–iv)

i.   The set of observed variables is causally sufficient (This means that there are no omitted variables which are common causes of the observed variables or that if there are such unmeasured common causes, they take the same value for all units in the population, [2000, 22].

ii. Every unit in the population has the same causal relations among the variables

iii. The distribution of the observed variables is faithful to a directed acyclic graph representing the causal structure.

iv. The statistical decisions required by the algorithm are correct for the population.

Under these assumptions the correctness of the SGS algorithm is implied by the following theorem:

> If P is faithful to some directed acyclic graph, then P is faithful to G
> if and only if (i) for all vertices, X and Y of G, X and Y are adjacent
> if and only if X and Y are dependent conditional on every subset of
> vertices of G that does not include X and Y and (ii) for all vertices
> X and Y such that X is adjacent to Y and Y is adjacent to Z and X
> and Z are not adjacent, $X \rightarrow Y \leftarrow Z$ is a subgraph of G if and only if
> X, Z are dependent on every set containing Y but not X or Z. [2000,
> 82, Theorem 3.4]

The SGS algorithm uses clause (i) in this theorem to establish adjacency relationships. Clause (ii) is then used to orient these adjacency relationships, insofar as this is possible.

As an illustration of how the algorithm works, suppose that we are given the following (in)dependence relationships (where $X \perp Y$ means that X is unconditionally independent of Y, $X \not\perp Y$ means that X is not independent of Y, and $X \perp Y / Z$ means that X is independent of Y conditional on Z):

> $W \not\perp A$ for all subsets of U, C, X, and Y
> $U \not\perp A$ for all subsets of W, C, X, and V
> $W \perp U$
> $W \not\perp U$ conditional on all subsets of the remaining variables containing A
> $A \not\perp C$ for all subsets of W, U, X, and V
> $X \not\perp C$ for all subsets of W, U, A and V
> $V \not\perp C$ for all subsets of W, U, A and X
> $X \perp V$
> $X \not\perp V$ conditional on all subsets of remaining variables containing C
> $A \not\perp X$ conditional on all subsets of the remaining variables containing C
> $A \not\perp V$ conditional on all subsets of the remaining variables containing C.

Other pairs of variables independent conditional on at least some sets of other variables

From this information, we can infer, first, that the adjacency relationships must be the following:
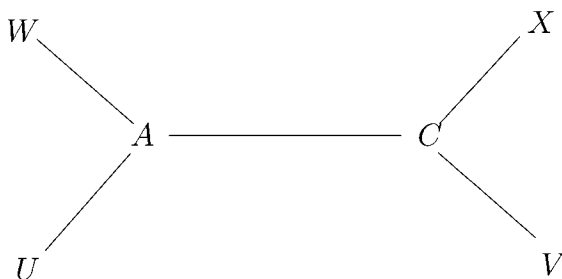
Second, from the non-adjacency of W and U, the fact that U and A and W and A are adjacent and the fact that W and U are dependent conditional on all other

subsets of variables containing A we can infer that W, U and A are oriented as $W \rightarrow A \leftarrow U$. Similar reasoning shows that X, V and C are oriented as $X \rightarrow C \leftarrow V$ and that A, C and X are oriented as $A \rightarrow C \leftarrow X$. The only graph that is consistent with these adjacency relationships and orientations is:
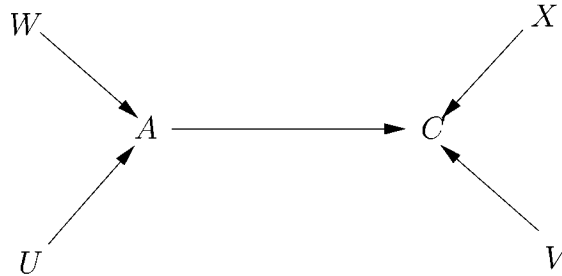
Thus in this particular case, the output of the SGS algorithms will be a single, unique causal structure. In other cases, as noted above, the output will be a class of structures that are not distinguishable by the algorithm. For example, given the information that X and Z are dependent, both unconditionally and conditionally on Y, that Y and Z are dependent, both unconditionally and conditionally on X, and that X and Y are unconditionally dependent but independent conditional on Z, these relationships could be generated by any one of the following causal structures (assuming CM and faithfulness):

$$X \rightarrow Z \rightarrow Y \quad X \leftarrow Z \rightarrow Y \quad Y \rightarrow Z \rightarrow X$$

(Note that all three graphs have the same adjacency relationships and the same unshielded colliders — namely, none and hence are faithfully indistinguishable).

We thus see that even in the absence of specific background knowledge, the algorithms devised by SGS can infer, from information about unconditional and conditional (in)dependence relationships among some set of variables, the class of causal structures involving those variables, assuming the causal Markov and faithfulness conditions. Often, there will be a number of different causal models in this equivalence class. In this sense, there is, as SGS, see it, something right about the widely accepted claim that statistical information underdetermines causal structure, that correlation does not imply causation, and so on. On the other hand, as we have seen, assuming **CM** and faithfulness, statistical information can at least cut down on or restrict the set of causal models that are consistent with that information. Indeed, it may well turn out that all such models share some causal structure (e.g. in all of them X may be a direct cause of Y) even if they differ at other points in the causal claims they make. Moreover, in special cases, a single causal model may be consistent with the statistical information, as the above example illustrates. In such cases, although not in general, correlational information does indeed imply (in conjunction with other domain general assumptions) causal conclusions.

The assumptions 4.3.1 are obviously rather restrictive and are not satisfied by many realistic examples of social science data sets. Among other things, the assumption (4.3.1i) of causal sufficiency is often violated — in real life examples, data often reflect the influence of omitted common causes of the observed variables. Are there methods of causal inference that are reliable in such circumstances? SGS, [2000] describes additional algorithms according to which, again assuming the Causal Markov and faithfulness conditions, "reliable causal inferences can sometimes be made from appropriate sample data without any prior knowledge as to whether the system of measured variables is causally sufficient" [124–5]. Among other things, in the appropriate circumstances these methods can allow researchers to detect the presence of unmeasured common causes. The reader is referred to SGS, [2000, Ch. 6] for details.

5   CONCLUSION

What can be concluded from this long survey? It is indeed possible to make reliable causal inferences from non-experimental data, if (but only if) various additional "extra-statistical" background assumptions, described above, are met. These assumptions are strong and restrictive and (at least in many cases) are "causal" in character, thus vindicating the common claim that reliable causal conclusions require causally committed assumptions as input (cf. [Cartwright, 1989]). Whether the relevant assumptions required for the reliable use of some causal modeling technique are satisfied in any given case will depend, of course, on the details of that case. Nonetheless, it seems plausible that in many of the contexts in which such models are used, the background assumptions that may be reasonably taken as known are not strong enough, in conjunction with the observed statistical data, to single out a single causal model (or perhaps even a small set of such models). In such contexts, researchers would do well to avoid the common practice of estimating a single favored model and instead consider a larger class of models that are consistent with the observed statistics and to look for other sources of evidence that may help to discriminate among them.[9] If it is not possible to do this, a substantial measure of skepticism toward any particular model seems justified.

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[9]For examples of the use of such additional evidence see [Achen, 1982; Woodward, 1995].

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FUNCTIONAL EXPLANATION AND EVOLUTIONARY SOCIAL SCIENCE

Harold Kincaid

1 INTRODUCTION

From their conception to the present, the social sciences have invoked a kind of explanation that looks suspect by the standards of the natural sciences. They explain why social practices exist by reference to the purpose or needs they serve. Yet the purposes invoked are generally not the explicit purposes or needs of any individual but of society or social groups. For example, Durkheim claimed that the division of labor in society exists in order to promote social solidarity and Marx thought that the state served to promote the interests of the ruling class. Social scientists have found these explanations as irresistible as their critics have found them mysterious. This chapter traces the controversies over these explanations — generally called functional explanations — and argues that they are widespread in some of our best current social science and that they can provide compelling information in some cases, despite the many doubts about them.

Section 2 surveys the general usage of functional explanations from classic sociologists to the present and common doubts about them. Section 3 surveys past accounts of how functional explanations work and their problems and develops two distinct models of functional explanation, selectionist explanations that show how a practice exists in order to bring about its effects and functional role analysis which explain systems in terms of component parts. Sections 4, 5 and 6 discuss specific instances of functional explanation as well as some general mechanisms thought to underlie them. Section 4 looks at norms, institutions and rational choice mechanisms, and 5 and 6 at inequality, organizational ecology, behavioral ecology, and varieties of selectionist mechanisms. Section 7 turns to functional explanation as functional role analysis. Section 8 draws morals and points to open questions.

While the overwhelming focus of this chapter is on functional explanation, I think functional explanation also raises a number of broader issues in the philosophy of the social sciences and philosophy of science more generally. These will surface briefly throughout, and I want to outline them here. I make no pretense to provide a thorough or conclusive discussion, only enough mention to show possible connections.

A long-standing dispute in and about the social sciences is the extent to which large scale social phenomena can be explained in individualist terms.[1] Variants of functional explanation might seem to support the individualist side of this dispute by explaining social phenomena entirely in terms of effects that exist in order to promote various individual traits. Other variants might argue against individualism by seeing groups as the units of selection or by seeing social structure as in some sense prior to individual traits. A related issue concerns the explanatory power of evolutionary psychology and human behavioral ecology: its defenders [Tooby and Cosmides, 1992] and Smith [1981] claim that all of culture, including social organization, can be explained in terms of psychological modules selected for in the Pleistocene.

Not far away from both individualism, evolutionary psychology, and behavioral ecology are questions about the unity of science. Individualism is often put forward as a thesis motivated by the goal of integrating the social and natural sciences; evolutionary psychology and behavioral ecology are given a similar justification. Similarly, those such as Gintis who advocate evolutionary game theory see it as providing a "common language" to unify the social sciences [Gintis, 2000]. An alternative picture depicts scientific unity as a piecemeal affair and doubts that functional explanation in the abstract is likely to suffice to integrate the social sciences.

Finally a broad set of questions about theories, models and explanation surface in debates over functionalism. A deep rooted tradition in philosophy of science and in much science itself sees science as essentially about producing theories and sees explanatory power as resulting from their ability to unify and generalize [Kitcher, 1993]. Models with idealizing assumptions are often an essential part of that process. Skeptics doubt that theories have such a central role, that unification explains, and that models with unrealistic assumptions explain.[2] They advocate an alternative picture that focuses on piecemeal causal explanation which downplays the role of abstract theories and places tight constraints on successful models. Many defenders of functional explanation in some or the other form defend it on the grounds of its ability to provide theories, to unify and to model. Hence these issues are joined in any discussion of functionalism.

2   HISTORY AND CONTROVERSIES

No definitive history of functionalism in the social sciences has been written. This deficit is perhaps due in part to two factors. First any such account would be nearly coextensive with much of the history of the social sciences in total. Moreover, from its inception, functionalism has been ambivalent between two different claims (detailed in the next section) — namely, that society is composed of component parts with specific functions as opposed to the claim that there is some kind of

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[1]For further development of this debate, see the essays by Little and Zahle, in this volume.
[2]See Kincaid [1996; 2004].

selective mechanism ensuring that those social parts exist in order to serve their functions. Different social science traditions emphasize one or the other of these claims for different reasons at different times in their history, making a cohesive narrative difficult. In what follows I impose an order on this messy history.

Most of the key founding figures in sociology and anthropology — Comte, Spencer, Marx, Durkheim, Weber, Radcliff-Brown, and Malinowski — espoused versions of functionalism. Comte [1974], who coined the term sociology, had a fundamental interest in the foundations of social order combined with the common 19th century belief that useful analogies can be drawn between biological organisms and societies. He also had to fight for the legitimacy of sociology as a discipline, and he may have done so by appealing to similarities between the new social science and the much more established science of biology. Organic analogies and a concern with stability thus led naturally to the idea that society was composed of a set of interlocking component social parts — families, classes, and cities were his favorite elements — and that these parts contribute in different ways to the stability of the whole. That contribution was their function.

Missing from Comte is the functionalist emphasis on selective processes, though it was implicit in the idea of contributing to stability. This is not surprising given the pre-Darwinian times. However, another pre-Darwinian, Spencer, brought the selection through competition idea to social explanation (and indeed to biology through his influence on Darwin). Spencer [1975] adopted Comte's organicism. Societies were composed of interdependent parts analogous to those of organisms. He put this in terms that survive to this day: societies have structures (morphologies) and structures have functions; those functions are to meet the needs of the overarching society. He combined this distinction between structure and function with the claim that societal forms follow an evolutionary pattern from simple and undifferentiated to complex and specialized functions as they developed.

These ideas are in Comte, but Spencer develops them in more detail. Grafted on to them, however, was Spencer's defense of laissez-faire. Competition among free, self-seeking individuals produces efficient structures. Implicit here is the claim that those structures not only have a function but exist because they bring them about. Comte might have agreed that social structures exist in order to realize their functions, but the causal process was unspecified. Spencer is the first inkling of a mechanism, though he admittedly was vague on how his selection mechanism actually connected to his other functional claims.

Standard histories (cf. [Turner and Maryanski, 1979]) trace the next developments in functional thinking to Durkheim who then influences Radcliff Brown and Malinowski in anthropology and then all three together set the stage for Talcott Parsons, the grand functionalist theorist of the 20th century. That narrative is too simple, I believe, and results in a diminished appreciation of the place of selection through competition in the functionalist story. To anticipate, a standard criticism of functionalism in its mature Parsonian form is illegitimate teleology, purposes without design. But since the 19th century there have been persistent attempts to develop a naturalized social selectionist account that would undergird

functionalist claims in the social sciences. Some of the best contemporary work, I shall argue later, continues that tradition.

Missing from traditional histories of functionalism are Marx and, more controversially, Baldwin and Weber. Marx's functionalism pervades his core ideas. The "superstructual" aspects of society — the state, ideology, etc. — exist in order to promote the basic class relations characterizing the mode of production. Those class relations in turn exist in order to promote the productive forces. Marx is clear in each case that institutions have a characteristic effect or function that in some sense explains why the institutions exist.

Marx is not altogether clear on the mechanisms behind these explanations, but certainly he is groping toward some sort of competition and selection process. This explains Marx's reaction to Darwin: though Marx's core views were firmly established before the publication of *Origin*, he found in Darwin "a basis in natural science for the historical class struggle" and rational explanation for teleology [1922, 245]. Marx provided the most developed causal mechanism for the claim that class relations exist in order to promote the forces of production. He claimed to see a process whereby further development of the material means of production become limited by the existing social relations, resulting in crises and an ensuring change in social relations. Of course the details, plausibility, and generality of that story is up for grabs, but Marx certainly does try to describe a roughly selective mechanism undergirding this functional analysis.

When it comes to the functional explanation of institutions like the state, things are fuzzier. We do not have the simple fettered growth — crisis — revolution model. Presumably the claim is that those regimes and state forms persist that are consistent with basic class relations and those that are not are eliminated. How this is all supposed to work for Marx is left undeveloped, though subsequent followers have provided more detail.

Another interesting figure if we want to explain functionalism as it is practiced today, especially in anthropology, is James Baldwin. Known now for the possible effect named after him — the Baldwin effect where mutations occur to make an acquired trait have a genetic basis — Baldwin made novel efforts to combine Darwinian natural selection, learning, and cultural change. Organisms learn by imitation, particularly humans. Imitation is not random, but shows a process of differential adaptation. This leads to a form of "social heredity" of culture that is different from genetic inheritance, though it too supports a selective process. This is one of the first clear, relatively rigorous discussions of gene-culture coevolution. When cultural selection returns again later, it will be in the work of Leslie White, who implicitly invokes Marx (he wrote during the heyday of McCarthyism) and explicitly refers to Baldwin.

Max Weber [1978] was openly critical of organic analogies and the unilinear evolution found in the early functionalists. Yet, demonstrating once again the appeal of functionalist accounts and the Darwinian mechanism of variation and selection, Weber's concrete empirical analyses of social phenomena extend the kind of selective mechanism found in Marx and Baldwin [Runciman, 2001]. For exam-

ple, Weber's account of religious development invokes various kinds of selection and retention processes. The exemplary prophet passes on beliefs and values that compete with others in the ensuing generations and are selected by their fit with the needs of individuals. Then competing organized practices compete for power, with success determined by features of the social environment. Similarly, Weber's account of the protestant ethic and its role in the rise of capitalism has at least one reading that calls on similar processes: it was not the upward mobility of individuals from Protestant families per se that lead to change but the set of practices that Protestant family firms and workers brought with them that was crucial to the development of capitalism. They were, as Weber, puts it, the "carriers" of beliefs and values that were more or less adapted to their conditions of life.

Durkheim [1965] is the most influential of the early functionalists. He borrowed Spencer's structure/function distinction, but put it to an empirical use that Spencer did not. In *The Division of Labor in Society* [1933] and *The Elementary Forms of Religious Life* [1961] Durkheim argued that particular social institutions serve to promote social solidarity. The division of labor does so by increasing interdependence among individuals. Religious beliefs symbolize the society in which they are embedded and thereby promote social integration. He backed up both with extensive empirical discussions.

Durkheim's functionalism sometime extended beyond identifying functions to explaining institutions as existing because of their functions. He explicitly separates causes of social phenomena from their functional effects. Yet he appeals to a Darwinian competition mechanism (explicitly invoking Darwin) in discussing the division of labor: increasing population causes increasing competition which results in increasing specialization. It looks like the division of labor exists because it allows societies to successfully function in the face of individual competition. Arguably we have here again the ongoing tension between functionalism as analyzing the role of parts in the whole and as providing a causal mechanism based on those roles.

Durkheim's ideas had a strong influence on two of anthropology's founding fathers, Radcliffe-Brown and Malinowski. Both attributed their basic orientation to him, yet made important elaborations in the functionalist picture. Radcliffe-Brown [1948] emphasized two crucial ideas: the distinction between structure and function and the notion of functional prerequisites. The first step in explaining society is to identify social structure — "the sum total of all social relationships of all individuals at a given time" [1948, 55]. Kinship systems were a prime example of social structure for Radcliffe-Brown (not surprisingly given anthropology's early interest in small scale societies) and they also illustrated his belief that social structure can be further divided into subsystems or roles.

There is a clear sense in Radcliffe-Brown that this project of identifying structure was explanatory in its own right. It required showing how the structures of a given society met its "functional prerequisites" or its "necessary conditions of existence," viz. consistency — a clear definition of roles and rights — and continuity.

The continuing question of whether (1) institutions have functions or (2) have them and exist because of them likewise arises in Radcliffe-Brown's functionalism. He denies that he is explaining historical origins but does claim to account for "sociological origins." Perhaps Radcliffe-Brown is sliding from their positive effects to the conclusion that things exist for those effects. But perhaps not. The key issue may be what "accounting for" comes to. Radcliffe-Brown sees science as essentially about producing laws. He may then think that if he can relate functions and needs in a lawlike way, he has explained them without making a causal claim linking effects and existence.[3] We will see that similar assumptions about explanation surface in more contemporary accounts of functional explanation [Cohen, 1978].

Malinowski [1944] added further important wrinkles and some differences in emphasis. Identifying social structure meant institutional analysis. That required identifying, among other things, relevant norms, material culture, and societal needs met by the practice in question. Institutions served needs that were largely the needs of *individuals* rather than the collective needs of society as in Radcliffe-Brown.

Radcliffe-Brown and Malinowski were important influences on the next and most elaborate functionalism, that of Talcott Parsons. Parsons' functionalism aims to provide a full fledged theory of societies in general. He identifies what he believes to be the essential components or subsystems of any society. The working of these components is then explained in terms of their contribution to social equilibrium. Social equilibrium is maintained when the universal functional needs or prerequisites are met.

This grand vision is fleshed out with a plethora of further nomenclature that becomes increasingly baroque over Parsons' career. But in short form, Parsons' theory is this. There are four basic subsystems in any society: the biological, personality, cultural, and social. The biological system concerns inherent needs, drives, etc. of humans. The personality system involves psychological dispositions, cognitive states, etc. Ideas and symbols form the cultural system. A social system is the pattern of roles and statuses that structure society. Each of these systems must meet four functional needs:

1. adaptation – obtaining resources from the environment

2. integration – maintain coherent relationships among their component elements

3. goal attainment – setting goals and allocating resources to achieve them

4. latency – reproducing organizational structure and managing tensions between units.

Parsons is an "analytic realist" about these categories: they "correspond not to concrete phenomena, but to elements in them which are analytically separable." [1937, 730]

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[3]This is precisely the same kind of analyses advocated by Hempel in roughly the same time period [Hempel, 1970].

Running parallel to Parsons' functionalism in the late 50s were developments in anthropology harking back to the selectionist strain found earlier in Marx, Baldwin and Weber. Functionalism in the guise of Radcliffe-Brown and Parsons had little space for history or change; societies were integrated, self-contained systems. Both tenets were challenged by the rise of cultural ecology or ecological anthropology. Steward [1955] and White [1949] were the most influential early proponents. For both, key issues were how societies and the subsystems composing them adapted to ecological surroundings. The selective processes described by ecologists in biology were relevant to the form and processes of change that described societies. These ideas were the direct inspiration of contemporary functionalism of the sort advocated by Rappaport [1967] and Marvin Harris [1979], both of who provided numerous explanations of social practices in terms of some kind of selective processes.

At the present, various versions of functionalism are alive and well, making it a continuing active research program in the social sciences. I will sketch some representative examples in later sections.[4]

I won't try to sketch the corresponding history of criticism that functionalism generated, for most of the standard criticisms are present in some from the beginnings in the 19th century. Rather let me list some of the most important:

Questions about mechanism and illegitimate teleology: Many [Elster, 1983; Vayda, 1987; Little, 1989] claim that functional explanations do not provide mechanism connecting beneficial practices with their effects and are thus unconfirmed and/or inadequate explanations. Because there is no mechanism, functional explanations end up with a mystical form of teleology — the need for social institutions makes them exist.

Questions about change, diversity and agency [Layton, 1997]: In its Parsonian form, for example, functionalism sees society as a self-maintaining system where social control is emphasized at the expense of individual agency and societal conflict and cultural diversity downplayed in favor of universal societal components.

Questions about illegitimate use of biological explanations [Ellen, 1982; Hallpike, 1986; Fraccia and Lewontin, 1999; Bryant, 2004]. Societies do not have clear boundaries, do not have the equivalent of genes, do not reproduce, do not merely react to their environments but change them, have guided rather than blind change, and are not populations of traits. All of these differences show that biological analogies to natural selection and organismic organization have no legitimate place.

Questions about tautology and vacuousness [Hallpike, 1986]: A persisting charge against functional explanation is its lack of content. This flaw is seen in various aspects of functional explanation. The essence of functionalism seems to be the

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[4]As a rough guide, contemporary work breaks down into traditional sociological theorizing influenced by Parsons [Alexander, 1985; Luhman, 1995], rational choice game theory, evolutionary game theory [Bowles, 2004], cultural selection theory [Boyd and Richerson, 1985; Durham, 1991; Cavalli-Sforza and Feldman, 1981], mimetics [Aunger, 2000], behavioral ecology [Smith, 1992] and evolutionary archeology [Lyman and O'Brien, 1998].

claim that stable societies survive, where to be stable is to persist and thus survive. Or societies are said to adapt, but adapting is given no more content than simply "try to cope with their situation." Neither claim seems to be saying anything nontrivial. Functionalists' predilection for putting societies or their practices into complex and highly abstract classificatory schemes likewise seems nonexplanatory.

3   CLARIFYING ISSUES

This section clarifies what is involved in functional explanations as the prelude to assessing them. There is an enormous literature over many years attempting to analyze talk of functions in general — across the social sciences, the biological sciences, and common sense usages [Allen *et. al.*, 1998]. Though various useful ideas come from this literature, its main project seems to me misguided from the start, for it presupposes an impossible and unhelpful notion of what is to give an account of something. Thus it is necessary to first discuss what we are doing in giving an account of functional explanation.

Standard philosophical approaches have tried to give the individually necessary and jointly sufficient conditions for "has the function of" or "exists in order to." Such a definition is supposed to hold for all uses of these expressions. The evidence for these definitions are linguistic intuitions, usually on the part of the philosopher doing the analyzing. In short, an account is a formal definition of the concept.

This project is suspect for many reasons: (1) There is good empirical evidence from cognitive science that we don't represent our concepts in terms of necessary and sufficient conditions but use prototypes and relative similarity instead [Rosch, 1978]. (2) Even if we did represent our concepts in this way, it is implausible to think that functional language must have the same content regardless of whether it is used in biology, the social sciences, or in every day talk about artifacts. (3) Even if we found necessary and sufficient conditions that matched linguistic intuitions, it is not clear what that tells us about the science. Why should philosophers' intuitions about what they would say tell us anything about the science? It is not even clear that capturing scientists linguistic intuitions about language use would tell us much, for we want to know how functional explanations work and when they are legitimate, not something directly illuminated by even scientists' linguistic intuitions.

Thus in providing an account of functional explanation, I am not providing a definition tested against linguistic intuition. Instead we want some kind of framework that catches the main features of at least some classes of functional explanations and to do so in way that helps us evaluate them. Thus we want to know: How do they work? What kinds of processes in the world do they commit us to? What is their relation to causal explanations? and so on.

A first useful distinction — one that the definitional project seeking a universal definition ignores — is between functional explanations as the citing of roles in a complex system contrasted with functional explanations that claim something

exists in order to do something. If we ask why there is a carburetor on my old Toyota or what it does, we can answer that it serves to provide a proper mix of fuel and air to the pistons. We describe its typical causal inputs and outputs, its causal role in the system that is my car. However, knowing that it has a role does not mean it automatically exists in order to do so. A typical causal effect of my brake pads is to cause the rotors to wear, yet we know they weren't designed in order to cause wear. In general showing that A has the systematic effect B is not the same as offering the explanation A exists because it does B.

We saw in our historical survey that functionalism vacillates, often unconsciously, between identifying functions in the sense of describing social structure vs. explaining social practices as existing because of their effects. So much in Radcliffe-Brown or Parsons is only about describing social structure. Alternatively, some ecological anthropology explains particular social practices as existing because of their effects without any parallel commitment to arguing that there is a cohesive structure to the overarching society and certainly without describing it in systematic detail.

In general, the claim that societal functions exist in order to do specific things is the more controversial of the two, for it seems to commit us to a rather mysterious causal process that is somehow nonetheless seductive. The bulk of this chapter thus focuses on functional explanation in this teleological sense. Nonetheless, functionalism as the description of integrated systems has been quite influential among social scientists and at the same time little discussed by philosophers of social science. In the last section of this chapter I discuss some of conceptual and empirical issues these explanations raise.

There is a third sense of functionalism prominent in the philosophy of mind and cognitive science literature that I should mention to avoid confusions. Proponents of functionalism in these areas argue roughly that the material realization of mental states is not essential to them — it is the input/output relations and the relations to other functional states that make them what they are. A root idea here is presupposed by functional explanations in the social sciences referring to causal roles and to the selectionist accounts given later for "a exists in order to b." The common idea is that causal explanation can proceed at a given level of aggregation without specifying the microlevel physical detail that realizes it. In other words, we can describe causal relations that abstract from details about the components realizing the entities that stand in those causal relations. Both explanations in the social sciences of causal roles and of "existing in order to" will depend upon this assumption, but they are not exhausted by it.

Let's look at several contemporary accounts of the idea that social practices exist in order to bring about their effects. Faia's [1986] *Dynamic Functionalism* makes causal feedback relations central. On this picture a functional explanation shows how A's causing B in turn influences A itself. For example, contacts outside ones social group reduce prejudice which in turn causes greater social contacts. While explanations in these terms are no doubt given with some success in the social sciences, they still do not get at the stronger sense that As exist in order to B.

Feedback relations for Faia are just circular or mutual causation. By Newton's laws of motion, every body in the universe causes and is caused by every other. Thus if feedback warrants a functional explanation, then every physical process in the universe can be functionally explained. Obviously this conception is too wide. Moreover, circular causation is symmetrical — each component can equally be the cause of the other. However, functional explanations in the social sciences are usually not neutral in this way. For example, the state exists in order to promote the interests of the ruling class according to Marx, but that does not commit us to believing that those interests exist in order to promote the state.

Cohen [1978] makes some progress in narrowing the field of feedback relations to those that look more like functional explanations. According to Cohen, functional explanations are a kind of consequence explanation — explaining the existence of something because of the consequences it has. To show that social practice A exists in order to B, we have to show that it is a law that when A has a disposition to produce B, A comes to exist. Cohen uses this account to clarify Marx's functionalism. The relations of production exist in order to promote the forces of production in that it is a law that when new class relations would promote technological progress, they come to exist. This sort of feedback relation presumably is much narrower in application than Faia's mutual causation.

However, Cohen's account is inadequate as it stands. Cohen takes functional explanations to consist in deducing the event to be explained from a specific kind of law. But there are well known problems with the general picture of explanation assumed here. There can be deductions from laws that do not explain because the laws are irrelevant: it is a law that men who take birth control pills do not get pregnant. Similarly, showing that A exists when it causes B does not show that A exists in order to B — the connection might just be accidental. A second problem with Cohen's account is that it requires too much. Larger corporations may exist because they can take advantage of economies of scale. Yet they do not automatically get larger when doing so would have that effect — lack of foresight, resources, etc may prevent it.

Stripped of its deductivist account of explanation and substituting causal notions in its place, a related possible account of functional explanations would be that A exists in order to B if:

1. A causes B

2. A persists because it causes B

3. A is causally prior to B, i.e. B causes A's persistence only when caused by A.

The first claim is straightforwardly causal. The second can be construed so as well. At t_1, A causes B. That fact then causes A to exist at t_2. In short, A's causing B causes A's continued existence.[5]

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[5]Wright's [1973] account is a partial inspiration here, but it has to be stripped of its conceptual analysis pretensions. And the requirement here is to explain persistence rather than existence.

The third requirement serves to distinguish functional explanations from explanations via mutual causality. If A and B interact in a mutually positive reinforcing feedback loop, then A causes B and continues to exist because it does so. Yet the same holds for B vis-à-vis A. Functional explanations do not generally have this symmetry. Thick animal coats exist in order to deal with cold temperatures, but when cold temperatures are present there is no guarantee that thick coats arise. And surely, even if they do, they do not cause the cold to persist.

On the view advocated here, functional explanations are a unique subset of general causal relations. We can therefore now answer two general complaints about functional explanations in the social sciences: that they make an illicit appeal to biological analogies and that they are tautologous or vacuous.

The most general description of a causal system describes a set of individuals whose values evolve through state space. At this level of description we are told very little: current entities stand in some relation to past ones. Natural selection is inevitably an instance of this system, given that it is a causal system. Functional explanations as causal are also an instance. Every causal system is analogous in being a dynamical system. The point here is that whether one set of causal relations is analogous or disanalogous to another depends on the level of description we are using.

So at the most abstract level it is a trivial truth that functional explanations are indeed analogous to Darwinian evolutionary systems in so far as they are causal systems. They are disanalogous in that social entities have no DNA that replicates. But then the HIV virus has no DNA either (it is an RNA virus). We find analogous processes in DNA and RNA organisms despite the differences because we abstract from the details to identify abstract causal patterns.

So do functional explanations commit us to some illegitimate analogy to natural selection? No, because natural selection explanations are just one realization of the above schema which is thus the more general pattern [Kincaid, 1986; Harms, 2004]. A's causing B may result in A's persistence by means that don't involve genetic inheritance, literal copying of identifiable replicators distinct from their vehicles or interactors, etc. In fact not all biological processes of natural selection require this level of analogy — differential survival can be caused by other processes (see [Godfrey-Smith, 2000]).

In this regard, Pettit [1996] notes that explanations of this general form do not even require a *past* history of selective processes. He argues that to establish a functional explanation we need only prove "virtual selection." Virtual selection refers to processes that would exist if some social practice with beneficial effects were to change. Suppose golfing may not be present now because of the positive benefits it had in the past, but if golfing were now challenged, then there would be pressures to maintain it. This virtual selection is just one way to make it true that A persists because it causes B, where B is a beneficial effect.

We can give a similar answer to the tautology or vacuity objection. Functional explanations in the social sciences certainly can be relatively uninformative as in "societies exist because they are stable" or "societies that cannot solve their prob-

lems are outcompeted by those that do." This is only slightly more informative than "the entities in dynamic systems persist so long as they are stable" or "organisms that are most able to survive, survive." However, it is only at this abstract level of description that content is lacking. When we pick a specific trait in a specific environment, "those that survive, survive" becomes "those finches with long beaks gain greater access to seeds in specific environments, causing greater production of similar offspring." The uninformative tautology has become an informative and highly contingent claim.[6]

The point is thus that functional explanations in the social sciences are not inherently empty anymore than they are inherently analogous or disanalogous to biological processes. It all depends on the details — on the extent to which they can provide convincing evidence that the three conditions for functional explanations hold for specific social practices.

Seeing this point will allow us to diffuse another standard worry about functional explanations, viz. namely that they provide no mechanisms [Elster, 1983]. "Mechanisms" is a buzz world that can cover quite different things. In particular, mechanisms might be vertical or horizontal. If the Fed influences GDP in raising interest rates because doing the latter increases savings, the savings are a horizontal mechanism, one between the two social entities. However, there is another sense of mechanism that refers to the lower level entities-individual agents — that realize social processes. These are vertical mechanisms.

Arguments for one kind of mechanism need not be arguments for the other. Thus Elster's claim that we must have individualist mechanisms to prevent spurious correlations makes no sense for vertical mechanisms, for they are not intervening variables. Describing everything individuals did in bringing about the social event of the Fed's raising interest rates will not control for other aggregate variables that confound our evidence that interest rates cause increased savings.

Furthermore, identifying horizontal mechanisms is not necessary to confirm causal claims. The standard clinical trial of a new drug, for instance, usually involves randomization to a control and treatment group with the aim of showing that the drug has an effect; the mechanism of action may not be known at all, but if the trial is conducted correctly we can make well confirmed causal claims. (And if the thought was that the problem is not one of confirmation but explanation, we explain the outcome without citing the intervening causal process as well.) In general we can check for all possible confounding causes (or mechanisms) by using Mill's methods without know the precise mechanism actually at work.

Let me turn next to three important, confusion-causing complexities in functional explanation that we should note: their relation to functionalism as grand social theory, their relation to nonfunctional causes, and the kinds of questions they may and may not answer. Keeping these in mind can help avoid needlessly unproductive criticisms and defenses of functionalism.

Functionalism can either be taken in the grand theory sense as a full theory of

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[6]Sober [1984] defends Darwinian natural selection against the common charge of being a tautology in this way and in more detail.

society or given more modest aims. Parsons, of course, wanted a total theory as did Marx — a complete functionalist explanation of all aspects of society. Functionalism as social theory claims to be a full explanation of social organization, social change, etc. Every aspect of society is related to its ability to promote societal needs. Two points about functionalist social theory of such great ambition are worth making. Specific functional explanations might be compelling without committing us to explaining everything in the social realm by its function or even committing us to the possibility of grand social theory in general. Given that many deny that functional explanations are ever successful, it will be a useful result to show how particular functional explanations can work well, even if the prospects for a grand functionalist theory is not defended. As a result, my focus here is on specific, low level functional accounts, though I will have some things to say about functionalism as grand social theory later when I discuss functional explanation as componential analysis.

Another point to note, to avoid some common confusions, concerns the relations between functional and nonfunctional explanation. Frequently the two are treated as mutually exclusive. They need not be for two related reasons. Functional explanations are a kind of causal explanation. Causal explanation involves picking out salient aspects of a complex causal field, i.e. picking out an element from the totality of factors causally involved. So we might have a functional explanation for why A persists and yet their might be further, nonfunctional causes involved in explaining A's persistence. In graphic terms, not all cases must be of type 1 rather than type 2:

Moreover, it is frequently helpful to think of explanations as answering questions [Garfinkel, 1981; van Fraassen, 1981]. Questions, however, have their content determined in interesting ways by context. In particular, context sets implicit contrast classes for an appropriate answer. If I ask "why Adam ate the apple?" then there is an implicit contrast class for Adam (as opposed to whom), ate (as opposed to what other action), and apple (as opposed to what other object). Thus an answer to one question with a specific contrast need not be an answer to all other versions of the same question — hence the basis for the Willy Sutton joke: "Question: Why do you keep robbing banks? Answer: That is where the money is." So it is possible that there are good functional answers to a question with one set of contrast classes but not for another. In brief, functional explanations need not answer all questions to answer some.

It is also helpful to distinguish various kinds of evidence that might be advanced in favor of functional explanations. We can place those kinds on a continuum running from direct demonstrations that the three conditions hold to indirect plausibility arguments that infer that they do. Direct evidence would show that a practice, institution, etc. had a certain causal effect and that having that effect caused it to persist. If we have data measuring the persistence of institutions, practices, etc. along with data about presence and absence of institutions and their alleged effects, then standard causal modeling exercises are possible.[7]

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[7]For discussion of causal modeling in the social sciences, see Woodward, in this volume.

(a) either

or
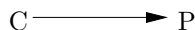
(b)
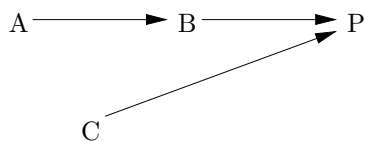
Figure 1. Competing vs. noncompeting explanations. In (a) the functional explanation where A causes B and that causes A's persistence is thought to be mutually exclusive to the situation where something else that causes A's persistence. But that is not necessary. (b) shows a case where A has both functional and nonfunctional causes.

As we will see below, such data is sometimes available.

However, such decisive data often are not on offer, and social scientists then must resort to more indirect and contentious kinds of evidence. Let me mention several and their corresponding difficulties: trait environment correlations, optimality arguments, and stability arguments.

Anthropologists studying small scale societies sometimes claim that different social practices are correlated with differences in ecological environment. They then infer from this that the practices provide benefits in these environments and persist because they do so. Biologists have long used such evidence in support of natural selection explanations of animal traits. But such evidence has plenty of room for confounding. Showing correlation is not necessarily showing causation, and moreover, is not showing that the trait persists because of its effect. Inferences thus have to be made with care.

Optimality arguments try to establish that some practice would be the best way to solve a specific problem, then find instances of that practice, and then infer that it exists in order to solve that problem. So economists argue that the best way to maximize profits is to equate marginal revenue with marginal cost, and then conclude that when firms do so behave, they do so in order to maximize profits — either by rationally seeing that this works best or by economic survival of the

fittest. Similarly, ecological anthropologists will argue that some foraging practice maximizes caloric intake and infer that it exists in order to do so.

Optimality arguments are only as strong as the inputs that go into them. We have to ask: have we measured all the relevant costs and benefits of the practice? Do we have good evidence about which practices are actually feasible? Do we have good reason to think that there is some process picking out the optimal? Optimality arguments are convincing when we can confidently answer affirmatively to all three questions, questionable to the extent that we cannot.

Stability arguments do not show that a practice is the best, only that once a trait comes to dominate, no other alternative practice is likely to replace it. There are forces in place that cause the practice to persist because of its effects. Put another way, we show that the practice is a local optimum.

Optimality and stability arguments are ubiquitous in some parts of the functionalist literature. There are two kinds of broad mechanisms that are sometimes invoked to underpin optimality and stability arguments: individual maximizing and selectionist processes. The former assumes in some fashion that individuals do the best they can, given their constraints and that in the process of doing so, create or sustain certain institutions, practices, etc. Those practices then can be said to persist because of their contribution to individual maximizing. Selectionist accounts describe the differential replication of social practices. Traits that produce differential replication are thus explained as persisting because of their effects. These mechanisms are spelled out in varying degrees and varying ways throughout the social sciences offering functional explanations.

With the account of functional explanation developed above and these various distinctions in hand I turn to the question where, how and with what success these sorts of explanations are employed in sociology and anthropology. There is no prospect here of answering these in blanket fashion as we have seen others try to do; we have to work case by case, asking if the three requirements are met. However, we can gain some generality in two ways: by considering functional explanations of broad social phenomena and by considering basic general mechanisms such as rational choice and selective processes. Throughout I focus on these two mechanisms as they are used to explain organizations, norms, and inequality.

4   RATIONAL CHOICE, NORMS, AND INSTITUTIONS

Let me turn first to functional explanations of norms, in particular explanations given by advocates of the "new institutional sociology," rational choice and game theory.   These approaches use directly the functionalist language of "in order to." So Nee and Ingram [1998] in a volume on the new institutional sociology argue that individuals jointly produce and uphold norms to capture the gains from cooperation. Aoki takes the same general perspective, but considers specific cases: he argues that property rights and community norms serve to regulate external diseconomies in a commons domain and that trading club norms in the medieval Middle East existed to promote efficiencies in the face of informational

deficiencies [2001]. These are all functional explanations precisely because they claim that specific norms make some benefit possible and persist because they do so.[8]

Let's look at a paradigmatic example in more detail. In *Analytic Narratives*, Greif seeks to explain "the process of state formation, the determinants of political and economic performance, and the politics of resource mobilization" in late medieval Genoa [1998]. During the 12th century Genoa was formed as a self-governing city republic headed by elected counsels. Genoa's ruling elite came from clans formed around feudal landholders. Those clans sought out profitable enterprises and sought to use the consulate to advance their own interests. Despite these potential conflicts, for much of the 12th century there was no military confrontation between the clans. However, economic growth was slow as the clans did not collaborate in acquiring external acquisitions. In the late 12th century the consulate system broke down. After some years of civil war between the clans, the podestra system (basically a city manager) replaced the consulate system and led to peace and economic expansion based on joint external acquisitions.

Greif explains the cooperation of the 12th century and the establishment of the commune in terms of a repetitive complete information game in which the strategies are cooperate or challenge. The commune was a mutual deterrence equilibrium realizing the strategy set (cooperate, cooperate). However, the extent of cooperation was restricted by the incentives for investment in military resources. Thus, Greif ties Genoa's slow economic growth and lack of joint international acquisitions to the commune. The greater cooperation in the podestra system was made possible by increased external military threats (viz. from Barbarossa), which increased the incentive for cooperation over military investment and thus also led to joint international acquisitions and thus economic expansion. The podestra system is shown to be an equilibrium outcome in a collusion game between the clans and the podestra.

In our functionalist terms, then, Greif gives us multiple functional explanations. The consulate system — the set of norms that made it possible — existed in order to capture gains from cooperation. The podestra system — again a complex set of norms defining the functioning of the office itself and the interaction of the parties — existed for the same reason in the presence of an external threat.

How do these explanations fair? Many issues are at stake. In the remainder of this section I focus on two questions: (1) Are these explanations inherently superior as economists often claim because they do not take norms as given but explain them based on constrained optimization? (2) Are the optimality arguments given for these explanations as rational choice explanations compelling?

Advocates of explanations like those listed above think they have a virtue that many sociological explanations do not, viz. they do not invoke unexplained norms. They explain norms themselves and do so in terms of individuals doing the best they can, given their situation and desires. In short, norms are explained, and

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[8]There are complex issues about exactly what constitutes norms and institutions that I am skirting here. For further discussion of norms, see Rouse, this volume.

they are explained in a way that does not make unwelcome assumptions about the malleability of individuals and the power of culture. These explanations provide a compelling mechanism behind the existence of institutions and norms. As a result, they avoid the common charge that functionalist explanations have no place for agency.

I argued earlier that appeals for mechanisms had to be evaluated in the context of what else we know. That seems to be the case here. Is every account of norms or institutions in terms of socialization, i.e. learning, automatically suspect? That seems a strong claim in the face of vast bodies of research in social psychology and elsewhere. It is likewise not obvious that the thesis of optimizing behavior has any automatic precedence. We have many well confirmed experimental results suggesting that individuals do not always optimize, because they follow norms of fairness, because they make mistakes in probabilistic reasoning, or because they have inconsistent preferences or objectives. So, the value of explaining norms in nonnormative terms is an open empirical issue.

Furthermore, it is doubtful that game theory models eliminate norms in any case. Games require that there be rules of the game defining payoffs, possible moves and their timing, the preferences of actors, and what about all of the above is common knowledge among the players. These are typically taken as given, not explained. Yet these elements are kinds of norms or institutional facts. Moreover, as we will see below, many games have multiple possible equilibria. Which equilibrium is hit upon is usually explained in terms of "salience" or "focal points," again assuming some preexisting norms or conventions.

The examples described above certainly do not explain in an institutional and norm free way. Greif takes the structure of feudal society as given. His agents are clans defined by their social position as landholders. This is a rich social structure with an accompanying set of norms, roles, etc. that spell out the institutions. The possible strategies of the clan are also taken as given, resulting from the previous history of Genoa. These descriptions are the framework on which his game theory account works. Greif explicitly acknowledges all of this [1998, 47].

I would again argue that this dependence on social structure has important lessons for the general philosophy of science debates mentioned in the introduction. Game theory is a useful technology or tool. But by itself it does not unify the social sciences or support individualism. It does not have that much content; content has to be filled in to apply it. Greif's actors are clans not individuals. The game theory analysis works by building on sociological understanding, not replacing it. The connections between economic, political and psychological accounts of the social world are not made simply by the use of a common technology, but by the substantive attempts to interconnect them in concrete explanations.

So Greif's explanations cannot be judged superior on methodological grounds alone; they have to be supported more directly. The explanations given here are rational choice explanations. They are supported by arguing that the norms in question are strategies in a game that would be adopted when a Nash equilibrium is reached. A Nash equilibrium is the situation where each person's choice constitutes

the best reply to the choices of the others. Rational individuals will see this and act accordingly. In short, we have an optimality argument for a rational choice explanation of the institutions or norms in question. This is standard fare for rational choice game theory accounts of social processes.[9]

Let me discuss the general problems that face these kinds of functional explanations and then ask how well Greif handles them. As in all optimality arguments, we want to know that these explanations are based on an accurate specification of the game that is being played — the costs and benefits, strategies, preferences, etc. of the actors — and on good evidence that there is some mechanism bringing about the optimal. I think there can frequently be doubts about both.

Determining what game is actually being played, if any, can be difficult because many of the defining components of the game may be unobservable. The classic question here is inferring the utility functions of the agents, when only behavior is observed. This is complicated by the fact that real agents occupy many roles at once and thus may be playing multiple games, including metagames — games about what games to play (see [Ross, 2005]). Needless to say, this can make presenting a convincing case quite difficult.[10]

Equally serious difficulties arise for the claim that there is a mechanism picking out the optimal. Traditional game theory accounts of institutions are rational choice explanations. The institution is a Nash equilibrium where each player chooses his or her strategy as best response to all other strategies. The agent is assumed to know the possible strategies, payoffs, etc. and then to deduce rationally the best strategy.

One problem is that many games that seem relevant to explaining institutions do not have a single Nash equilibrium. In fact, for many situations, nearly any strategy can be an equilibrium. So rational deduction from knowledge about payoffs cannot be the whole explanation of the given institution — how did the agents hit on the same equilibrium? Obviously something essential is missing.

Suppose the claim is that, for example, a specific norm exists in order to capture gains from cooperation. Then we are claiming that the norm's function is the complete cause of its persistence. The mechanism supposedly realizing that process is alleged to be rational choices of individuals. However, what rational individuals should choose in the situation of multiple equilibria is underdetermined, so we have good reason to think our claim is false — that the norm's benefits are not the full cause of its persistence.

One standard answer to this problem from game theorists is that some equilibria are more salient — they are focal points. However, then the norm is not explained by its contribution to gains from cooperation alone. It may still be partially so explained, but the rest of the explanation comes from whatever explains the salience point. That is likely to be preexisting norms and institutions. This illustrates my earlier point that something can be both functionally and nonfunc-

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[9]For further discussion of rational choice models, see Pizzorno, this volume.

[10]Economists working in industrial organization — market structure and concentration issues — confront this problem often as they explicitly acknowledge [Sutton, 2001].

tionally explained without the explanations necessarily competing, depending on what we are trying to explain.

This can be put in terms of contrast class and background information as we suggested earlier. We need to distinguish between two questions: Why is there a norm (rather than none at all)? versus why this norm rather than that? The first may get a functional explanation when the second does not. We may have good evidence that there is a norm rather than none because individuals rationally saw that having some norm would allow gains form cooperation. But we may also have evidence that the reason this particular norm was picked rather than others was not rational deduction from known payoffs, etc., but has to do with something else in the causal field, viz. the common culture that makes some strategies more salient than others.

Another route to avoiding the multiple equilibria problem is to deny that it exists. One way to do that is take the heroic route of Aumann [1986] and claim that rational agents facing the same information, etc. will have to reach the same conclusions. That strategy seems committed to a full logic of inductive inference, something we have good inductive grounds to think unlikely. An alternative route to eliminate multiple equilibria is to require more than just Nash equilibria. This is the project of the so-called "refinements" literature in game theory. The emerging consensus is that this project is not productive or, in other terms, too productive, for there are numerous extant proposed refinements.

Aside from the multiple equilibria problem, game theory rational choice mechanisms also make quite unrealistic rationality assumptions, even when multiple equilibria are not present. Individuals are required to calculate equilibria from information about the game that many smart students cannot do with training. Sometimes there is no known algorithm for calculating equilibria and they can only be found by a trial and error process. Frequently they require players to be rational Bayesian agents, something the empirical literature tells us we seldom are. For a great many rational choice game theory accounts in the social science, these problems raise serious doubts that the phenomenon in question has been shown to exist in order to maximize player's joint utility functions.

How does Greif's work stand up in the face of such problems? Fairly well. By taking as given quite a bit of the existing social organization and by making extensive use of historical context and evidence, he can make a fairly convincing case about the games that were being played. And while he invokes the subgame perfect Nash equilibrium concept (a refinement of Nash), a charitable interpretation of his account would not take the Genoese as actually calculating their best replies in that equilibrium. Implicit here is a learning mechanism where the relative payoffs — the nature of the game being played — are adjusted to reach the equilibrium that allows the podestra system to survive. For example, to make collusion with one of the clans by the podestra unlikely, various incentives and information pathways have to be controlled, which the Genoese found ways to do. However, this mechanism is not modeled or even verbally described in any detail, and it is a mechanism that falls outside rational choice game theory — in the

evolutionary or selective mode of analysis. It is to those sorts of instantiations of functional explanations that we turn to next.

5 INEQUALITY

The work discussed in this section looks at functional explanations of inequality. This research falls somewhere in the middle of the rational choice — pure selection continuum. Individuals doing their best are either explicitly invoked or consistent with the accounts given. Yet formal rational choice explanations, game theoretic or otherwise, are not given nor is there explicit appeal to differential survival processes. Nonetheless, the claim is made that inequality exists because of its effects and various kinds of evidence are given. These characteristics are representative of much functionalist work in the social sciences.

A long tradition of sociological research seeks to explain inequality in income, wealth, status, and authority in terms of the traits of individuals. A well-known and influential paradigm of this work is Blau and Duncan's study of the American occupational structure. For Blau and Duncan, "a pervasive concern with efficiency" leads to outcomes where "achieved status becomes more important than ascribed status." [1967, 430]. Individuals are sorted by their better ability to do the job. Using path analysis and a large data set, Blau and Duncan support these claims by examining the relative influence of one's own education, first job, father's education and father's occupation on occupational mobility.

Blau and Duncan's findings were expanded upon by further studies, with other individual variables and new data sets added. *The Bell Curve* [Herrnstein and Murray, 1994] is one of the later installments. The functional explanation common to all this literature is the claim that inequality exists in order to promote efficiency. Mechanisms are not made explicit: there is no appeal to rational choice maximizing models to provide the mechanism nor is there any explicit appeal to differential survival processes. So what is the evidence? It is only various correlation between traits potentially connected to efficiency. It is hard not to conclude that the evidence is weak.

A second question about Blau and Duncan's account of inequality concerns its completeness. We noted earlier that functional causes do not necessarily preclude other, nonfunctional causes from acting at the same time. These explanations of inequality are a case in point. While they put forth their account as if efficiency considerations explained all, there are good reasons to think the situation is considerably more complex. If inequality is functionally explained, it is so only partially. At most, inequality exists in order to promote efficiency, given preexisting social stratification.

There are two ways to see this. The first comes from the following useful analogy. Suppose we have a pen full of dogs of different sizes with differing abilities to capture bones thrown into the pit. Suppose also that dogs prefer larger bones to smaller ones and that they fight it out according to that preference when a bag of bones is thrown into the pen each day. When the fighting settles down, we

will have canine inequality. Is that inequality explained by differences in ability to perform the job of bone snatching? It cannot be entirely. The distribution of bone sizes in the bag is a key component — bags with large variances will lead to large inequality, those with smaller variances, smaller inequality. For Blau and Duncan's case, the parallel is the preexisting set of occupations. Ability may determine which rung of the ladder you stand on, but it does not determine how far apart the rungs are or how many there are.

A second reason to think that these functional explanations are only part of the story — only explain given an already existing social structure — comes when we try to provide a relatively realistic picture of the process whereby individuals are assigned to jobs. When we say that the best individuals are assigned to the job, what is the reference group — best of which lot? In the models of perfect information and competition of neoclassical economics, the answer would be: best of all individuals. But any sociologically realistic account would realize that individuals are not matched with jobs that way. Much research [Granovetter, 1995] shows that individuals find jobs through social networks. Those networks are determined at least in part by preexisting social divisions of race, ethnicity, and the like. A realistic account of the mechanism behind the functional explanation appeals to other causal factors in addition to efficiency. This is another instance of our general moral that functional explanation presupposes rather than replaces explanations in terms of social structure.

Explanations of inequality in the Blau-Duncan tradition are individualist in spirit — they aim to explain everything in terms of the traits of individuals. An alternative functionalist approach to inequality that avoids individualism is that of Tilly [1998]. Tilly argues that inequality cannot be explained in terms of traits of individuals, for there are "categorical" inequalities existing independently of individual differences in ability. Categorical inequality is the social grouping of individuals into types, backed up by systematic processes reinforcing those divisions. These divisions may be based on conscious discrimination, but need not be.

Tilly identifies several basic mechanisms behind categorical inequality: exploitation, opportunity hoarding, and emulation.[11] All involve using existing social categories such as race as a means to further individual interests. Exploitation uses social categories to deprive individuals of their full contribution to a joint activity. Opportunity hoarding comes about when a resource is confined to individuals of one group. Emulation happens when individuals form new organizations based on existing social categories.

Tilly has given a functional explanation: "categorical inequality persists [because it] facilitates exploitation and opportunity hoarding [and] solves organization problems." Unlike Blau and Duncan, Tilly's functional explanation does not take social structure as given but seeks to explain it. Again, the mechanisms are not

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[11]Tilly actually adds a fourth — what he calls "adaptation." Adaptation is solving problems using existing social distinctions. However, that seems to me simply to be a more general description of what is going on in the case of exploitation, opportunity hording, and emulation.

explicit rational choice models nor are they explicitly put in terms of differential survival.

There are two main questions to ask about Tilly's functional explanations: is categorical inequality always the best way to solve problems? And does categorical inequality exist solely because of its functions? We can have doubts of the first kind because discrimination has costs — slaves rebel — and yet Tilly's evidence almost entirely focuses on the benefits to individuals. Unfortunately, we also have good reason to think that categorical inequality persists for reasons other than its value in exploitation, etc. We have lots of evidence, much of it experimental and robust, that: humans naturally tend to ascribe environmental causes to individuals (the "fundamental attribution error" — [Ross, 1977]); tend to blame the victim [Lerner and Miller, 1978]; reason via stereotypes, including social stereotypes [Bar-Tal *et. al.*, 1989]; display in group bias [Tajfel and Turner, 1979]; and blame themselves when they are discriminated against [Milner, 1983]. In short, there seem to be natural response patterns in humans that make categorical inequality likely. Tilly's functional explanation is unlikely to be the whole story.

6   SELECTIONIST MECHANISMS

The functional explanations of inequality discussed in the last section are not full rational choice explanations, in that they do not explicitly state and solve a maximizing under constraints model. And though they invoked competition, they were also not explicit selectionist models of differential survival. In this section we turn to explicit selectionist underpinnings for functional explanations. There has been an explosion of research on such social processes in the last decade, research that is arguably some of the most promising social science around.[12]

What is a selectionist mechanism for a functional explanation? Obviously, it must be a mechanism that realizes the three basic conditions. As suggested in Section 3, selectionist mechanisms in the abstract have very broad scope [Kincaid, 1996; Harms, 2004]. The root idea is competition — differential growth (positive or negative) of entities of different kinds. When we identify the traits responsible for differential growth, then we know that those traits persist because of their effects. At this level of abstraction, nothing commits us to the full details of a standard biological natural selection process involving genes, phenotypes and reproduction.

Because selection mechanisms can be described very generally, there are numerous different ways they can be instantiated in concrete causal processes and likewise diverse ways of formulating and evaluating claims about those processes. It will be useful upfront to distinguish the various parameters that can be combined to produce specific selectionist mechanisms and evidence for them. In particular, selective processes may differ on: the extent to which human reproductive fitness

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[12]For further discussion of selection and evolutionary explanations, see Whimsatt and Haines, this volume.

plays a role, the kinds of things that compete, the kinds of evidence supplied, and the extent to which process are modeled formally.

We need to distinguish several different claims about selective social processes and their relation to genetic inheritance. There are multiple parameters that we might want to consider, among them being the extent to which it is cultural selection that explains rather than biological, the extent to which it is current reproductive value that explains as opposed to past, and the extent to which the explanation is a complete one or not. Species without learning and under strong selective pressure that have traits that are fully explained by relatively recent biological reproductive value represent one pole. Species with extensive learning capability acquired through a distant history of biological selection and minimal existing selective pressure but intensive current cultural selection are on the opposite end of the spectrum. Various combinations in between are possible. Exactly where humans fall on this spectrum is controversial.

We also need to ask what kinds of things we are trying to understand. Selectionist mechanisms are used to explain at least three different targets: the traits of individuals, the traits of groups, and the traits of social systems, i.e. of individuals or social groups that stand in some defining relationship to one another. The traits of individuals might be a propensity to commit a crime or to hold certain beliefs. The traits of groups might be a marketing strategy of an organization. The traits of a social system might be its type of production relations.

The distinction between selection of individuals and selection of groups hides some ambiguities that we should address here. An emerging consensus on biological and social evolution [Sober and Wilson, 1998] sees natural selection as a multi-level process that can act at various levels in the biological and social hierarchy, claims that the selective effects at the various levels can be identified and combined into a unified explanation, and is optimistic that cultural selection can integrate social phenomena with the biological. A corollary of this consensus is that evolutionary game theory may provide a universal language for the social and biological sciences [Gintis, 2000].

This consensus is too quick. So-called "multilevel" selection models are multilevel in only one, relatively weak sense; the consensus account of group selection includes groups only in an anemic sense. Common to the current consensus is the following picture: group selection of a trait occurs when the trait is differently distributed in different groups in a population and those groups with a higher frequency of the trait are thereby more fit in that group size increases relative to other groups. In this situation the frequency of a trait can increase in the population as a whole, even though it may be less fit in each group. If the effects on group productivity are strong, the trait can evolve.

Note two things about this notion of group selection. The fitness of the group is defined by ability to increase in size — to increase the number of *individuals* in the group. Thus the unit of measurement is individual organisms specified by trait. It is this choice of unit that makes an intergrated multilevel account possible:

the effects of genic, individual, and group selection are compared in terms of differential survival of individual organisms of specified types.

Though dominant, this is not the only extant notion of group selection. To see that there is another conception, consider the basic requirements of the Darwinian mechanism, viz. individuals that reproduce individuals similar to themselves, variability in the traits of individuals, and differential survival of some kinds of individuals into the next generation. It is perfectly possible that these apply to groups of individual organisms per se. So group selection can occur when there are different kinds of groups that produce new groups that resemble them, when groups vary in their traits, and those traits have varying influences on the next generation. This is group selection where the units of measurement are groups, not individual organisms. If a trait leads to more groups of one kind, there can be group selection regardless of what happens to the number of individual organisms in them. Arguably this notion of group selection is what various biologists and social scientists have had in mind. It was explicitly contrasted with the current consensus notion in the mid 1980s [Kincaid, 1986; Damuth and Heissler, 1987].

The complications introduced by group selection in the second sense have not received sufficient attention. Group selection in the multilevel sense studies a different dependent variable than that based on survival of groups. Thus, the claims of multilevel selection to integrate both group and individual processes and the biological and social are suspect. There are also complex issues surrounding the very idea of selection "acting at a level" that I cannot address here. But at the very least it is important to keep the two different senses of group selection — differential survival of individuals because of group membership and differential survival of types of groups — distinct.

Two more important parameters concern how explanations are formulated and the kinds of evidence given for them. In formulation, the difference is mathematics: are the causal selective processes given some kind of explicit mathematical treatment? In the case of evidence, the difference is between direct types of evidence — measuring the causal influence of A on B and then the influence of that causal relation on A's persistence — and more indirect evidence that infers the presence of a causal mechanism from a model with various idealized assumptions that somehow fit the facts.

I want to look at four examples of research that span the range of the differences listed above and to say something about their relative success: Hannan and Freeman's work in organization ecology, Bowles' evolutionary game theory account of property rights, Richerson and Boyd's cultural selection theory, and Smith's work in evolutionary or behavioral human ecology.

I look first at organization ecology which provides a mathematical formulation of the selective process, takes the traits of groups to be the relevant target, provides direct evidence, and provides an account where biological selection plays no role. While I use Hannan and Freeman's work as an exemplar, theirs is but a part of a much larger literature that shows, for sociology, an usual degree of continuous research building on past results [Pfeffer, 1993].

Hannan and Freeman develop a model of the differential birth and death of organizations, the kinds of environments they face, and the strategies that are likely to do best in those environments. If organizations change slowly and if they compete for resources, then there should be differences in the founding and death of organizations as the number of organizations increases, assuming a fixed resource base. In other words, selection of organizations should be observable. If environments differ in significant ways, then we might expect that different kinds of organizations — organizations with different traits or strategies — would predominate in different types of environments. Those traits would then contribute to organizational survival and persist in the population because they do so. They would exist in order to take advantage of the specific environment in question. They would be functionally explained.

Borrowing from the ecologist Levins [1968], they distinguish three types of environments: stable, coarse grained variable, and fine grained variable environments. Intuitively, an environment is stable if the resource is always present, coarse grained variable if the resource comes in "lumps," and fine grained if it comes and goes at short, repeated intervals. Hannan and Freeman then take the further concepts of "specialist" and "generalist" from ecology, where specialists rely on a single resource and generalists on a range.

With these distinctions in hand, there are some natural predictions about which strategies should be present in which environments, assuming that selection is operative. Generalists will do best in variable course grained environments and specialists best in stable environments. If environments are sufficiently fine grained, then specialists may dominate there as well. These distinctions thus support some fairly precise functionalist predictions at the theoretical level.

Hannan and Freeman are able to tie these theoretical predictions to some impressive data: large original data bases on environment types and organizational strategies for restaurants and semiconductor firms. They control for a number of possible confounding factors — age effects, size effects, etc — and find the correlations between environment types and strategies holds up well.

Such work in organization ecology is a vivid counterexample to the various skeptical claims about functional explanation in the social sciences. Selectionist mechanisms can be precisely formulated that are instances of the general pattern described in Section 3 and that do not require biological equivalents of genes. Organizations are sufficiently stable and defined to undergo birth and death. The tautology problem — organizations that survive, survive — is solved in precisely the same way it is in evolutionary biology: by turning the abstract selection claim into concrete, defeasible claims about the causal interaction between types of traits and the environment. Nothing in these explanations entails that society as a whole is a throughgoing, stable entity or that change does not happen.

It is also useful to note what this work says about controversies over individualism, the role of the biological, and levels of selection. These are macrolevel explanations that abstract from the causal processes at the individual level. The selective process is group selection in a straightforward sense that makes no com-

mitment to individual success. There is clear recognition that there is more to society than the culture of the evolutionary psychologists — which is only ideas in individual brains — and no prospect of explaining these phenomena solely in terms of psychological modules developed in the Pleistocene. The models developed can be represented in evolutionary game theory, but that in no way gives us a general theory relating individual behavior to the phenomena at hand.

A second, large body of work emphasizing selective mechanisms is found in evolutionary game theory. Bowles [2004], for example, provides an interesting evolutionary game theory account of the emergence of property rights among hunter-gather early humans. The main elements of the model are:

1. Individuals of four types:

   Sharers, where two sharers split a good evenly when they interact

   Grabbers, where grabbers get the good when they meet sharers and fight when they meet each other, gaining the good or cost of defeat with equal probability

   Punishers, who divide equally with each other and sharers, and who try to punish the grabber and either gain the good or bear the cost of defeat

2. Cultural transmission process where those strategies that do best are most likely to be adopted by the next generation

3. Group selection, measured in terms of differential survival of individuals as influenced by the average payoff per group

4. Second order punishment: individuals who do not punish are punished

Bowles shows that in this model there are two stable equilibria that cannot be invaded: the Hobbesian, where the grabbers outnumber the sharers and there is frequent fighting and no punishers, and the Rousseauian, with a mixture of sharers and punishers and no grabbers. However, when the possibility of property rights are added to the mix, then a mixed strategy is possible: act like a grabber when you possess something, otherwise act like a sharer. This is the Bourgeois strategy from the chicken and egg game. Bowles runs simulations that show that Bourgeois invades Rousseauian equilibriums to the extent that there is not uncertainty about possession. The transition from hunter gathering to farming made possession much easier to determine than under the presumably Rousseauian equilibrium during the hunter gather period. The upshot is that property rights persist once settled agriculture comes on the scene because it maximizes the success of groups with property rights.

Two obvious problems confront Bowles' account: First, our evidence about the details of human environments, social structure, biology, etc. in such distant time periods is sufficiently thin that any account is likely to be underdetermined by the evidence. Secondly, Bowles' models depend on numerous unrealistic assumptions, some of which can in no way be seen as rough approximations or idealizations.

I have nothing original to say about the first problem which has been discussed at length elsewhere and moreover, it is not applicable to contemporary uses of evolutionary game theory. The second problem is, and it is thus worth saying a few words about the problem.

There is a considerable temptation among scientists and philosophers to advance simple universal criteria that tell us when a simplified model succeeds. If it is "insightful," "provides understanding," and "similar" to the system it is modeling, then it is a good model. These defenses are inadequate. Insight and understanding are subjective states of investigators; warm and fuzzy feelings are not well known signs of reliability. "Similarity" is just too easy, since everything is similar to everything else in some respect. But what is the right respect? A well defended model with idealizing or false assumptions requires showing that we have good reasons to believe the causal processes identified in the model are actually at work in the world in the way the models says they are. Bowles provides no such argument, and much of evolutionary game theory applied to social phenomena is in the same boat.

One route around this requirement is to weaken the conclusions of the model. It is commonly said that idealized models at least show possibilities. But there are possibilities and possibilities. To describe a logically possible world is not to describe a socially possible one. One needs evidence that the simplifications and falsifications do not produce a socially impossible world. Possibility in a model and real world possibility are not the same; getting from one to the other requires argumentation.

I turn next to the selectionist models of Boyd and Richerson [1985; 2005a; 2005b]. They have developed one of the more sophisticated accounts of selectionist mechanisms with close attention to the interaction of cultural and genetic selection. For Richerson and Boyd, the target of selection is culture, and culture is information stored in the head of individual humans. When information in the brain of one individual produces behaviors that increase the odds that related information will appear in the head of another individual, we have a selective processes.

Boyd and Richerson distinguish three "forces" affecting cultural evolution: guided variation, biased transmission, and natural selection. *Guided variation* is learning. Using some specific standard that might be either innate or learned itself, judgments are made about the ability of various cultural variants — ideas, beliefs, etc. — to promote that standard. Guided variation does not depend on the extent of existing cultural variation, for new variants are generated in the learning process. *Biased transmission*, like guided variation, is differential adoption of culture based on fit with some standard, but biased transmission works only on existing cultural variants. Technological diffusion is a widely studied example. *Natural selection* occurs when one cultural variant spreads more than another without any evaluative standard determining whether it is adopted. Cultural practices that lead to greater fertility would be a case point.

In terms of the model developed in Section 3, all three mechanisms are forms of differential survival and thus possible grounding for functional explanations. However, Boyd and Richerson are particularly concerned with identifying the circumstances under which cultural selection would evolve by natural selection, and distinguishing these different forms is essential for that task.

The paradigm case for Boyd and Richerson is selection for individual traits — the information in the head. However, Boyd and Richerson do at various places allow for the possibility of cultural group selection [2005]. As is typical, their discussions are ambivalent on the exact processes involved. At one point they discuss tribes with competing norms and use differential survival of the *tribe* as their metric. At other points [2005], they envision group membership causing differences in survival of types of *individuals*.

Boyd and Richerson rely primarily on previous studies by others to support their claims, and much of their efforts are analytical — separating possible causes — rather than explanatory. They cite extensive literature arguing that culture matters. They cite an equally extensive literature describing the processes of transmission of culture from individuals to individuals.

Let's look at two possible criticisms of Boyd and Richerson' work: (1) culture is not particulate in the way the selection models requires [Kuper, 2001] and (2) they illegitimately treat societies as populations of *individuals* [Fracchia and Lewontin, 1999]. Culture is a complex integrated set of meanings; it does not come in natural units. Societies are not just a population or collection of individuals; they are organized and structured.

Both criticisms have force. For Boyd and Richerson, information is always "in the head." Yet there is plentiful work from recent cognitive science that suggests this picture is much too simple. Cognition is embedded in larger physical and social environments in essential ways [Clark, 1998]. That social environment does indeed have organization that makes it very different from the standard population of population genetics, their paradigm. Hannan and Freeman's work on the differential survival of organizations shows that if anything does. The moral is thus one we have seen before: selectionist models are not universal theories in and of themselves and they often depend upon social science to provide them with the framework within which selection works.

Both criticisms also overreach. Some aspects of culture are relatively particulate. Boyd and Richerson refer often to work on the diffusion of innovations. New inventions certainly look like individual units that can be counted. If the claim is only that they are bundled with other information, Boyd and Richerson can (and do) concede that sometimes that it is complexes that are transmitted and selected. They do not emphasize such situations and there may be an argument that they are much more important than Boyd and Richerson grant. But that would not seem to show they always are so or that when they are bundled, no selectionist model is possible. Runciman [1989], for example, explicitly invokes a selectionist style model in the style of Boyd and Richerson for cultural practices, which are precisely interlocking sets of roles, beliefs, etc.

If the social world is not simply a population, it does not follow that nothing about it can be described in population terms. Hannan and Freeman's work does exactly that, albeit with a population of organizations.

I turn finally to Smith's [1981] instantiation of human behavioral ecology. Human behavioral ecology is a trend used largely in the study of small-scale human societies. Like Hannan and Freeman, this approach borrows models from evolutionary ecology. The standard usage is of optimality models, frequently optimal foraging models. Smith studied Inuit hunting practices, in particular group hunting size. Assuming that groups maximize caloric intake, Smith develops a model of the optimal group hunting size, derives predictions from it, and tries to show that it matches reality. The result is supposed to support a functional explanation: Inuit hunting group size exists in order to promote caloric intake.

What is the mechanism for this explanation? It is not some kind of cultural selection for hunting practices. Human behavioral ecology in general claims that social practices exist in order to promote current reproductive fitness. Caloric intake, Smith argues, is a good proxy for reproductive fitness, hence it is measured.

Smith represents the opposite pole of the continuum from Hannan and Freeman. For them, selection acted on social entities and biological selection was nowhere in sight. For behavioral ecology, selection acts on genes and social selection seems nowhere in sight. While it is certainly imaginable that some social practices persist in part because they promote current reproductive fitness — religious beliefs concerning fertility are a possibility — it seems highly implausible that all or even the majority do. This seems right even if you think that any cultural selection account has to be explicitly integrated with biological processes. Boyd and Richerson, for example, describe gene-culture coevolutionary models of some plausibility that show that maladaptive cultural practices can evolve by natural selection. There would seem to be rather large problems for the entire human behavioral ecology program.

So the upshot is that there are a variety of possible selectionist processes that could undergird functional explanations. They provide a range of different substantive takes on the relation of the biological to the social and the nature of social explanation. They vary in their ability to provide convincing evidence. Yet it is hard not to think that some versions of the selectionist still represent one of the more promising trends in modern social science.

7   FUNCTIONAL EXPLANATIONS AS SYSTEMS ANALYSIS

My focus throughout this chapter has been on explanations that claim social practices exist in order to fulfill their functions. However, much of functionalism past (Radcliffe-Brown, Parsons) and present (Luhman) has been about identifying the components of social organization and their interactions. In other words, functionalism has been about the analysis of social systems.

Put in these terms, functionalism is, as Davis [1959] argued long ago, not essentially distinct from social science in general. Even at this most general level of

functional theorizing, there are many interesting and surprisingly unexplored issues concerning the extent to which there is a unique and meaningful way to divide the social world into societies [Tilly, 1984]. Functional accounts, however, generally add further constraints to the very general idea of analyzing social components and these raise further questions. Frequently, it is claimed that the components have to fit together in special ways — ways that realize or are compatible with emergent properties of the social system. There are different ways to pursue these claims and difficulties in each case of identifying clear theses. And always in the background are the prospects for functional explanations in the stronger sense — of identify what things exist in order to do rather just what they happen to do.

Obviously the issues here are broad and numerous, too much so to say anything compelling in a short space. However, this aspect of functional explanation is undeniable and thus cannot be ignored. I close with a sketch of some of the issues.

We can distinguish at least the five following questions about functionalism as systems analysis. There are no doubt interesting connections between these issues, but they are relatively distinct.

1. What are the systems being analyzed?

2. Are there universal components of any social system that suffice to explain its basic characteristics?

3. What are the components of social systems and how are they identified?

4. Is there one right way to divide a social system into components, and if not, what does that say about the reality of those components?

5. How does identifying components explain?

In its purest form, traditional functional analysis — with Durkheim, Radcliffe-Brown, and Parsons being prime examples — gives the following "organicist" answers.

1. System being analyzed: The system being analyzed is "society," where that is identified with a clearly bounded unit and where the boundaries are spatial, cultural, and political. Small scale societies like the Neurer or modern nation states are paradigms.

2. Components: There are basic components that every society has and it is these that are fundamental to any explanation and comparison. Kinship systems or role and statuses are paradigm examples.

3. (a) What are the components? Here there is of course not complete agreement, but there is a basic set that is common to much functional explanation and much standard social science (note the relevance of Davis' claim in this regard): kinship systems, social roles, norms, which identify, reinforce, etc. social roles, values, which might be equated with

individual beliefs, public symbols, etc. and social structure, which describes at a minimum the relationships between social roles.

(b) How are components identified? The most explicit answer comes from Parsons (not a good sign!) who apparently thinks that the basic components of society are the necessary preconditions for human action (in some writings) or the necessary conditions for social stability or survival (in others). Seeing this Kantian a priori trend in Parsons makes greater sense of where he is generally coming from (see [Munch, 1988]). Other functionalists in this camp focus on societal needs more directly and see that relation as more contingent and empirically determined. Components are identified by their characteristic effect on social system functioning, usually described as stability or survival.

4. Unique way to divide: Since the components identified in (3) are universal, there are not other competing, equally legitimate analyses into components nor are there competing and equally valid ways of dividing societies into the universal components themselves.

5. How does componential analysis explain? Functionalists in this tradition vacillate in this regard. Often they claim that when components can be linked in a lawful relationship — relating for example social structure and kinship — then explanation has been achieved. At other points the explanation comes from showing the contribution of components to social stability, survival, or needs.

Associated with the organicist strand are several deep assumptions in the philosophy of science: that theories are essential and central to science, that theories describe lawful relations between elements, and that explanation comes from picking out those relations. These are a central part of the broad philosophy of science issues raised by functionalism that we identified in the introduction.

There are reasons, on my view, to doubt the organicist picture at nearly every step. Tilly [1984] has argued persuasively that focusing on society as a whole gets the scale wrong. From the anthropologists we get a related concern: that societies are not in fact cohesive cultural units in the sense that there is one culture shared by all. Rather, culture takes on diverse forms in different contexts and social locations, with the actual practice being a fluid, ongoing matter of negotiation.[13] It might seem obvious that every society has roles, norms, values, kinship systems, etc. But behind this truism are lurking some hard questions: What exactly do roles, norms, etc. come to? Social scientists use them in different and perhaps incompatible ways. Norms, for example, are sometimes taken as regularities in behavior, sometimes as requiring specific beliefs on part of the actor, and other times as requiring sanctions. So a first important task is simply getting a clear conception of these components.

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[13]For development of these criticisms, see Risjord and Rouse, this volume.

A second related concern arises from trying to spell out the relations among these allegedly universal social components. Parsons, for example, distinguishes between the cultural, personality, and social subsystems present in every society. Yet elements of each seemed to be intertwined with all the others [Parsons, 1951], so it is unclear what the relation between them is. Are they real independent entities that can stand in causal relationships? The problem here parallels a long and relatively unproductive literature trying to specify what "culture" is and how it might separate anthropology from sociology.

A third set of problems concerns the uniqueness and reality of social components as well as their ability to fully explain. Are there multiple ways to divide up societies into "subsystems" as Parsons calls them? When are such divisions competing explanations and when are they complimentary? Should we take these entities as "real" or merely as useful instruments? These issues get surprisingly little attention in the social sciences or, for that matter, in the philosophy of social science literature. A natural route to approach them would be with functionalism of the third sort I distinguished early on — by asking if they represent real causal patterns that we can identify independently of their particular realization in individual behavior.

A further and fundamental issue concerns how the organicist tradition intends to explain by means of the components it picks out. We saw representatives like Radcliffe-Brown taking explanation to come from the citing of lawful relations between components. However, that presupposes a well refuted picture of explanation, viz. namely, the nomological deductive model. Organicists can move away from that conception of explanation by focusing on the causal contribution that components make to system survival or stability.[14]

This suggests that componential analysis is perhaps likely to fare best with a selectionist framework already in place. The organicist strand in functional explanation is preDarwinian in that it treats societies as types with an underlying essence. Moving to a selectionist picture means dropping that assumption. It is then an empirical question what entities and at what level of aggregation and abstraction are sufficiently stable to compete over scarce resources. Answering that question would then be part and parcel of identifying social systems and their components or aspects. There may not be universal components with sufficient content to do much work in explaining social organization.

8   CONCLUDING MORALS AND OPEN QUESTIONS

The overriding moral of this chapter is that functional explanations in the social sciences work — not always and not easily, but sometimes. They can make illicit biological analogies or reduce culture to biology, but they need not. They can be empty or tautologous but that is not inevitable either.

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[14]An alternative approach here would be to use cybernetic or homeostatic causal relations, something found in the functionalist tradition but not discussed here.

Not surprisingly, assessing functional explanations raises numerous questions in the philosophy of science. Some have taken functionalism in its selectionist version as a universal language of social science that would unify the social sciences and the social and biological sciences. Some have thought that unification would vindicate some form of individualism. We have found reason to be skeptical about these claims.

The most promising version of functionalism is apparently some form of a selectionist process. Yet one of its most powerful instantiations — evolutionary game theory — runs directly into the problems raised by highly simplified models — the problems of when do they explain and how do we know? Keeping clear on which results are interesting intellectual exercises only and which are giving us well confirmed explanation of real world phenomena is an important and hard task.

A final pressing complex of questions concerns the scope functional explanations — how much of the social world can they account for? The selectionist variant faces this question in a form that actually arises for natural selection in biology — how much of development, life history, and large scale ecological organization can be explained in terms of natural selection? And to what extent are explanations of the former autonomous and necessary presuppositions of natural selection explanations themselves? We have seen that the same kinds of questions confront functionalism in the social sciences when it faces the issues of integrating selective processes using different metrics and in determining what games people are playing and why.[15] There is clearly much interesting work to be done — conceptual and empirical — around the functionalist tradition in the social sciences.

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[15]For an interesting stab at seeing what some of the issues are, see [Laland *et al.*, 2000].

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EVOLUTIONARY EXPLANATIONS

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Evolutionary theorizing in sociology and anthropology has a long and controversial history. This chapter offers an historical reconstruction of this controversy. The evolutionary theorists, theories, and studies that I consider follow logically from my answer to the question: What does it mean to be an evolutionary sociologist or anthropologist? In this historical reconstruction, it means using concepts and theories from evolutionary biology to account for human social behavior, social organization, social change,[1] and cultural evolution. All of the socioliogists and anthropologists I consider use evolutionary biological theory as the primary analytical tool for their work.

I use two interrelated, yet distinct, questions to structure the form and content of this historical reconstruction of the controversy about evolutionary theorizing in sociology and anthropology. The first question is: How did evolutionary theorists bring evolutionary biology into sociology and anthropology? To capture their two very different ways of forging this link, I use the distinction between evolutionary explanations and evolutionary analogies. Evolutionary explanations theorize or explore empirically how natural selection shaped human social behavior, social organization, social change, and cultural evolution by causing humans to behave in ways that maximize their inclusive fitness in past and current environments. Evolutionary analogies use the logic of natural selection to offer sociological and anthropological answers to sociological and anthropological questions. The second question is: How did these very different ways of bringing in evolutionary biology play out in sociology and anthropology?

To answer these questions, I offer outline accounts of exemplary efforts at evolutionary theorizing in sociology and anthropology from the nineteenth century to the present.[2] I begin by introducing central theoretical concepts of evolutionary biology that are required to understand both the content and climate for reception of evolutionary theorizing in sociology and anthropology. I then turn to my outline accounts, using the distinction between evolutionary explanations and evolutionary analogies to trace major movements in the history of evolutionary theorizing in sociology and anthropology. I begin with the evolutionary explanations of Herbert Spencer. I then use an examination of their fate to frame my analysis of the eclipse of evolutionary explanations and subsequent turn to evolutionary analogies.

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[1] When used without further qualification, the term "social change" should be taken to mean long-term, macrolevel social change.

[2] For broad surveys of evolutionary theorizing in sociology and anthropology see [Carneiro, 2003; Degler, 1991; Ingold, 1986; Maryanski, 1998; Orlove, 1980; Sanderson, 1990].

Finally I outline the return of evolutionary explanations that began with the human sociobiology debate and continue in the work of human behavioral ecologists, evolutionary psychologists, and gene-culture coevolutionary theorists. I conclude by identifying a set of unsettled issues that should drive evolutionary theorizing in sociology and anthropology in the future.

1   CENTRAL THEORETICAL CONCEPTS — SOME TERMINOLOGY FROM EVOLUTIONARY BIOLOGY

Ever since Charles Darwin published his theory of evolution by means of natural selection in 1859, the debate about evolutionary theorizing in sociology and anthropology has centered on the concept of natural selection. Darwin used natural selection to refer to the selection of traits that enhance an individual's ability to compete and survive in the struggle for existence (e.g., better foraging strategies, better ability to avoid predators). It explained differential survival of individuals. He used sexual selection to refer to the selection of traits that enhance an individual's reproductive success but do not increase its ability to survive (e.g., the tails of male peacocks and antlers of male elk). It explained the evolution of such traits because of "the advantage which certain individuals have over others of the same sex and species solely in respect of reproduction". The modern concept of natural selection includes both Darwin's natural selection and his sexual selection. For modern evolutionary biologists, natural selection is "the nonrandom survival and reproductive success of a small percentage of individuals of a population owing to their possession, at that moment, of characters that enhance their ability to survive and reproduce" [Mayr, 1997, 309]. Understood in this way, natural selection includes four ways that phenotypic traits may contribute to the relative ability of an organism to survive and transmit copies of its genes to the next generation (i.e. its fitness). The first way is by increasing the probability of survival (e.g., by being better able to get food). The second way is by increasing fecundity (e.g., by producing more gametes). The third way is by increasing access to mates (e.g., by having more attractive faces and bodies). The fourth way is by increasing the probability of fertilization (e.g., by having higher quality sperm). Traits that increase the probability of survival and fecundity of individuals with these traits relative to those without them evolve by natural selection; those that increase relative access to mates and probability of fertilization evolve by sexual selection.

Six things about this way of understanding the concept of natural selection are essential for a properly contextualized history of the controversy about evolutionary theorizing in sociology and anthropology. First, answers to questions about the role of natural selection in the evolution of particular traits, including human social behavior, constitute a distinct type of explanation in terms of ultimate or evolutionary causes. Second, when an evolutionary biologist talks about natural selection maximizing fitness, he means that selection will favor a course of action that yields the highest reproductive success in relation to competing courses of ac-

tion. "This is what he means by using the shorthand 'individuals maximize their fitness by . . . ; he does not usually wish to imply that selection will proceed to a maximum value for [fitness]" [Parker, 1984, 31-2]. Third, fitness is always relative to other individuals in the same population. Fourth, fitness is always dependent on the environment. Environmental contingency, historical specificity, and probabilism are hallmarks of evolutionary explanations. Fifth, the theoretical ideas of inclusive fitness, kin selection, parental investment, and reciprocal altruism are all connected to individual selection. These concepts were developed to explain how altruistic behavior can evolve by natural selection, where altruism is any behavior that increases another individual's fitness at a cost to one's own fitness. At their core was the fundamental insight of evolutionary biologist William D. Hamilton [1964a; 1964b] that individuals can transmit copies of their genes to future generations not only by producing offspring (direct fitness) but also by helping close relatives to produce additional or extra offspring (indirect fitness). Hamilton's concept of inclusive fitness includes both direct and indirect components of fitness. Kin selection is selection for these shared genes in individuals related by common descent, with Hamilton's rule (i.e. $rb - c > 0$, where r stands for relatedness, b for benefit to relative, and c for cost to altruist) specifying the conditions under which individuals should engage in behaviors that increase the fitness of others at a cost to the actor's own production of offspring. Parental investment is a type of kin selection, with parents putting resources into relatives (i.e. existing offspring) rather than into the production of additional offspring [Trivers, 1972; Trivers and Willard, 1973]. Reciprocal altruism [Trivers, 1971] explains apparently altruistic acts toward unrelated individuals in terms of the potential for repayment in the future in situations where the likelihood of future interaction is high.[3]

This clarification of central theoretical concepts focuses attention squarely on the individual as the unit of selection. A sixth and final thing about this way of understanding the concept of natural selection is also necessary for a properly contextualized understanding of the controversy over evolutionary theorizing in sociology and anthropology. The unit of selection (i.e. the entity or entities being actively selected in the process of natural selection) has been the subject of vigorous debate in evolutionary biology. It is important, however, not to miss the consensus for the controversy. Some participants in the debate, most notably Richard Dawkins [1977; 1982], have argued for a "gene-centered" view of evolution. As evolutionary biologist John Maynard Smith [1989, 59-60] points out, the logic of Dawkins' argument for the gene as the fundamental unit of selection may be clear "yet it seems to run counter to the obvious fact that it is, by and large, individual organisms which are the target of natural selection, and which in consequence evolve organs that ensure their own survival and reproduction. Hearts,

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[3]Hamilton later [1975, 140] took care to point out that kinship is only one way of getting the benefits of altruism to fall on individuals who were likely to be altruists. According to him, what made the inclusive fitness approach to social behavior useful was (1) that it was more general than kin selection or reciprocal altruism, (2) that it could be used where no grouping in apparent and (3) that it could be used where fitness effects were not easy to estimate or specify.

legs, teeth and kidneys really are organs that ensure the survival of individual organisms. If it is our genes, and not organisms, that replicate, why should it be organisms, and not genes, that are adapted for survival? The answer is that, so long as Mendel's laws are obeyed, a gene can increase in frequency only by making the organism in which it finds itself more likely to survive and reproduce." Other participants in the debate have argued for higher levels in the biological hierarchy as units of selection [Wynne-Edwards, 1962; D. S. Wilson, 1975; 1983; 1997; 2001]. Evolutionary biologists who treat individuals as the fundamental unit of selection do not deny the possibility of group selection. What is at issue is the relative importance of individual selection and group selection, the conditions under which group selection has significant evolutionary consequences, and the possibility of selection operating at multiple levels simultaneously. This debate about the unit of selection will figure prominently in my historical reconstruction, from Herbert Spencer right through to the unsettled issues that should drive evolutionary theorizing in sociology and anthropology in the future.

2   THE EVOLUTIONARY EXPLANATIONS OF HERBERT SPENCER

Spencer was one of the most influential thinkers of his time, largely because he was an evolutionary theorist in the modern biological sense of the term "evolution". This is how he saw himself and how his contemporaries responded to him and his work. It is what marks his place in the history of sociology and anthropology and what explains why he is singled out for individual prominence in my historical reconstruction of the controversy over evolutionary theorizing in sociology and anthropology.

Spencer developed his evolutionary explanations of human social behavior, social organization, and social change by participating in nineteenth-century debates about the fact and mechanism of biological evolution. To establish the fact of biological evolution, the first evolutionists had to dislodge the special creation solution to the organic origins problem. In his biological works, Spencer used standard nineteenth-century arguments from classification, embryology, morphology, and distribution to challenge the hypothesis of special creation. By demonstrating that the hypothesis of evolution can explain facts which anti-evolutionists claimed could be explained only by special creation and facts which special creation cannot explain, these works help to establish the fact of evolution. By 1895 evolutionary biologist August Weismann [456] could write: "*As to the fact* that evolution has taken place there can no longer be any doubt nowadays; and, accordingly, Huxley was able to affirm recently that 'if the Darwinian hypothesis [natural selection] were swept away, evolution would still stand where it is'. Certainly, evolution has the value of a fact; it is only as regards the tracing of it back to its natural causes that there is any diversity of opinion among us".

To explain the fact of evolution Spencer turned to French biologist Jean Baptiste Lamarck's mechanism of use inheritance, arguing that structures which organisms acquire during their lives through use or disuse of organs in response to environ-

mental influences can be passed on to their offspring. For Spencer, like Lamarck, acquired characters can be inherited. "All the acquisitions or losses wrought by nature on individuals, through the influence of the environment in which their race has long been placed, and hence through the influence of the predominant use or permanent disuse of any organ; all these are preserved by reproduction to the new individuals which arise, provided that the acquired modifications are common to both sexes, or at least to the individuals which produce the young" [Lamarck, 1809/1984, 113].

Spencer discovered this Lamarckian formulation of use inheritance in 1840 when he read the geologist Charles Lyell's *Principles of Geology* [1831-3]. Most of the scientific community agreed with Lyell's conclusion that this explanation of biological evolution was unscientific — but not Spencer. In use inheritance he found the evolutionary explanation of organic change that would remain one of the cornerstones of his theory of organic evolution. The introduction of its other cornerstone, embryologist Karl Ernst von Baer's law of individual development, in 1857 left the Lamarckian foundation of his theory unchanged. Spencer used von Baer's law of individual development to specify the course of organic change as a movement from homogeneity of structure to heterogeneity of structure through a process of successive differentiations and integrations. Explained by use inheritance, the transition from homogeneity to heterogeneity is contingent upon favorable environmental conditions. A more heterogeneous structure will develop only if the environment demands more complex habits. Otherwise, there will be stasis or retrogression. "We may, therefore, without any teleological implication, consider the fitness of homogenesis and heterogenesis to the needs of the different classes of organisms which exhibit them [Spencer, 1898/1966, 235].

In his early biological works Spencer used evolution and use inheritance as synonyms. Then, in 1859, Charles Darwin published his alternative explanation of biological evolution, natural selection, in *The Origin of Species*. Unlike most of his contemporaries, Spencer immediately adopted the environmental selection of random variation as a cause of evolutionary change. In a letter to Darwin on February 22, 1860 he wrote:

> You have wrought a considerable modification in the views I held — while having the same general conception of the relation of species, genera, orders &C as gradually arising by differentiation and divergence like the branches of a tree & while regarding these cumulative modifications as wholly due to the influence of surrounding circumstances I was under the erroneous impression that the sole-cause was adaptation to changing conditions brought about by habit, using the phrase conditions of existence in its widest sense as including climate, food, & contact with other organisms...But you have convinced me that throughout a great proportion of cases, direct adaptation does not explain the facts, but that they are explained only by adaptation through natural selection. [Burkhardt, *et al.*, 1993, 98]

In *The Principles of Biology*, in his other post-1859 works, and in post-1859 editions of earlier works, Spencer followed Darwin and argued that neither use inheritance nor natural selection was a sufficient cause of biological evolution. But where Darwin argued that natural selection was the principal mechanism of biological evolution in all times and all places, Spencer concluded that natural selection was the principal cause of organic change only for inferior plants and animals and for the early evolutionary stages of superior plants and animals. In higher life forms, including humans, use inheritance is the primary mechanism of biological evolution. Here, natural selection maintains environmentally-induced adaptations by eliminating unfit individuals. Because individuals whose hereditary constitutions are best adapted to the current environment survive, over time, this differential survival of individuals leads to a gradual continuous change of populations.

Spencer did not make the modern distinction between genetic evolution (i.e. information that is transmitted through genetic mechanisms) and cultural evolution (i.e. information that is transmitted through nongenetic mechanisms like learning). For him, all evolutionary change, including its sociological expression, is genetic, to use the modern term. Because acquired characters are transmitted through genes and not through culture, use inheritance also explains human social behavior, social organization, and social change. "Be it rudimentary or be it advanced, every society displays phenomena that are ascribable to the characters of its units and to the conditions under which they exist" (Spencer 1896:8-9).

Spencer's analysis of political institutions in *The Principles of Sociology* provides a particularly clear demonstration of how human social behaviors and social structures depend on the nature of their environments and the physical, emotional, intellectual, and behavioral traits of their members that provide advantages in these environments. This analysis used the militant/industrial distinction to specify two radically different types of political organization. In each type, what is important is the survival of individuals — exactly what his Lamarckian model of evolution assumes.

The militant type of society specifies the nature of political organization which accompanies chronic warfare. In this environment, the society as a whole must survive if any individual is to survive. Cooperation is therefore compulsory. Where intersocietal conflict is rare or absent altogether, joint action for offense and defense is unnecessary. Here voluntary cooperation confers survival advantages on individuals in the struggle for existence and the industrial type of society is adaptive. If individuals and societies reciprocally determine each other, as Spencer claimed, then the physical, intellectual, and emotional traits of members of industrial societies should differ from those of their militant counterparts. This is exactly what Spencer found. In industrial societies sentiments like loyalty, faith in government, and patriotism are rarely exercised while humane sentiments like honesty, truthfulness, forgiveness, and kindness evolve because of high levels of use.

By using Lamarckism to specify the nature of the relationship between individuals and society, Spencer could also offer a literally Lamarckian explanation of social change. If, as Lamarck argued, individuals must remain in harmony

with their environments and if their environments change, then individuals must also change. Social change is thus a response to environmentally-induced changes in the physical, emotional, intellectual, and behavioral traits of individuals. In Spencer's more explicitly Lamarckian terms, environmental changes create new needs, new needs require new habits, which, in turn, require changes in the physical, emotional, intellectual, and behavioral traits of individuals. These changed individuals then mold societies into corresponding forms. Because the environmental changes that trigger this process can originate in the social relations which make up a society and in the relations among societies, the individual-society relationship is reciprocal, with societies and people modifying each other through successive generations. For Spencer, the rate of social evolution is limited by "the rate of organic modification in human beings" [Spencer, 1880/1961, 366].

Bringing use inheritance together with a relational conception of society permitted Spencer to explain "the growth, development, structure, and functions of the social aggregate, as brought about by the mutual actions of individuals whose natures are partly like those of all men, partly like those of kindred races, partly distinctive" [Spencer, 1880/1961, 24]. It also permitted him to explain what will happen if government intervention interferes with the survival of the fittest. Where no such legislation exists, weak individuals die before they are able to reproduce and individuals with "fitter structures" produce the next generation. Legislation that artificially preserves the feeblest members of a society lowers the physical, emotional, intellectual, and behavioral quality of its members. Because these individuals now survive to reproduce, the process is cumulative. "From diminished use of self-conserving faculties already deficient, these must result, in posterity, still smaller amounts of self-conserving faculties" [Spencer, 1880/1961, 313]. "Law-enforced plans of relief" are also maladaptive because as "an opiate to the yearnings of sympathy" [Spencer, 1850/1966, 144, 146], they "diminish the demands made upon it, limit its exercise, check its development, and therefore retard the process of adaptation". For both reasons, interfering with the survival of the fittest exacerbates rather than ameliorates misery.

3   THE ECLIPSE OF EVOLUTIONARY EXPLANATIONS IN SOCIOLOGY AND ANTHROPOLOGY

During his lifetime, Spencer was taken seriously by his contemporaries whether or not they agreed with him. Because his evolutionary explanations eliminated both design and Designer from nature and society, some of his contemporaries dismissed his evolutionary theorizing as irreligious, purely materialistic, and inherently atheistic. Others, including Darwin, argued that it did not conform to the canonical standard for science set out in the philosophies of science of John F. W. Herschel, William Whewell, and John Stuart Mill.[4] But Spencer's high reputational standing at the time of his death suggests that most of his contemporaries found the

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[4]For Spencer's responses to these criticisms see [Haines, 1991, 1992].

reconciliation of religion and science and the philosophy of science that Spencer set out in the first volume of his Synthetic Philosophy more compelling than these criticisms. After his death in 1903, however, Spencer's reputation plummeted to the point that by the late 1930s sociologist Talcott Parsons [1937/1968, 3] could echo intellectual historian Crane Brinton's verdict that "Spencer is dead". What had changed?

Among the factors that changed the climate for reception of evolutionary explanations during this time period, three stand out: sociologist Emile Durkheim's case against biological explanations of social facts, anthropologist Franz Boas' case for what anthropologist George W. Stocking has aptly described as "the sovereignty of culture", and the close association of Spencer's evolutionary explanations and social Darwinism. Durkheim's case against evolutionary explanations focused squarely on Spencer. The defining feature of his "exclusively sociological alternative" is its holistic view of the individual-society relationship. If, as Durkheim argued, societies exist *sui generis* as entities with their own needs, goals and interests, then there is no place in a science of sociology for evolutionary explanations of human social behavior or social evolution.[5] Social facts must be explained by other social facts and the answer to the question "Functional for whom?" must always be society. This rule for explaining social facts does not mean that Durkheim ignored evolutionary biology when constructing his explanation of the transition from traditional society to modern society. Like Spencer before him, Durkheim recognized that this transition involved an increase in social differentiation. But, where Spencer believed that use inheritance explained the increase in the division of labor, Durkheim [1893/1964, 266-268] offered a very different explanation and, through this explanation, a very different way of bringing evolutionary biology into sociology and anthropology.

> If work becomes divided more as societies become more voluminous
> and denser, it is not because external circumstances are more varied
> [as Spencer argued], but because struggle for existence is more acute.

Darwin justly observed that the struggle between two organisms is as active as they are analogous. Having the same needs and pursuing the same objects, they are in rivalry everywhere. As long as they have more resources than they need, they can still live side by side, but it their number increases to such proportions that all appetites can no longer be sufficiently satisfied, war breaks out, and it is as violent as the insufficiency is more marked; that it to say, as the number in the struggle increases. It is quite otherwise if the co-existing individuals are of different species or varieties. As they do not feed in the same manner, and do not lead the same kind of life, they do not disturb each other. What is advantageous to one is without value to the others. The chances of conflict thus diminish with chances of meeting, and the more so as the species or varieties are more distant

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[5]This view of the individual-society relationship helped prepare the way for the functionalism of Malinowski and Radcliffe-Brown in anthropology and, later, of Parsons in sociology, with the work of all three further undermining evolutionary explanations in sociology and anthropology.

from one another. Thus, Darwin says that in a small area, opened to immigration, and where, consequently, the conflict of individuals must be acute, there is always to be seen a very great diversity in the species inhabiting it. He found turf three feet by four which had been exposed for long years to the same conditions of life nourishing twenty species of plants belonging to eight genera and eight classes. This clearly proves how differentiated they are.

> Men submit to the same law. In the same city, different occupations can co-exist without being obliged mutually to destroy one another, for they pursue different objects. The soldier seeks military glory, the priest moral authority, the statesman power, the business man riches, the scholar scientific renown. Each of them can attain his end without preventing the others from attaining theirs. It is the same even when the functions are less separated from one another. The oculist does not struggle with the psychiatrist, nor the shoemaker with the hatter, nor the mason with the cabinet maker, nor the physicist with the chemist, etc. Since they perform different services, they can perform them parallelly...

> That settled, it is easy to understand that all condensation of the social mass, especially if it is accompanied by an increase in population, necessarily determines advances in the division of labor.

Two things are clear from this quotation from *The Division of Labor in Society*. First, Durkheim did not reject Spencer's evolutionary explanation of the course of social change because it introduced concepts from evolutionary biology into sociology and anthropology. He rejected it because it relied on an evolutionary explanation, use inheritance, to account for this increase in social differentiation. Second, and even more important to my historical reconstruction of evolutionary theorizing in sociology and anthropology, is the very different way in which Durkheim brought Darwin into his sociology. Where Spencer offered literally Lamarckian explanations of social life, Durkheim used analogical reasoning, thus introducing what would become the dominant way of bringing evolutionary biology into sociology.

In anthropology, evolutionary explanations were targeted as part of a larger case against cultural evolutionism led by Boas [1896; 1940; 1952] and the Boasians (e.g., Kroeber, Lowie, Mead, Benedict). These anthropologists rejected the unilinear stage theories of cultural evolution set out by, among others, Edward B. Tylor [1871] and Lewis H. Morgan [1877/1964], their underlying assumption that the movement from one stage to another was inevitable, and the way the comparative method was used to produce these evolutionary sequences. In the place of the "grand system of the evolution of culture, that is valid for all humanity" [Boas, 1904, 522] they offered detailed ethnographic studies that explained differences in behavior across social groups in terms of differences in cultural traditions. Boas did not take this argument for the causal primacy of culture all the way to cultural determinism, largely because of his training in physical anthropology [Williams,

1996]. The same cannot be said for Alfred A. Kroeber's and Boasians who accepted Kroeber's concept of "the superorganic" and its corollary, the sovereignty of culture [Degler, 1991].

The impact of Boas and the Boasians on the climate of reception for evolutionary theorizing in anthropology does not end here. Talking about the sovereignty of culture neatly dovetailed into a conception of humans as *tabula rasa* organisms. If, by nature, the brain is a blank slate, then human nature is shaped by culture, most variation between human groups is cultural, and the search for evolutionary explanations of human social behavior and its products must be abandoned. That they were abandoned is clear from historian Carl Degler's [1991, 203] observation that once popular terms and concepts like "heredity," "biological influences" and "instinct" (understood as context independent) almost disappeared from the social sciences literature in the 1930s and 1940s. By setting evolutionary explanations against environmental explanations, defenders of cultural analysis in anthropology could join forces with defenders of structural analysis in sociology and argue that evolutionary explanations of human social behavior and its products rendered the environment causally irrelevant — despite the fact that environmental contingency and historical specificity are hallmarks of evolutionary theory, including its Spencerian formulation.

The impact of Durkheim, Boas, and their followers on ongoing attempts to bring evolutionary biology into sociology and anthropology was immediate. In sociology, classical human ecologists turned to the biological discipline of ecology for theories and concepts to illuminate sociological phenomena. Like Durkheim, they used analogical reasoning to bring biology in, arguing that biological communities and human communities are analogous. In an environment dominated by cultural analysis, Robert E. Park and Ernst Burgess [1921] recognized that this analogy holds only if culture is excluded. They excluded culture by treating "community" and "society" as conceptually distinct phenomena, with "community" referring to the biotic level of organization and "society" to its cultural counterpart [Park, 1936; Park and Burgess, 1921]. Biotic organization arises out of unplanned, nonrational adjustments made in the struggle for existence. It is identical, in principle, to plant and animal communities and thus the proper field of investigation for human ecologists. The customs, beliefs, artifacts, and technologies constituting the cultural level [Park, 1936] complicate, but do not fundamentally alter, the basic ecological processes. This way of bringing biology into sociology played an important role in keeping evolutionary theory alive in sociology. It did not call for evolutionary explanations of human social behavior, social organization, or social change and thus could not be attacked on the same grounds as Spencer's evolutionary theorizing.

This distancing mechanism was not an option for their anthropological counterparts who regarded the concept of culture as the major contribution of anthropology to science [Alvard, 2003]. Evolutionary anthropologist Leslie A. White [1960, ix] used two quotations to summarize the impact of Boas and the Boasians on evolutionary theorizing in anthropology during this period. "The theory of cultural

evolution [is] to my mind the most inane, sterile, and pernicious theory in the whole theory of science. . . ' These words, by Berthold Laufer, in a [1918] review praising Lowie's *Culture and Ethnology*, fairly well expressed the point of view of the Boas group which dominated much of American anthropology for decades. Twenty-three years later, Melville J. Herskovits was 'glad to affirm his belief' in an antievolutionist position [1941]".

The need to distance structural analysis and cultural analysis from evolutionary analysis became more pressing as Spencer's association with social Darwinism[6] took center stage in the controversy over evolutionary theorizing in sociology and anthropology. By paying close attention to the ethical implications of social Darwinism and the scientific legitimation of the status quo it offered, critics could hope to undermine not just Spencer's evolutionary explanations of human behavior but any and all policies derived from such explanations, including the positive and negative forms of eugenics. For them, it was not sufficient for sociologists and anthropologists to follow evolutionary biologists and abandon use inheritance in favor of natural selection when constructing evolutionary explanations. For them, any and all evolutionary explanations must be abandoned and, along with them, the notion that biology is destiny. Thus by 1944 [1959] Richard Hofstadter could use the conclusion of *Social Darwinism in American Thought* to announce the death of all evolutionary explanations in sociology and anthropology. "[S]uch biological ideas as 'the survival of the fittest'. . . are utterly useless in attempting to understand society". The "life of man in society. . . [is] not reducible to biology and must be explained in the distinctive terms of cultural analysis".

The unrelenting attack on social Darwinism continues to inform the case against Spencer's way of bringing evolutionary biology into sociology. But Spencer's most prominent and influential modern critics attacked on very different grounds. Why? The most obvious answer is that they knew something that Spencer's earlier critics could not know: Spencer got the mechanism of biological and, therefore, social evolution wrong.

Use inheritance had come under attack during Spencer's lifetime, with Spencer himself recognizing the claim by the "neo-Darwinians" that natural selection was the sole cause of biological evolution as the most serious scientific challenge to his evolutionary biology and his evolutionary sociology. Centered on the question "Can the effects of use and disuse be transmitted?" [Weismann, 1895, 421], the controversy over use inheritance intensified during the last decades of Spencer's life, with Spencer and Weismann taking the leading roles. By this time in his life (his seventies) Spencer was no stranger to controversy. But persistent health problems had long before forced him to ignore most attacks on his ideas. What was it about this controversy that led him to abandon this strategy? Simply this: Because Spencer believed that use inheritance was the also primary mechanism of social evolution, the outcome of this debate would profoundly affect his evo-

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[6]Some modern critics of Spencer have suggested that social Spencerism would be a more appropriate label for this way of applying evolutionary biological theory to humans (but see [Jones, 1980].

lutionary explanations of human social behavior, social organization, and social change. Played out in major general and specialty journals, the controversy ended with Spencer and Weismann agreeing to disagree. The neo-Darwinian threat to Spencer's evolutionary explanations was thus avoided.

Use inheritance was not abandoned by most evolutionary biologists until the Modern Synthesis of the 1940s and not conclusively refuted until the 1950s when molecular biologists demonstrated that no information contained in the properties of the somatic proteins could be transferred to the nucleic acids of DNA [Mayr, 1991]. Once it was established that phenotype cannot influence the genotype and, therefore, that acquired characters cannot be inherited, Spencer's reliance on this mechanism rendered his evolutionary explanations of sociological (and biological) phenomena obsolete. Individuals and societies do reciprocally influence each other, as Spencer argued, but this influence is not transmitted genetically through the inheritance of acquired characters, as he also argued.

The timing of this paradigm shift in evolutionary biology may have been right for modern critics to reject Spencer on these grounds but the modern case against Spencer was directed at his explanation of long-term, macrolevel social change. According to the received view, Spencer's evolutionary theorizing is a form of developmentalism; developmentalism cannot adequately account for social change and, therefore, Spencer's explanation of social evolution is irremediably flawed [Haines, 1997]. Modern critics argue that this explanation is developmental rather than evolutionary for one of four reasons: (1) it confounds the evolutionary and developmental models of change [Bowler, 1988; Nisbet, 1969] (2) its mechanism is the Lamarckian law of progressive development [Freeman, 1974], (3) its mechanism is the metaphysical principle of the persistence of force [Freeman, 1974; Peel, 1971], or (4) its mechanism is the immanent dialectic set out in the militant-industrial dichotomy [Turner, 1985; 1993].

The first argument acknowledges Spencer's debt to von Baer but misrepresents his law of individual development as a source analogy for Spencer's specification of the mechanism of organic and social evolution. It only specified the course of change. The second argument assigns Lamarckism its proper role but is marred by a fundamental misunderstanding that, for Spencer, Lamarkism meant an inherent tendency toward progress or perfection. Lamarckism, for Spencer, meant use inheritance. The third argument misconstrues the logical status of the persistence of force and its role in Spencer's biology and sociology [Haines, 1992]. The persistence of force is not a metaphysical principle. It is a fundamental law which together with the law of evolution generates the kind of causal explanation that Spencer called ultimate-causal explanation. Ultimate-causal explanations verify proximate causal explanations (e.g., use inheritance and natural selection) by deducing them from simpler and more general laws. Because the principles of biology and principles of sociology are the most general laws of the sciences of biology and sociology, they can be deductively interpreted only by documenting their affiliation on the law of evolution. By tying together facts from biology and sociology, the persistence of force pointed to a fundamental unity that underlies their appar-

ent diversity — exactly what the Whewell-Herschel-Mill canons of good science prescribed. The fourth argument misses Spencer's Lamarckian argument for the social environment as the major source of adaptational variation. Whether the organization for offence and defense or the sustaining organization is more "largely developed" depends upon the nature of the interactions that occur between a society and its neighboring societies in the struggle for existence. If these interactions are hostile, then militancy evolves; if peaceful, then industrialism is adaptive. Because the shift from militant to industrial organization occurs only in response to changes in the superorganic environment, societies do not cycle between militancy and industrialism regardless of their superorganic environments as Turner's immanent dialectic suggests.

4   FROM EVOLUTIONARY EXPLANATIONS TO EVOLUTIONARY ANALOGIES

The modern critique of Spencer's evolutionary theorizing both reflected and contributed to the changing climate for the reception of evolutionary explanations in sociology and anthropology. It reinforced the shift from human social behavior to social organization, social change, and cultural evolution as the foci of evolutionary social science. It also reinforced the shift away from evolutionary explanations of these phenomena that had begun with Durkheim, gained momentum in the 1920s with the work of Boas and the Boasians, and accelerated in the 1940s in response to the atrocities of Nazi Germany. By the 1950s the "substance of academic debate had dramatically changed. Any hereditarian explanation of social or cultural characteristics or ability was prone to be classified as racist. Naturalism and biological reductionism were generally viewed with suspicion" [Barkan, 1992, 342]. It is not surprising, then, that the revival of evolutionary theorizing that began in the 1950s and continued through the 1960s did not rely on evolutionary explanations to bring evolutionary biology back into sociology or anthropology.

In anthropology the works of V. Gordon Childe, Julian H. Steward, Leslie A. White, Marshall D. Sahlins, Elman R. Service, and Marvin Harris exemplify approaches that played key roles in a "return to evolutionism" — a return that White [1960, vii] concluded was "inevitable if progress was to continue in science and if science was to embrace cultural anthropology." In *Evolutionism in Cultural Anthropology*, Robert L. Carneiro [2003, 116-117] suggests that Childe's [1942; 1951] case against "uncritical and indiscriminate diffusionism" and his focus on the dynamics of cultural evolution rather than on typology and chronology, "helped to soften the ground on which the tree of a resurgent evolutionism was to take root and grow tall." Steward's [1937; 1949; 1955; 1956] ecological anthropology focused attention on the effect of the environment (e.g., the quality, quantity, and distribution of resources) on culture. By demonstrating empirically how particular groups adapt to specific environments, his method of cultural ecology challenged the Boasians' claim for the autonomy of culture — at least for elements of culture like technology, and production systems that constituted "the culture core" [Orlove, 1980].

White [1943; 1945; 1949; 1959] focused more directly on cultural evolution, identifying levels of energy use as a key determinant. Like White, Sahlins and Service [1960] were more concerned with general principles that were fundamental to the theory of cultural evolution than with specific adaptations. Their edited book, *Evolution and Culture*, helped accelerate the return to evolutionary theorizing in anthropology, with Sahlins' [1960] distinction between specific evolution and general evolution playing a major role. Harris [1968; 1977; 1979] played a key role in the development of neo-functionalist evolutionary theorizing in anthropology. His use of analogical reasoning to bring evolutionary biology into anthropology and his choice of analog reproduced emerging developments in evolutionary sociology at this time. Because evolutionary analogies became *the* way to bring evolutionary biology into sociology, I use outline accounts of the evolutionary theorizing of sociologists Amos Hawley, Otis Dudley Duncan, and Talcott Parsons to offer an historically contextualizing evaluation of the move to analogical reasoning that occurred in both anthropology and sociology.

4.1   The ecological theorizing of Amos Hawley and Otis Dudley Duncan

Existing historical reconstructions of evolutionary theorizing in sociology routinely point to the success of its modern ecological variants and to the key role of the work of Amos Hawley in this success. Hawley's (1950) theory of community structure and development forms the principal conceptual bridge between classical and contemporary ecological theorizing in sociology. In *Human Ecology* Hawley responded to the most serious criticism of classical human ecology (e.g. by [Firey, 1945; Wirth, 1945] by abandoning the conceptual distinction of community and society. But he did not treat culture as a basic explanatory concept, arguing instead that the term culture "simply denotes the prevailing techniques of adjustment by which a population maintains itself in its habitat. The elements of culture are therefore identical in principle with the appetency of the bee for honey, the nest-building activities of birds, and the hunting habits of carnivora" [Hawley, 1950, 69].[7] By making this argument, Hawley could retain both the conceptual apparatus of classical ecological theorizing and its underlying assumption that biological communities and human communities are analogous. This analogy reached its fullest development in his own ecological theorizing, with its central hypothesis of the community as the essential adaptive mechanism. According to this hypothesis, human communities are adapted units which exist *sui generis* as entities characterized by emergent properties. For Hawley [1950. 42], a community is "a collective response to the conditions of a given habitat".

   In the preface of *Human Ecology*, Hawley [1950] openly acknowledged that his hypothesis of the community as the essential adaptive mechanism and the ecologi-

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[7]As McPherson [2004] points out, culture plays a more important role in Hawley's last work. This work focuses on how advances in technologies of transportation and communication expand the range of possible interrelationship structures within an environment.

cal theory it anchors follow logically from his use of the superorganismic concept of community as the source for his biological analogy. As superorganisms, biological communities are important units of selection, their boundaries are well defined, and temporal community change (succession) is a deterministic process analogous to the development of an individual organism.

> The unit of vegetation, the climax formation, is an organic entity. As an organism, the formation arises, grows, matures, and dies. Its response to the habitat is shown in processes or functions and in structures which are the record as well as the result of these functions. Furthermore, each climax formation is able to reproduce itself, repeating with essential fidelity the stages of its development. The life-history of a formation is a complex but definite process, comparable in its chief features with the life-history of an individual plant. The climax formation is the adult organism, the fully developed community, of which all initial and medial stages are but stages of development. Succession is the process of the reproduction of a formation, and this reproductive process can no more fail to terminate in the adult form of vegetation than it can in the case of the individual plant [Clements, 1916, 124-125]; see also [Clements and Shelford, 1939; Allee *et al.*, 1949].

The subject of this analogy, human communities, are also self-regulating entities characterized by organismic cohesion or integration, with members working together to produce a unified whole which benefits all members. Adaptation is thus a collective phenomenon that involves cooperation of all of the organisms in a given area. "Students of life are in general agreement that the universal tendency is for organisms to confront the environment not as individuals but as units in a cooperative effort at adaptation" [Hawley, 1950, 30]. Like biological communities, human communities change as units through a process of succession. "The community, like the individual organism, is something that grows. It proceeds from small beginnings, passes through a series of developmental stages, and eventually attains a mature [climax] state" [Hawley, 1950, 52].

The superorganismic concept of community dominated biological ecology until the late 1940s. It was not without its critics, however. In a series of articles, Henry A. Gleason [1926; 1939] used his empirical observations and quantitative studies of plant communities to argue for his "individualistic concept of community." According to it, the "vegetation unit is a temporary and fluctuating phenomenon, dependent, in its origins, its structure, and its disappearance, on the selective action of the environment and the nature of the surrounding vegetation" [Gleason, 1939, 93]. Each community is the result of a unique combination of individual species and environmental conditions. These combinations are unique because no two species share the same niche and because no two environments are identical. Individuals from species whose physicochemical tolerances and requirements are met in a particular area survive; migrants from other species die. "Communities come and go, mere temporary alliances of plants thrown together by fate and

history" [Colinvaux, 1978, 72].

Gleason's articles were published in major ecology journals but they were ignored by most American ecologists until the 1940s. In 1947 *Ecological Monographs* published three major papers [Cain, 1947; Elger, 1947; Mason, 1947] that, together with the concept of the vegetational continuum (Curtis) and gradient analysis (Whittaker) compelled most biological ecologists to abandon the superorganismic concept of community in favor of the individualistic alternative [MacIntosh, 1985; Sheail, 1987; Simberloff, 1980].

Not all biological ecologists accepted this paradigm shift. Eugene P. Odum's influential textbook, *Ecology* [1953], did not cite Gleason's individualistic concept of community in the first two editions [MacIntosh, 1985]. His concepts of ecosystem and ecosystem development [Odum, 1969] reintroduced the superorganismic concept of community into biological ecology, albeit in the terminology of holism and emergent properties. By grounding his ecological theorizing in the biological ecology of Allee *et al.*, [1949] and Odum [1953], Duncan reproduced the biological framework that had dominated ecological theorizing in sociology since its origins. In his POET model or "ecological complex", a population (P) is a system with emergent properties, social organization (O) is the collective adaptation of a population to its biotic and abiotic environments (E), the web of life is a system of organization, and human communities, like biological communities, are entities which compete for survival [Duncan and Schnore, 1959; Duncan, 1961; 1964]. The POE of the POET model instantiates the same assumptions about the nature of human communities as Hawley's biological source, the superorganismic concept of community.[8] The inclusion of the technology (T) element in the ecological complex reflects Duncan's belief that changes over time can be explained as adjustments to technological change. The ecological complex thus links community structure [Hawley, 1950] with community change [Ogburn, 1950] through its emphasis on the reciprocal influence of each element of the ecological complex on each other element.[9]

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[8]In his 1984 comparison of ecological and Marxist approaches to urban sociology, Hawley continued to ground his ecological theory in the superorganismic concept of community — as evidenced by the biologists he cites (e.g., [Clements and Shelford, 1939; Odum, 1969] and the way he described ecological processes (e.g., "Succession involves a recurring competition among associations of species for possession of a habitat giving rise to a series of displacements of one association of species by another until a climax phase is reached, making the end of the process" [909].

[9]It was this emphasis on functional interdependence or interrelatedness that prompted Duncan to introduce the concept of ecosystem from biology into sociology. In later writings he focused on the ecosystem processes of energy flow, biogeochemical cycling, and information flow. He continued to follow Allee and Odum and offered holistic interpretations of these processes, even though these explanations had been successfully challenged in biological ecology [Goodman, 1975; Harper, 1977; McNaughton, 1977].

4.2   The evolutionary theorizing of Talcott Parsons

There can be no doubt that advances in ecological theorizing in sociology and anthropology helped keep evolutionary theorizing alive in both disciplines. The 1960s also saw a resurgence of interest in evolutionary theories of social and cultural change, with Talcott Parsons playing a leading role. In the face of his ringing endorsement of Brinton's death knell for Spencer, Parsons may seem an unlikely advocate of evolutionary theorizing in sociology and anthropology. But, where Spencer offered an evolutionary explanation of social change that treated the individual as the unit of selection, Parsons opted for an evolutionary analogy, arguing that sociocultural evolution comes about through a process analogous to natural selection.

Parsons' source for this evolutionary analogy was biologist A. E. Emerson's model of group selection. This model of group selection explains biological evolution in terms of heritable variation and selection but the unit of selection is the group and not the individual. "Some biologists have been loath to recognize the group as a system. We have many erroneous concepts of group behavior based upon inability to conceive of the group as an entity of more than a statistical summation of individuals" [Emerson, 1956, 148]. To avoid errors that follow from treating the individual as the sole unit of selection, Emerson argued that groups are adapted units with functional divisions of labor analogous to those of biological organisms, that their properties are emergent properties and, therefore, that group traits can evolve only if natural selection operates on genetically similar populations as units. Through the selection of entire groups, groups with detrimental genetic traits are eliminated in competition with groups that lack these traits. What is important in biological evolution is group survival and not the survival of individuals.

This systems concept of group and its evolutionary corollary that selection operates on groups as units form the core of Parsons' theory of sociocultural evolution.[10] For Parsons [1961; 1964; 1966; 1971], like Durkheim before him, societies (the groups which are the focus of his theory of sociocultural evolution) are adapted units that exist *sui generis* as entities characterized by emergent properties. Societies vary in their principal structural components (e.g., instrumental complex, kinship system, power system, religion, and cultural complex) and thus in their potentials for further evolution. Selection operates on this structural variation.

For Parsons, advancement or "progress" presupposes the development, by innovation or adoption by diffusion, of evolutionary universals. These structural innovations confer survival advantages on societies in inter-societal competition. Therefore, societies that develop or adopt evolutionary universals increase their generalized adaptive capacity. Other societies disintegrate, remain structurally unchanged in insulated environments, or are absorbed by more advanced societies. For Parsons, then, sociocultural evolution is the differential proliferation and

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[10]For a detailed analysis of Parsons' intellectual debt to Emerson, see [Haines, 1987].

extinction of societies, the causes of social change are found in the characteristics of societies, and what matters is group (societal) survival.

Parsons used these evolutionary arguments to construct a structural taxonomy or sequential ordering of societies by level of advancement. It divided the evolution of societies is into three stages: primitive, intermediate, and modern. In making the claim that sociocultural evolution has proceeded from simple to progressively more complex forms, Parsons was careful to point out that sociocultural evolution was neither unilinear nor inevitable, thus further distancing himself from Spencer.

Did these distancing mechanisms work? How did Parsons' evolutionary theorizing fare in sociology? Compared to Spencer's earlier effort at evolutionary theorizing, it did not generate much criticism for two reasons. First, Parsons' way of bringing evolutionary biology into sociology did not produce evolutionary explanations of human social behavior, social organization, or social change. By using analogical reasoning, Parsons could offer a sociological explanation of long-term, macrolevel social change. Second, Parsons' evolutionary analogy did not challenge orthodoxy in either evolutionary biology or sociology. The for-the-benefit-of-the-group form of group selection was widely accepted in evolutionary biology at the time Parsons constructed his evolutionary analogy. Its holistic view of the individual-group relationship neatly dovetailed into the functionalist conception of the individual-society that dominated sociology at this time, largely through Parsons' own efforts. This does not mean that Parsons' evolutionary theorizing was without critics, however. The most prevalent and damning line of attack argued (incorrectly) that, like Spencer's evolutionary theorizing, Parsons' theory of sociocultural evolution is a form of developmentalism and thus must be dismissed as irremediably flawed [Giddens, 1984; Nisbet, 1969; Smith, 1973; Sztompka, 1993]. For Parsons and his critics, as for evolutionary anthropologists and their critics, the controversy over evolutionary theorizing in sociology and anthropology was still played out in the theory arena. This would change with the debate about human sociobiology that began in 1975.

5   THE HUMAN SOCIOBIOLOGY DEBATE

The debate about sociobiology, especially human sociobiology, will forever be associated with three names: Edward O. Wilson, Richard C. Lewontin, and Stephen Jay Gould — Wilson for writing *Sociobiology: The New Synthesis* [1975], the book that launched the debate and Lewontin and Gould for their relentless attack of sociobiology and its practitioners. What is sociobiology? What was it about Wilson's book that made it so controversial? How did the sociobiology debate play out in sociology and anthropology?

In *Sociobiology: The New Synthesis*, Wilson (1975:4) defined sociobiology as "the systematic study of the biological basis of all social behavior", where "biological basis" means possible ultimate causes and "all" means that sociobiological theory can be used to study human social behavior. Understanding what "biological basis" means is key to understanding the sociobiology debate because the

concern with ultimate questions and causes is what makes sociobiological explanations of social behavior "evolutionary". As behavioral ecologist John Alcock [2001a, 10] stresses: "Wilson's one-sentence definition of the discipline may suggest that any scientist working on any biological aspect of social behavior qualifies as a sociobiologist. But in reality persons who call themselves sociobiologists, or at least those who tolerate this label, invariably use evolutionary theory as the primary analytical tool for their work. These individuals usually ask and try to answer one basic question: What role did natural selection play in shaping the evolution of this society or that social behavior?"[11]

The core concepts of this new synthesis of the theory and empirical findings of behavioral ecologists and evolutionary biologists more generally (e.g., [Alexander, 1974; Hamilton, 1964a; 1964b; Lack, 1966; Trivers, 1971; Williams, 1966] are instantiated in sociobiological solutions to what Wilson [1975, 3] identified as the "central theoretical problem of sociobiological analysis": "How can altruism, which by definition reduces personal fitness, possibly evolve by natural selection?"[12] One possibility was the for-the-benefit-of-the group explanation of group selectionist models of natural selection like those of ecologists Emerson and Vero Wynne-Edwards. These ecologists treat group traits or social phenomena as emergent properties of social systems that can only be explained by hypotheses that are distinct from those designed to explain individual phenomena. Wynne-Edwards' [1959; 1962; 1963] hypothesis of population regulation is one such hypothesis that made the group selection thinking that was prevalent at the time explicit [Maynard Smith, 1976; 1989]. It explains altruism this way: population size is regulated by behavioral controls that subordinate individual fitness to population fitness. Where there controls operate, individuals reproduce at lower than maximal levels. This reproductive restraint on the part of individuals prevents the extinction of the populations to which they belong. Groups without these controls exceed the carrying capacities of their environments and are driven to extinction. Population fitness thus "depends on something over and above the heritable basis that determines the success of individuals of a continuing stream of independent members" [Wynne-Edwards 1963, 624]. It depends on the selection of entire groups.

This hypothesis and the group selectionist model it instantiated were abandoned by evolutionary biologists by the late 1960s, with George C. Williams' [1966] compelling critique of group selection playing a major role in this paradigm shift (see also [Lack, 1966; Maynard Smith, 1964]. In *Adaptation and Natural Selection* Williams [1966] argued that the varying degrees of cohesion and social organization that populations show can be explained by individual selection. Most evolutionary biologists at the time were convinced by these arguments that "behaviors which conform to the 'interests of the group,' in Wynne-Edwards's sense, are basically

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[11]This focus on natural selection does not mean that sociobiologists ignore other causes of evolutionary change such as genetic drift.

[12]Until recently, theoretical discussions about and empirical studies of altruism focused on helping behaviors. Altruistic punishment is now receiving more attention by evolutionary social scientists.

the result of individual adaptations to individual interests" [Wiens, 1966, 279]. Sociobiologists agreed. Like evolutionary biologists more generally, sociobiologists may have come down on different sides of the individual versus gene as the unit of selection but they joined forces in rejecting this form of group selection and its corollary that social behaviors evolve to benefit the group as a whole.

The position that sociobiologists take in the unit of selection debate thus cannot explain why Wilson's "new synthesis" of his and other biologists' evolutionary analyses of social behavior generated so much controversy. Nor can it be explained by sociobiologists' use of inclusive fitness, kin selection, reciprocal altruism, and parental investment theory to solve the problem of altruism and to account for the differential reproductive success of individuals more generally. The answer to the question of why sociobiology was so controversial lies elsewhere — in Wilson's inclusion of "all" in his definition of sociobiology, thus claiming for sociobiology human social behavior — the traditional domain of the social sciences.

In assessing this claim and the reaction to it, it is important to recognize that the bulk of *Sociobiology* did not deal with humans. The discussion of the sociobiology of humans is addressed in the first and last chapters of a 27-chapter book and makes up less than 5% of the book. But in a climate where experiences with Nazi domination, genocide, eugenics, compulsory sterilization, and the IQ controversies ignited by works of Jensen [1969] and Herrnstein [1973] had rendered biological explanations of human behavior extremely suspect, Wilson's call for extending sociobiology to humans was bound to meet with resistance. The way in which he framed this call did nothing to allay these fears.

> For the present it [sociobiology] focuses on animal societies... But the discipline is also concerned with the social behavior of early man and the adaptive features of organization in the more primitive contemporary human societies. Sociology *sensu stricto*, the study of human societies at all levels of complexity, still stands apart from sociobiology because of its largely structuralist and nongenetic approach. It attempts to explain human behavior primarily by empirical description of the outermost phenotypes and by unaided intuition, without reference to evolutionary explanations in the true genetic sense. It is most successful, in the way descriptive taxonomy and ecology have been the most successful, when it provides a detailed description of particular phenomena and demonstrates first-order correlations with features of the environment. Taxonomy and ecology, however, have been reshaped entirely during the past forty years by integration into the neo-Darwinist evolutionary theory — the "Modern Synthesis", as it is often called — in which each phenomenon is weighed for its adaptive significance and then related to the principles of population genetics. It may not be too much to say that sociology and the other social sciences, as well as the humanities, are the last branches of biology waiting to be included in the Modern Synthesis. One of the functions of sociobiology, then, is to reformulate the foundations of the social

sciences in a way that draws these subjects into the Modern Synthesis. Whether the social sciences can be truly biologicized in this fashion remains to be seen [Wilson, 1975, 4].

To show what this way of "biologicizing" the social sciences would entail, Wilson used the last chapter of *Sociobiology* to suggest how human sex roles, aggressiveness, moral concerns, and religious beliefs could be connected to our evolutionary heritage, as represented in our underlying genetic dispositions. Small wonder, then, that most sociologists and anthropologists read this call for evolutionary explanations of human social behavior as both a threat to sociology and anthropology as autonomous disciplines and "the latest reincarnation of social Darwinism". This way of engaging sociobiology stuck, in large part because E. O. Wilson's most influential critics were not sociologists, anthropologists, or even social scientists. They were Lewontin and Gould, evolutionary biologists and Wilson's colleagues at Harvard.

The Lewontin-Gould response to Wilsonian sociobiology was immediate. It began with their signing a letter to the *New York Review of Books* (with other members of the Sociobiology Study Group of Science for the People) that Lewontin wrote to counter a positive review of *Sociobiology* by the embryologist and evolutionary theorist C. H. Waddington [1975]; [Allen *et al.*, 1975]. This letter linked sociobiology to earlier theories that "provided an important basis for the enactment of sterilization laws and restrictive immigration laws by the United States between 1910 and 1930 and also for the eugenics policies which led to the establishment of gas chambers in Nazi Germany" by arguing that the "latest attempt to reinvigorate these tired theories comes with the alleged creation of a new discipline, sociobiology." The Lewontin-Gould response then took the form of an extended critique in the March 1976 issue of *BioScience*. The impact of "Sociobiology — Another Biological Determinism" and its endorsement of the view of sociobiology as "an effort to cloak in modern terminology the age-old political doctrine that the main features of human social existence are biologically determined" [Alper *et al.*, 1976, 424] cannot be overstated.[13] By focusing attention on the political significance of sociobiology, the Lewontin-Gould response helped to define the terms of the debate about human sociobiology in a way that deflected attention from attacks on scientific grounds and from the theoretical and empirical responses to these criticisms. Nonhuman sociobiology may have benefited from the scientific dimension of the sociobiology debate [Alcock, 2001a] but the same cannot be said for human sociobiology, especially in sociology.

E. O. Wilson [1976] and others (e.g., [Alcock 2001a; Segerstråle, 2000] have suggested that Lewontin and Gould were motivated more by their Marxism than by their scientific objections to Wilsonian sociobiology. Gould later acknowledged that attacking first on political grounds may have dampened the effect of their scientific criticisms of sociobiology — a conclusion that the debate about human

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[13]E. O. Wilson [1976] responded to this critique in the same issue but his response, "Academic Vigilantism and the Political Significance of Sociobiology", was largely ignored.

sociobiology in sociology and anthropology corroborates. The Lewontin-Gould assessment of the political significance of sociobiology was taken up by sociologists and anthropologists who, like academics more generally [Segerstråle, 2000, 14], turned to Wilson's critics rather than to Wilson's writings for their understanding of sociobiology.

It is no surprise, then, that most sociologists and anthropologists followed Gould and Lewontin and attacked on political grounds, dismissing sociobiology as yet another use of biology in the service of ideology. Sahlins' [1976, x] widely-read and cited book, *The Use and Abuse of Biology: An Anthropological Critique of Sociobiology*, captured this response in its interpretation of E. O. Wilson's call for biologicizing sociology and anthropology as a call for "a major redirection in social science thinking. . . . *Sociobiology* challenges the integrity of culture as a thing-in-itself, as a distinctive and symbolic human creation. In the place of a social constitution of meanings, it offers a biological determination of human interactions with a source primarily in the general evolutionary propensity of individual genotypes to maximize their reproductive success". Bioanthropologist Lionel Tiger's personal reflections on life in "the human nature wars" draws a similar picture of the dominant response to sociobiology but from the other side: "Throughout the 1970s and early 80's, the opposition to the biosocial — or sociobiological — enterprise grew more heated. I felt a sense of almost physical apprehension, knowing how easily I could become the object of censure. At the meetings of the American Anthropological Association, conversation would stop and people would stare when I entered an elevator and they saw my name tag" [Tiger, 1996] as cited in [Segerstråle, 2000, 143].

Other sociologists and anthropologists attacked human sociobiology on scientific grounds but these attacks reproduced persistent misunderstandings that dominated scholarship on sociobiology more generally and continue to dominate negative sociological and anthropological engagements of human sociobiology [Alcock, 2001a]; see also [Crippen, 1994; Machalek and Martin, 2004; Sahlins, 1976]: (1) the view of sociobiology as a form of biological or genetic determinism, with genes determining an organism's behavioral traits regardless of the environment in which it develops, (2) the view that the reductionist principles of sociobiology could explain only an insignificant portion of social and cultural differences and similarities, (3) the view that sociobiology is the search for a particular gene for a particular behavioral trait, (4) the view that sociobiology engages in the production of untested and untestable "just-so stories", (5) the view that sociobiologists argue that what is must be, (6) the view that sociobiology equates "natural" with moral or ethical, and (7) the view that the ultimate causal explanations of human social behavior that sociobiologists seek are more important than the explanations of sociologists and anthropologists. These misunderstandings help to shore up the widespread view that human sociobiology seeks to reduce culture to genes, structure to genes, and thus sociology and anthropology to biology.

As Alcock [2001a] convincingly argues in *The Triumph of Sociobiology*, all of these views of sociobiology have been challenged successfully. Like other evolu-

tionary biologists, sociobiologists accept the interactive theory of development for all animals, including humans. Phenotypic characters of organisms are products of complex interactions between their genes and their biophysical, developmental, social, and cultural environments. Sociobiologists thus do not, and indeed cannot, claim that genes determine an organism's behavioral traits regardless of the environment in which the organism develops. As Dawkins [1982, 10] points out, when sociobiologists talk about 'a gene for' a trait, this is "never used to imply genetic determinism but rather as a shorthand for. . . genetic differences between individuals that are potentially or actually subject to selection." For sociobiologists, a biological basis for social behavior, including human social behavior, means only that a genetic disposition makes a contribution to the behavior phenotype. And, as E. O. Wilson himself stressed, the extent to which this is the case is an empirical question. The erroneous claim that sociobiologists search for a particular gene for a particular behavioral trait may also reflect the widespread use of the shorthand "gene for" such and such a trait in sociobiological literature. The views that sociobiologists argue that what is must be and that "natural" signals moral or ethical may reinforce the received view of the political implications of sociobiology but both presuppose that sociobiologists deny the role of social and cultural environments in the evolution of human social behavior. What sociobiolgists do argue is that genes affect behavior, including human behavior, to some degree and that the extent and nature of their influence is an empirical question. This routine practice of deriving and testing hypotheses also challenges the view that sociobiology is pure speculation. Alcock [2001a, 64] has described Gould's "just-so story" epithet as "one of the most successful derogatory labels ever invented" — largely because this epithet persists in the face of incontrovertible evidence that sociobiologists generate ultimate-causal explanations by deriving testable predictions about specific behavioral traits from sociobiological theory. Sociobiologists do not claim that this kind of explanation is more important than the causal explanations of sociologists and anthropologists. What they claim is that ultimate-causal and proximate-causal explanations are both necessary for a complete explanation of human social behavior, social organization, social change, and cultural evolution.

Not all sociologists and anthropologists adopted the hostile, dismissive response to sociobiology that dominated sociology and much of anthropology at this time. Some (e.g., [Boorman and Levit, 1980] used review articles to demonstrate the usefulness of sociobiological concepts, theories, and models for answering questions of interest to sociologists and anthropologists. Others, including those who called themselves human sociobiologists (e.g., [Ellis, 1977; van den Berge, 1977] argued (with limited success) that sociologists and anthropologists must take seriously the biological foundation of human social behavior and thus seek evolutionary explanations of human social behavior, social organization, social change, and cultural evolution. Evolutionary sociologist Gerhard Lenski [1976a, 531, 530] agreed that it was time to "abandon the extreme environmentalism to which we have been for too long committed". But he opted for a very different response to Wilson's call for bringing sociology into the Modern Synthesis: "Many will read this as a

classic example of intellectual imperialism, but I do not think it is intended that way. Rather, Wilson stands in the tradition of Sir Julian Huxley, George Gaylord Simpson, Theodosius Dobzhansky, Alfred E. Emerson, and other proponents of the Modern Synthesis who have tried repeatedly, without much success, to open up a dialogue between biologists and social scientists (although recently an number of anthropologists have begun to respond)." For Lenski, opening up this dialogue did not mean abandoning evolutionary analogies in favor of evolutionary explanations nor did it mean becoming an active participant in the debate about human sociobiology. In evolutionary sociology, the dominant response was more evolutionary analogies.

6   MORE EVOLUTIONARY ANALOGIES

6.1   *The ecological-evolutionary theory of Gerhard Lenski*

The evolutionary theorizing of Lenski [1970; 1976b]; Nolan and Lenski [1999][14] occupies an important place in my historical reconstruction for two reasons. First, because it spans the period immediately before and after the publication of *Sociobiology,* it is a strategic site for exploring the impact of the debate about human sociobiology on evolutionary sociology. Second, because Lenski tied his own evolutionary theorizing to complementary developments in evolutionary anthropology (Harris' neo-functionalist evolutionary theorizing, Sahlins' distinction between specific and general evolution), I can use my outline account of Lenski's ecological-evolutionary theory to bring these efforts at evolutionary theorizing in anthropology into my historical reconstruction.[15]

Like Parsons before him, Lenski argued that societies evolve through selection operating on a set of variations. At the core of his theory is Sahlins' [1960] distinction between general evolution and specific evolution. In Lenski's application of this distinction, specific evolution explains continuity and change within individual societies. At this level of analysis, change in not necessarily directional. To explain general patterns of history (e.g., the accelerating rate of increase of the human population, the expansion of the human population into new environments, the growth in the production of goods and services, and the increase in the scale and complexity of social structure), Lenski turned to general evolution. Its referent is not an individual society but all societies, past and present.

Specific evolution and general evolution are both processes of variation and selection [Lenski and Lenski, 1982; Lenski, 1983]. But the sources of variation and the mechanisms of selection are not the same in these two processes. In specific evolution intra-societal selection operates on random or purposeful variations in so-

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[14]Between 1970 and 1999 *Human Societies* went through eight successive editions.

[15]Sanderson [2002, 440] has described his evolutionary materialism (see [Sanderson, 1995; 1999] as "a more recent formalization and extension of Harris' model of social evolution". But, it is important to point out, that where Harris used the for-the-benefit-of-the-group model of group selection to ground his theory, Sanderson treats individuals as the unit of selection.

ciocultural elements produced by innovation, invention, or diffusion. Intra-societal selection is a relatively rational process in which individuals choose sociocultural elements which are in their own best interests.

In general evolution, by contrast, the units of selection are societies (defined holistically in the manner of Durkheim, human ecologists, Parsons and for-the-benefit-of-the-group group selectionists), the criterion of selection is degree of technological advance, and the outcome of this process of intersocietal selection is "as blind and purposeless as the outcome of the biological process of natural selection and just as indifferent to human beliefs and values, except for the genetically-based instinct of self-preservation which humans share with other animals. If technologically advanced societies tend to survive while more primitive societies are eliminated, it is not because the former are superior in anything except their possession of survival-relevant resources" [Lenski, 1983, 204]. For Lenski, then, intersocietal selection is an independent selective force that overrides intra-societal selection and the basic trends of human history "have been largely unintended and the product of impersonal social processes beyond human comprehension and control" [Lenski, 1983, 205].[16]

In his post-*Sociobiology* works, Lenski did not abandon his use of analogical reasoning to bring evolutionary biology into sociology or his focus on long-term, macrosocial change in favor of evolutionary explanations of human social behavior. Nor did he change his theory in response to the shift from group selection to individual selection that had begun in evolutionary biology in the mid-1960s and that formed the core of sociobiology. His ecological-evolutionary theory, like Parsons' evolutionary theory, exemplifies the use of evolutionary analogies in sociology. So does contemporary ecological theorizing.

6.2   Contemporary Ecological Theorizing In Sociology

In the late 1970s the focus of ecological theorizing in sociology shifted from communities and ecosystems to organizations and from ecological analogies to more explicitly evolutionary analogies of variation and selection.[17] The most prolific stream of this ecological research is the population ecology of organizations developed by Michael Hannan, John Freeman and their students and colleagues (cf. [Dobrev, Kim, and Hannan, 2001; Hannan and Freeman, 1989; Hannan, Carroll, and Polos, 2003]. Hannan and Freeman's [1977] paper in *American Journal of Sociology* sets out the core ideas of their ecological theorizing and its links with biological ecology. In it they identified the central problem of organizational ecology as

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[16] For recent assessments of the theoretical and empirical implications of Lenski's ecological-evolutionary theory see the papers by Kennedy, Nielsen, and Nolan in the 2004 special issue of *Sociological Theory*, "Religion, Stratification, and Evolution in Human Societies: Essays in Honor of Gerhard E. Lenski".

[17] That this shift did not signal the end of what Turner [2001, 9] calls "grand forms" of ecological theorizing offering accounts of large-scale, long-term changes in society is clear from Hawley's [1986] last work, *Human Ecology: A Theoretical Essay* and Turner's own [1994] ecology of macrostructure which takes ecological analysis to a system level.

"Why are there so many kinds of organizations?" [936]. To explain why "the diversity of organizational forms is isomorphic to the diversity of environments" [939], they brought Hawley's ecological theorizing together with theoretical and empirical arguments about organizational inertia. But where Hawley used the superorganismic concept of community as the source for his analogy, Hannan and Freeman turned to population ecology, using models of ecological competition (e.g., the Lotka-Volterra equations) and niche theory to develop a selection model of organization-environment relations. By arguing that the individual organization is analogous to the individual organism in biological ecology, they could apply core concepts of population ecology (e.g., natural rate of increase, carrying capacity, niche width (specialism or generalism), competitive exclusion, fitness-set theory, and grain) to organizations.

This way of reopening the lines of communication with biological ecology generated an ecological alternative to the prevailing adaptive model of organization-environment relations, with its focus on strategic choice. The adaptive model explains ecological isomorphism as a consequence of purposeful actions of managers, dominant coalitions, or boards of directors of organizations to adapt organizations to environmental constraints. Under the selection model of Hannan and Freeman [1977, 952], by contrast, "the problem of ecological adaptation can be considered a game of chance in which the population chooses a strategy (specialism or generalism) and then the environment chooses an outcome." Specialism is favored in stable or certain environments and where environmental variation is fine-grained. In other environments, generalists out-compete specialists for resources and thus become more prevalent in the population of organizations. For Hannan and Freeman, then the "*environment* was what optimized the fit between form and resources, not the internal adaptation of the organization itself" as Hawley had claimed [McPherson, 2004, 230]. Selection explains why there are so many kinds of organizations, how they are distributed across space, and how they change over time.

Aldrich's [1979, 27; McKelvey and Aldrich, 1983] formulation of organizational ecology also rejects the adaptive model in favor of a population ecology model that is "based on the natural selection model of biological ecology".[18] Aldrich acknowledged Hawley as an intellectual predecessor but, as Scott [1981] points out, Aldrich's ecological theorizing is best read as a contribution to the transition from closed to open systems models of organizations that began in the 1960s. Where open systems models use rational calculation and strategic choice as explanatory factors, Aldrich uses the nature and distribution of resources in organizations' environments to explain both diversity and change in populations of organizations [Aldrich, 1999]. His work corroborates claims by other organizational ecologists that populations of organizations have ecological properties that can be illuminated by analogical arguments about selection and shifts in population distributions of organizations that are products of selection.

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[18]In this case, however, the nature selection model is introduced indirectly through Campbell's [1969] selective retention model of sociocultural evolution.

McPherson's [1983] ecology of affiliation extends ecological theorizing in sociology by focusing attention on connections among individual, organizational, population, and community levels of analysis (see also [McPherson and Ranger-Moore, 1991; McPherson, Popielarz, and Drobnic, 1992]. By bringing insights from social network theory (e.g., the homophily principle) together with Lotka-Volterra competition models and niche theory, this multilevel approach offers a relational analysis of how "organizations compete ecologically in a niche space defined by the characteristics of the people in the community", with "internal network ties keeping members in the group and external ties pulling members out of the group" [McPherson, 2004, 232]. By considering processes of competition for individuals' time, energy, or attention and ways in which these individuals are connected in an explicitly multilevel model, network niche theory offers a powerful way of understanding how individual-level processes of recruitment and attrition affect organizational-level processes of stability, change, growth, and decline.

7 THE RETURN OF EVOLUTIONARY EXPLANATIONS

Ever since Durkheim, analogical reasoning has been the preferred way of bringing evolutionary biology into sociology. These analogies met with relatively little resistance, largely because they did not threaten the existence of sociology as an autonomous field.[19] The situation is very different in anthropology where the need for and value of asking and answering ultimate or "Why?" questions have always been much less controversial. Why? One factor that made the climate for reception of evolutionary explanations so different in anthropology is the prominent place of physical anthropology in the history of anthropology. A second factor is the long-standing participation of anthropologists in the study of nonhuman animal behavior and, in particular, the behavior of nonhuman primates. Human ethologists, biosocial anthropologists, and primatologists have used and continue to use evolutionary biological theory to develop and test predictions about specific behavioral traits of these animals [Altman, 1967; DeVore, 1965; Fox, 1975a; Kappeler and van Schaik, 2002; Sicotte, 1993]. Their programs of research helped to create and maintain a critical mass of anthropologists that pays close attention to developments and debates in evolutionary biology. A third factor that made anthropology more receptive to evolutionary explanations is the use, since the early 1960s, of the same theoretical ideas to examine the question of what primate behavior can tell us about human social evolution [de Waal, 2001; Fedigan, 1986; Fox, 1975b; Lee and DeVore, 1968; Rodman, 1999; Runciman, Maynard Smith, and Dunbar, 1997; Sperling, 1991] — not just by anthropologists who study nonhuman animals but also by those with no background in nonhuman animal studies [Daly and Wilson, 1999]. It is not surprising, then, that anthropologists have al-

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[19]Not all evolutionary sociologists today view Durkheim in this positive light. Some have identified his call for explaining social facts in terms of other social facts and the holistic conception of the individual-society relationship which underpins it as factors that work against using evolutionary explanations to bring insights from modern evolutionary biology into sociology.

ways been key players in the debate about human sociobiology. Nor is it surprising that, compared to their sociological counterparts, they have been more actively engaged in developing, testing, and defending human behavioral ecological, evolutionary psychological, and gene-culture coevolutionary explanations of human social behavior, social organization, social change, and cultural evolution.

Like human sociobiologists, human behavioral ecologists, evolutionary psychologists,[20] and some gene-culture co-evolutionary theorists ask and answer ultimate or "why?" questions about human social behavior. The exemplary sociologists and anthropologists I consider draw on the same theoretical insights from evolutionary biology — the need to study fitness consequences for individuals and not groups (understood as superorganisms), inclusive fitness, kin selection, reciprocal altruism, sexual selection, and parental investment theory — but in very different ways. Existing analyses of the connections among these evolutionary sociologists and anthropologists differ depending upon whether the analyst is a "lumper" or a "splitter." Lumpers treat human behavioral ecology, evolutionary psychology, and some gene-culture co-evolutionary theories as variants of sociobiology. This allows some participants in the debate about human sociobiology to use persistent misunderstandings of sociobiology to dismiss all attempts at constructing evolutionary explanations in sociology and anthropology [Benton, 1999; David, 2002; Horgan, 1995; Kenrick, 1995; Lloyd, 1999; Pigliucci and Kaplan, 2000; Rose and Rose, 2000; Rose, 1997; Sesardic, 2003]. Lumping allows other participants in this debate to defend human sociobiology by using theoretical arguments and empirical studies by human behavioral ecologists, evolutionary psychologists, and gene-culture co-evolutionary theorists to challenge these persistent misunderstandings [Alcock, 2001a; E. O. Wilson, 1998]. It allows still others [Freese, Li, and Wade, 2003; Machalek and Martin, 2004] to sidestep current controversies in evolutionary science that are the focus of splitters.

Splitters take seriously the extent to which persistent misunderstandings of sociobiology continue to mediate the reception of rival evolutionary explanations in sociology and anthropology.[21] It is not surprising, then, that they use a variety of mechanisms to distance human behavioral ecology, evolutionary psychology, and gene-culture co-evolutionary theories from sociobiology and from each other [Barash, 1982; Borgerhoff Mulder, *et al.*, 1997; Caporael, 2000; Caporael and Brewer, 1995; Cronk, 1991; Daly and Wilson, 1999 and responses; Flinn, 1997;

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[20]Evolutionary psychology is itself the site of vigorous debate about how to answer ultimate questions, with different evolutionary psychologists taking different positions on whether and how to bring in individual differences, culture, and development. My outline account will focus on the variant of evolutionary psychology that has had the greatest impact in sociology and anthropology — the so-called "inclusive fitness evolutionary psychology" [Caporael, 2001] exemplified by the work of [Barkow, Cosmides and Tooby, 1992; Buss, 1995; Cosmides and Tooby, 1987; Symons, 1979]. I will bring in other variants of evolutionary psychology into my concluding comments on unsettled issues in evolutionary theorizing in sociology and anthropology.

[21]Just how seriously they take the mediating role of these persistent misunderstandings is clear from the debate surrounding the 1997 decision to change the name of a major publication outlet for evolutionary social scientists from *Ethology and Sociobiology* to *Evolution and Human Behavior*.

Maryanski and Turner, 1992; Smith, Borgerhoff Multer, and Hill, 2001; Turke, 1990a and responses]. Nor is it surprising that these alternative ways of constructing evolutionary explanations continue to coexist with each other and with evolutionary theorizing that uses analogical reasoning to bring evolutionary biology into sociology and anthropology.

8   HUMAN BEHAVIORAL ECOLOGY

Like contemporary ecological theorizing in sociology, human behavioral ecology began in the mid-1970s with the application of theoretical arguments, models, and methods from evolutionary ecology to humans. Like their sociological counterparts, human behavioral ecologists also use these biological borrowings to refine and extend earlier attempts at ecological theorizing — in their case, those of cultural ecologists like Stewart, Lee, and Carneiro [Cronk, 1991; Winterhalder and Smith, 2000]. But, where ecological theorizing in sociology uses analogical reasoning to bring evolutionary biology into sociology, human behavioral ecologists offer evolutionary explanations of human social behavior. That links between human sociobiology and evolutionary anthropology were, and still are, strongest for human behavioral ecology is not surprising. Many of the theoretical arguments and empirical findings about behavior and fitness that E. O. Wilson brought together in his "New Synthesis" had been discovered by behavioral ecologists.

Both animal and human behavioral ecologists treat natural selection at the individual level as the driving force in evolution. Both treat behavior as one means by which individuals attempt to pass on copies of their genes in competition with genetically different rivals. Both develop and test hypotheses about the adaptive nature of behavioral traits without information on the genetic or developmental basis of these traits [Alcock 2001a; 2001b; Winterhalder and Smith, 2000]. Finally, both use the following evolutionary logic in their analysis of social behavior. Genetic variation causes behavioral differences in populations today as in the past. Natural selection then and now favors genes that produce phenotypes that enhance an individual's chances of passing copies of those genes to the gene pool of future generations. Therefore, individuals in ancestral and current environments should behave in ways that maximize their inclusive fitness. Which behavioral traits maximize inclusive fitness depends upon local ecological and social contexts. In the conditional strategies language of human behavioral ecology: In local context X, maximize fitness payoffs by doing x; in local context Y, switch to y, where x and y are alternative behavioral strategies available to individuals (e.g., a particular way of allocating parental effort between producing daughters and sons).

To explore empirically the fitness costs and benefits (adaptive trade-offs) for behavioral strategies in local ecological and social contexts, human behavioral ecologists apply game theoretic models and optimization models developed by animal behavioral ecologists [Axelrod and Hamilton, 1981; Krebs and Davies, 1993; Maynard Smith, 1982] to humans. Game theoretical models take account of ways in which the fitness payoff of one individual's behavioral strategies is affected by

what competitors are doing. Human behavioral ecologists have used them to study foraging decisions, cooperation, lineage exogamy, resource sharing and land tenure, and contests over resources (Cronk 1991) and to offer theoretical insights into the development of moral rules and cultural norms [Alexander, 1987; Cronk, 1991; Irons, 1991].

Human behavioral ecologists also develop and test optimization models, where the term "optimize" is used in the technical sense of "the maximum or minimum subject to specified constraints" (Krebs and McCleery 1984: 91-92). These models have three components: (1) assumptions about what is being maximized (ideally inclusive fitness but proxy currencies (e.g., calories, mating success) are often used as a maximization criterion), (2) assumptions about alternative courses of action in a behavioral activity (a behavioral strategy set), and (3) assumptions about individual-level and contextual constraints that shape the costs and benefits associated with each potential alternative (a constraint set) [Cronk, 1991; Krebs and McCleery, 1984; Winterhalder and Smith, 2000] From these assumptions human behavioral ecologists generate testable, falsifiable hypotheses about adaptive trade-offs that humans face within the activities of foraging, mate selection, and parental care and about adaptive trade-offs they they face between these behavioral activities [Anderson *et al.*, 1999; Gibson and Mace, 2005; Hewett, De Silvestri and Guglielmino, 2002; Marlowe, 1999; Smith and Bliege Bird, 2000].

Most studies of foraging are informed by optimal foraging theory [Hill, Kaplan, Hawkes, and Hurtado, 1987]. Some have applied the prey-choice model to foraging behavior and resource selection of humans. Starting from the assumption that energy capture is correlated with fitness, they ask "Which foraging strategies optimize net energy yield?" The maximization criterion is net acquisition rate of energy measured by costs (in time) and benefits (in calories), the behavior strategy set is diet combinations, and the constraint set includes the information available to foragers, their cognitive processing capacity, and the distribution, density and nutritional value of available resources [Borgerhoff Mulder, 1988; Winterhalder and Smith, 2000]. Tests in traditional societies have shown that individuals adopt different behavioral strategies under different sets of constraints such that their behavior maximizes measures of their inclusive fitness. Human behavioral ecologists have also used these models to improve our understanding of production in horticultural and pastoral societies [Patton, 2005].[22]

Studies of foraging behavior dominated human behavioral ecology in the 1970s. Since then studies of reproductive behavior have also played a key role in the growth of human behavioral ecology — in part because of their obvious link with reproductive success of individuals. Because the difference between "male" and "female" is genetic, males and females use different behavioral strategies in the same environment to enhance reproductive success and through it, the chances of getting copies of their genes into future generations.

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[22]For reviews of findings of earlier analyses of foraging decisions see [Borgerhoff Mulder, 1988; Cronk, 1991; Smith, 1983; 1992].

Male reproductive success is limited, in the first instance, by access to females with fertilizable eggs. Sexual selection favors the ability of males to compete with other males for access to these females, with more fertilizations providing greater opportunities for fitness payoffs. Human behavioral ecological studies of male-male competition show that individual differences in wealth, power, hunting skill, and violence are associated with differences in male reproductive success, largely through differential access to females [Betzig, 1988; Borgerhoff Mulder, 1988; Irons, 1980]. Intra-male competition affords females the opportunity to protect their much larger investment in offspring being choosy when selecting mates. Differential access to females thus also depends upon having traits that make males attractive to females.

Female reproductive success is limited by access to resources. Females should therefore choose males with good genes that will benefit their offspring (indicated by attractive faces and bodies) and males who are able and willing to transfer resources to them and their offspring, whether or not these offspring are genetically related to the male providing the resources. Borgerhoff Mulder [1990] used Orians' [1969] polygyny threshold model to predict that, under conditions of male control of resources and female-initiated mate choice, a female should choose a bachelor suitor if he has at least half the resources of an already-married suitor; otherwise she should become the second wife of the married suitor — even if this will involve sharing resources he controls with his first wife and her offspring. Paternal uncertainty makes resource transfers costly for males. Males should therefore select females who are likely to provide fitness payoffs to them through high reproductive quality (indicated by age, facial attractiveness) [Soler *et al.*, 2003] or by producing offspring who carry the male's genes. Both perceived mate fidelity and paternal resemblance to offspring explain variation in male parental investment.[23]

9   EVOLUTIONARY PSYCHOLOGY

Evolutionary psychologists distance themselves from human sociobiology and human behavioral ecology by arguing that neither pays sufficient attention to the mind's role in mediating the link between genes and behavioral outcomes. What makes evolutionary psychology "psychology" is precisely this focus on the human mind, understood as a series of domain-specific psychological mechanisms (i.e. information-processing procedures or decision rules) that constrain and enable human social behavior. What makes it "evolutionary" is its focus on the ultimate question of why humans have the psychological mechanisms we have. From the very beginning, evolutionary psychologists have underscored the importance of recognizing that the focus of evolutionary explanations of human social behavior must be psychological mechanisms and not behavioral outcomes. "To speak of natural selection as selecting for 'behaviors' is a conventional shorthand, but it is a

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[23]Mate choice is also contingent upon whether the relationship is short-term or long-term, the females' own physical condition, and the extent to which males take account of female's dominance status.

misleading usage. . . Natural selection cannot select for behaviors per se; it can only select from mechanisms that produce behavior" [Cosmides and Tooby, 1987, 281]. The goal of evolutionary psychology is to generate and test hypotheses about the evolution of these psychological mechanisms. "Research emphasis needs to shift away from the description and analysis of behavior — even in adaptive terms — to the discovery and characterization of psychological mechanisms as adaptations" [Tooby and Cosmides, 1989a, 32]; see also [Cosmides and Tooby, 1987; Tooby and Cosmides, 1989b; 1990; Symons, 1989]. Questions about how these psychological mechanisms produce or control social behavior must be answered in terms of proximate causes and thus not will not be discussed here — except to note that these proximate questions have received much less attention from evolutionary psychologists and their critics in sociology and anthropology than have their answers to the ultimate or "Why?" question.

According to evolutionary psychologists, psychological mechanisms evolved by natural selection to solve specific, recurrent adaptive problems in ancestral environments. Their use of the term "environment of evolutionary adaptedness" (EEA) to refer to these ancestral environments captures four core premises of evolutionary psychology. The first premise is that the key to understanding why humans have the psychological mechanisms we have is identifying the adaptive problems our ancestors faced. The psychological mechanisms that exist in current organisms exist because of their consequences for survival and reproduction in ancestral environments. The second premise is that, as adaptations, these psychological mechanisms increased inclusive fitness of individuals in ancestral environments. The third argument is that, as parts of a universal human nature that has not changed in the last 10,000 years, these psychological mechanisms continue to shape behavioral outcomes of humans today. The fourth premise is that psychological mechanisms that were adaptive in ancestral environments may not increase inclusive fitness in current environments — they may be adaptively neutral or maladaptive. Taken together, these four premises provide the framework for developing and testing evolutionary psychological hypotheses about psychological mechanisms as adaptations to the EEA, the behavioral outcomes of these psychological mechanisms, and the adaptive significance of these behaviors in ancestral and current environments.[24]

The first step in developing evolutionary psychological explanations of human behavior is identifying adaptive problems that our ancestors faced in the EEA. To avoid a persistent misunderstanding of the evolutionary psychological concept of the EEA, one thing must be clear from the outset. The EEA is not a particular place or particular period time — the African savanna where humans lived as hunter-gatherers in small bands of genetically-related individuals in the Pleistocene — as some critics of evolutionary psychology have argued. The EEA is "a statistical composite of the adaptation-relevant properties of the ancestral environments encountered by members of ancestral populations weighted by their

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[24]The output of an evolved psychological mechanism can also be physiological activity or information to other evolved psychological mechanisms.

frequency and their fitness consequences" [Tooby and Cosmides, 1990, 386-7].

Evolutionary psychologists typically distinguish between adaptive survival problems and adaptive reproductive problems and their ultimate causes — natural selection and sexual selection respectively [Buss, 1995; 1999; 2004]. Humans' preference for high calorie and fatty foods is an example of a psychological mechanism that evolved to solve an adaptive survival problem in the EEA — the need to secure sufficient food in an environment where food supplies were unpredictable. Individuals with this food preference engaged in behaviors which allowed them to survive at higher rates than did individuals without it, thus increasing the probability of passing copies of their genes to future generations. But survival alone does not ensure the transfer of genes to the next generation. Individuals may also be favored for psychological mechanisms that solve adaptive reproductive problems (e.g., mate value assessment, parental investment allocation). These adaptive problems and the psychological mechanisms that evolved to solve them are the focus of most evolutionary psychologists.

Like human behavioral ecologists, evolutionary psychologists offer evolutionary explanations of sex differences in how choosy individuals should be and in what they should be choosy about. But, because evolutionary psychologists treat humans as "adaptation executers" and not "fitness maximizers" [Smith *et al.*, 2001], for them, the key to understanding these differences is what went on in the EEA. To arrive at this understanding, they develop and test hypotheses about the different adaptive reproductive problems males and females faced in the past, the sex-specific psychological mechanisms that evolved to solve them, and the behavior generated by these mechanisms. In every case, males and females with these mechanisms (e.g., a preference for facial symmetry in mates (to assess mate value), sexual jealousy (to handle the threat of losing a mate), a female preference for resource-rich mates (to secure resources for her offspring), a male preference for female fidelity (to increase the probability of investing in his biological offspring), a preference for allocating parental investment differentially to male and female offspring (to handle different resource constraints)) had higher inclusive fitness in ancestral environments than their counterparts without them. Psychological mechanisms evolved because they conferred adaptive advantages in past environments.

Evolutionary psychologists reject the fitness-maximization assumption of human behavioral ecology when they turn to the question of why humans behave the way we do in current environments. For human behavioral ecologists what is at issue is still fitness maximization — individual variation, contemporaneous ecological and social constraints, and resulting adaptive payoffs. For human behavioral ecologists, humans, like other animals, are "fitness maximizers" because natural selection made them good at what they do. They are efficient foragers, make good mate choices, and differentially invest in sons and daughters because natural selection, acting on heritable differences between individuals in foraging strategies, male-male competition for mates, female choice of mates, and allocation of parental investment, favors behavioral strategies that maximize inclusive fitness.

This explanation is not an option for evolutionary psychologists who believe that there has been little, if any, genetic evolution in post-Pleistocence environments. For evolutionary psychologists, "[p]resent conditions and selection pressures are irrelevant to the present design of organisms and do not explain how or why organisms behave adaptively, when they do" [Tooby and Cosmides, 1990, 375]. Current behaviors are explained by adaptations to past environments. Evolved psychological mechanisms that exist in current organisms produce the same behaviors that they produced in ancestral organisms, whether or not these behaviors currently enhance inclusive fitness. Two corollaries follow from this view of humans as "adaptation executers". The first corollary is that, to the extent that current environments are evolutionarily novel, human social behaviors that reduce inclusive fitness are expected. The second corollary is that because current social behavior and the psychological mechanisms producing it are not products of the honing action of natural selection acting on variations between individuals in the present, fitness maximization is not the test of a Darwinian approach to human social behavior [Symons, 1979]. Anthropologists and sociologists thus should not attempt to answer ultimate questions by looking at the adaptive significance of current behavior; they should attempt to show how current behavior can be explained by adaptations to past life.

Some evolutionary anthropologists have attempted to do this by developing and testing evolutionary psychological hypotheses about facial attractiveness. If the facial features that modern humans find attractive evolved by sexual selection in ancestral environments to signal high quality mate status, then males and females should have preferences for facial features that signal youth and health — even if the link between facial features and mate status no longer holds. The same evolutionary logic has been used to study the impact of social status on mate choices. Women in modern industrial societies can acquire resources for themselves. But, if women's choice of mates is still largely determined by preferences that evolved in the EEA, then they should choose resource-rich men as mates in modern societies, regardless of their own social status. Men, by contrast, should pay little attention to the social status of women. Tests of these predictions by evolutionary psychologists both inside and outside anthropology support these predictions [Salter, 1996; Schützwohl and Koch, 2004; Schützwohl, 2005].

This way of answering ultimate questions has recently made its way into sociology, largely through the efforts of Satoshi Kanazawa. He has argued that evolutionary psychology can supply the general theory of values that rational choice theory needs [Kanazawa, 2001a; Horne, 2004], explain reproductive behavior in the contemporary United States [Kanazawa, 2003], and refine our understanding of social capital [Kanazawa, 2002; Savage and Kanazawa, 2004]. In making these claims about the value of evolutionary psychological explanations, Kanazawa did not confront a conceptual space that was already occupied by human behavioral ecology or any other empirical evolutionary sociology. The *Annual Review of Sociology* had published constructive critiques of sociobiology that highlighted the potential relevance of its core theoretical ideas for empirical questions of sociolog-

ical significance [Boorman and Levitt, 1980; Nielsen, 1994]. But few evolutionary sociologists had taken up their call to develop and test sociobiological hypotheses. One exception was the work of Jeremy Freese and colleagues. Their tests of the Trivers-Willard hypothesis about parental investment in sons and daughters [Freese and Powell, 1999; see also Freese and Powell, 2001; Kanazawa, 2001b] and Frank Sulloway's hypotheses about the effects of birth order on social attitudes [Freese, Powell and Steelman, 1999] had linked evolutionary sociology with human behavioral ecology.[25] But because the case for and against evolutionary explanations in sociology remained predominantly theoretical in focus, Kanazawa could avoid locating his evolutionary explanations in the context of the ongoing debates with human behavioral ecologists about this way of answering ultimate questions.

Human behavioral ecologists have responded to evolutionary psychology at three interrelated, yet distinct, levels. First and foremost, they have reiterated their case for the relevance of current fitness consequences in determining the adaptive significance of human social behavior and it corollary that humans tend to choose behavioral strategies that maximize their inclusive fitness. In doing so, they take care to point out that, with a few exceptions, neither side in the debate about the relevance of current fitness consequences adopts the extreme position that is often attributed to it: "current fitness consequences mean everything; current fitness consequences mean nothing" [Turke, 1990b, 449; see also Borgerhoff Mulder et al., 1997, Daly and Wilson, 1999; Jones, 2003a; Turke, 1990a]. Human behavioral ecologists acknowledge that humans today do engage in behaviors that reduce their inclusive fitness and that empirical support for models tested with data in natural settings has come largely from studies of traditional societies.[26] They also acknowledge that novel environments may decouple proximate goals (e.g., choosing a wealthy, high status mate if female; having many short-term sexual relationships if male) from current fitness consequences.

The second level of response questions the evolutionary psychological assumptions that current environments are evolutionarily novel, that humans lack the cognitive adaptations for behaviors that maximize fitness in novel environments even though these proximate mechanisms have produced behaviors with maladaptive effects since the EEA, and, therefore, that this decoupling is expected. One line of this questioning challenges the view that our psychological mechanisms have not evolved since the EEA [Irons, 1990; O'Gorman *et al.*, 2005; Smuts, 1991; Stressman and Dunbar, 1999; D. S. Wilson, 1994]. A second line argues that humans have the ability to behave adaptively in a wide range of environments (in

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[25]For a review of their more recent work and the work of Kanazawa, see [Freese, Li, and Wade, 2003].

[26]They openly acknowledge methodological issues surrounding measurement of inclusive fitness (e.g., use of proxies) in studies in natural settings but argue that research involving fictional scenarios, experiments, and questionnaires (the preferred methods of evolutionary psychologists) is "most instructive in conjunction with studies of real-world interactions" (O'Gorman, Wilson, and Miller 2005:383). For discussions of methodological differences between human behavioral ecology and evolutionary psychology see [Buss, 1995; 1999; Smith et al., 2001; Winterhalder and Smith, 2000].

part because of a flexible cognitive architecture) and, therefore, that the pursuit of these proximate goals in current environments has tended to maximize inclusive fitness. A third line focuses on the extent to which current environments are in fact evolutionarily novel. Some (e.g., [Alexander, 1979, 1986; Smith *et al.*, 2001, 131] suggest that "only the environmental details are novel, not the fundamental tradeoffs they present, nor the ability to recognize and appropriately react to those tradeoffs". Irons [1990; 1998] takes a different tack, introducing the concept of adaptively relevant environments to instantiate his claims that only some parts of our current environments are novel. If, as evolutionary psychologists claim, our evolved psychology is made up of domain-specific modules, then some behavioral strategies will have adaptive consequences in current environments.

At the third level of response, human behavioral ecologists join forces with other critics of evolutionary psychology to challenge specific assumptions and arguments of evolutionary psychology, including the importance of domain-general psychological mechanisms [Flinn, 1997; Jones, 2003b; MacDonald, 1991; Palmer, 1992; Shapiro and Epstein, 1998; Smith *et al.*, 2001],[27] the role of individual genetic differences [MacDonald, 1991; Plomin, 1990; Scarr, 1992; 1993; 1995], and the strength of the relationship between evolved psychological mechanisms and human social behavior [MacDonald, 1991; Shapiro and Epstein, 1998]. This level of response, like the first two, leads human behavioral ecologists to the same conclusion: the question of whether a behavioral strategy does or does not pay off in enhanced inclusive fitness in current environments is an empirical question that must be examined for each specific behavioral strategy in each specific local ecological and social context.

In assessing this claim and the evolutionary psychology it contests, some sociologists and anthropologists (e.g., [Alvard, 2003; Aunger, 2000; D. S. Wilson, 1994] have raised questions about the role of culture, pointing out that culture has received relatively little attention in human behavioral ecology and evolutionary psychology (but see [Alexander, 1979; Cronk, 1999; Irons, 1979; Jones, 2003a]). The question of how useful evolutionary biological theory is for understanding culture and its effects on human social behavior is the subject of vigorous debate in evolutionary sociology and anthropology, with different sides taking different positions on the issues of if, and if so, then how cultural evolution is Darwinian.

10   GENE-CULTURE COEVOLUTIONARY THEORIES

No one denies that culture matters for humans. And, other than adherents of extreme forms of what Cosmides and Tooby [1992] call "the Standard Social Science

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[27]The assumption of domain-specificity does not preclude the possibility of domain-general mechanisms but the verdict is not yet in on this possibility. Nor does it entail the assumption that these evolved psychological mechanisms operate independently of one another. Because modularity does not entail "information encapsulation", evolutionary psychologists are open to the possibility of "superordinate mechanisms" that function to regulate other mechanisms. This awaits future research, however.

Model," no one denies the genetic basis of human's capacity for culture. Where sociologists and anthropologists part company is when they confront questions of if, to what extent, and how our evolutionary history constrains the kinds of cultures and thus social structures we produce, reproduce and transform. Those who accept that culture is a superorganic process simply ignore biological constraints on culture. For gene-culture coevolutionary theorists *"[c]ulture is crucial for understanding human behavior.* People acquire beliefs and values from people around them, and you can't explain human behavior without taking this reality into account" [Richerson and Boyd, 2005, 3]. This does not mean a return to the cultural determinism, however.

One of the earliest and most contested gene-culture co-evolutionary theories was constructed by E. O. Wilson and physicist Charles Lumsden in *Genes, Mind, and Culture* (Lumsden and Wilson 1981a),[28] in part as a response to the charge that sociobiology had not taken proper account of either the human mind or the diversity of cultures (where culture includes mental constructs and behaviors transmitted from one generation to the next by social learning). After theorizing the linkage between genetic and cultural evolution, they developed abstract mathematical models (drawn from physics) that connect genes to individual mental development, individual mental development to culture, and culture to genetic evolution. At the core of their coevolutionary theory is the concept of epigenetic rule. Epigenetic rules are genetically determined, biological processes that direct the assembly of the mind, using information from ecological and cultural environments. "These rules comprise the restraints that the genes place on development (hence the expression 'epigenetic'), and they affect the probability of using one culturgen (the basic unit of inheritance in cultural evolution) rather than another" [1981a, 7]. For Lumsden and Wilson, then, humans are not *tablua rasa* organisms. Their "leash principle" makes their specification of the linkage between genetic evolution and cultural evolution "metaphorically more vivid: genetic natural selection operates in such a way as to keep culture on a leash", with the leash symbolizing "genetically prescribed tendencies to use culturgens bearing certain key features that contribute to genetic fitness" [1981a, 13]. Individuals with epigenetic rules that predispose them toward the selection of successful culturgens have greater survival and reproductive success than other individuals and are favored during the course of genetic evolution. "Thus, over many generations, the human population as a whole has moved toward a 'human nature' out of a vast number of possible, and has fashioned certain patterns of cultural diversity from an even greater number of patterns possible" [Lumsden and Wilson, 1981b, 6].

Genes, Mind, and Culture received considerable attention from evolutionary biologists, anthropologists, and sociologists, in part because Lumsden and Wilson used it as a vehicle to answer critics of sociobiology. Lewontin's [1981, 23] review presented this work as an attempt to save sociobiology, stressing that the "only

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[28]See also [Dawkins, 1977; 1982]. See [Segerstråle, 2000] for an historically contextualizing evaluation of Dawkin's theory that pays close attention to its connections with the coevolutionary theorizing of Lumsden and Wilson.

trouble is that each step of the model-building process is carefully designed to achieve that end. The authors have tried to cover their tracks by dusting their path with epsilons and deltas, but the plan is clear." Others took aim at Lumsden and Wilson's conclusion that genes keep culture on a leash. Many sociologists and anthropologists simply followed anthropologist Edmund Leach [1981] and dismissed this (and other) gene-culture coevolutionary theories on the grounds that culture explains human social behavior and "cultural differences have no relationship at all to genetic differences".

Sociologists and anthropologists who accepted that genes and culture are closely coupled responded to this line of attack in two very different ways. Some, including sociologist Joseph Lopreato [1990; 2002], defended the argument that genes bias culture so as to favor rules that maximize inclusive fitness. For Lopreato [1990, 208], the answer to the question of why human social behaviors evolve and persist is to be found in "the individual's tendency toward adaptive behavior" — understood as behavior that maximizes inclusive fitness.[29] Other sociologists (e.g., [Dietz, Burns, and Buttel, 1990] argued that because culture is relatively autonomous of genetic control, cultural evolution generally will not shape behavior and social organization in ways that maximize genetic fitness.

In anthropology, some coevolutionary theorists (e.g., [Durham, 1976; 1990]) sidestep this issue altogether by not applying evolutionary biology to the study of culture or even studying social behavior per se. Two of the most influential gene-culture coevolutionary theorists, Peter J. Richerson and Robert Boyd, keep the focus on human social behavior (Boyd and Richerson 1985).[30] But they argue that Lumsden and Wilson's genes-keep-culture-on-a-leash metaphor "only gets at half of the story" [Richerson and Boyd, 2005, 194]. What is the other half of the story?

Richerson and Boyd [2005] agree with Lumsden and Wilson that humans have an evolved psychology that shapes how we behave by shaping how we think, what we learn, and thus what cultural traditions exist, persist, and spread in human populations. But, for them, taking the causal role of our evolved psychology seriously is necessary but not sufficient for an understanding of human behavior that accords culture its proper role. For this evolutionary explanation in terms of evolved psychological mechanisms to be sufficient, the relationship between our evolved psychology and culture would have to be unidirectional, with the direction of influence running from evolved psychology to culture. This relationship is not unidirectional, however. At the same time that genetic elements of our evolved

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[29]In *Human Nature and Biocultural Evolution* Lopreato [1984] used a gene-culture analogy to defend this position. By 1990 he had favored an approach that combined gene-culture coevolutionary theorizing and the search for a theory of human nature. Since then he has rejected the former in favor of constructing a theory of human nature anchored in the fitness principle [Lopreato, 2002].

[30]For analyses of points of continuity and difference between their coevolutionary theory and those of Durham and Cavalli-Sforza and Feldman [1981] see [Durham, 1990]. For comparisons of these and other attempts at constructing coevolutionary theories see [Laland and Brown, 2002; Richerson and Boyd, 2005].

psychology shape culture, natural selection "acting on cultural variation shaped the environments in which our psychology evolved (*and is evolving*). The coevolutionary dynamic makes genes as susceptible to cultural influence as vice versa" [15], emphasis added.[31] An adequate understanding of human social behavior thus demands a theory which also accords culture its proper role by recognizing that "culture itself is subject to natural selection" [13], that natural selection operating on culture can favor very different behaviors than those favored by natural selection acting on genes, and, therefore, that not all of the processes shaping culture are products of our evolved psychology. Their population-based evolutionary theory does just this by setting out how and why we are largely what our genes *and* our culture make us.[32]

At the core this theory is an evolutionary analogy. Its source, like that of most evolutionary analogies in sociology and anthropology, is natural selection, but with a twist. For Richerson and Boyd, the use of natural selection as an analog for cultural evolution highlights the central importance of population thinking in evolutionary biology and "culture can't be understood without population thinking" [5]. Described by [Mayr, 1991] as key to the Darwinian revolution, population thinking challenged the nineteenth century view that all individuals of a species are essentially alike. What Darwin recognized, and what population thinking emphasizes, is the reality of genetic variation among individuals in a population and the importance of this heritable variation in their struggle for existence (i.e. as the raw material for natural selection). Two corollaries of population thinking are key to Boyd and Richerson's evolutionary analogy. The first corollary is a concept of biological species as a population of organisms that carry a variable pool of inherited information through time. The second corollary is an explanation of the properties of species in terms of the natural selection of individuals in specific environments, mutation, and genetic drift (i.e. random changes in the gene pool of a population). Traits that are carried by individuals that survive and reproduce persist and spread.

Applied to culture, population thinking leads to a conception of culture as "something that is acquired, stored, and transmitted by a population of *individuals*" [Richerson and Boyd, 2005, 81], where the "something" is information capable of affecting behavior whether consciously or not (i.e. such mental states as ideas, knowledge, beliefs, values, skills and attitudes), where "acquired" means acquired socially (primarily) by imitation, where "stored" means stored (primarily) in human brains, and where "transmitted by a population of *individuals*" means that cultural evolution, like genetic evolution, is a population-level consequence of (pri-

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[31]It is interesting here to note Lumsden and Wilson's description of their view of gene-culture coevolution on the first page of *Genes, Mind, and Culture* as an interaction "in which culture is generated and shaped by biological imperatives while biological traits are simultaneously altered by genetic evolution in response to cultural innovation".

[32]Sociologist W. G. Runciman [1998, 163] has used this kind of analogical reasoning to ask "whether a recognizably neo-Darwinian research programme could be designed for sociology on the basis of that social change is seen as a process analogous but not reducible to natural selection". See also [Chattoe, 2002].

marily) natural selection. As a populational phenomenon, culture is a variable pool of inherited information (i.e. cultural variants) carried by individuals. The same cultural evolutionary processes explain the production, reproduction, and transformation of all cultural traditions, adaptive or not.

Inertial processes (i.e. unbiased sampling and faithful copying of models) reproduce cultural traditions. Three sets of processes cause culture to change: random forces, decision-making forces, and natural selection. Random forces refer to cultural mutation and cultural drift. Decision-making forces take two forms: guided variation and biased transmission. Biased transmission occurs because people preferentially adopt some cultural variants rather than others. These preferences can be content-based (e.g., a conformist bias), frequency-based (e.g., a predisposition to adopt the most common cultural variants), and model-based (e.g., a homophily bias and a predisposition to imitate successful individuals and individuals who are accorded high levels of prestige). Natural selection refers to changes in the cultural composition of a population that are caused by the effects of holding one cultural variant rather than others. These processes of social transmission selectively retain and spread cultural variants which accumulate and eventually produce complex technologies and social organization. Complex cultural adaptations, like complex organic variations, evolve by the accumulation of small variations.

In setting out these cultural evolutionary processes, Richerson and Boyd [79] pay particular attention to the difference between biased transmission and natural selection. "Biased transmission depends on what is going on in the brains of imitators, but in most forms of natural selection, the fitness of different genes depends on their effect on survival and reproduction, independent of human desires, choices and preferences" [70]. Transposed onto cultural evolution, this difference means that these evolutionary cultural processes can and do produce very different evolutionary outcomes. For Richerson and Boyd, then, the critical question becomes that of relative importance. "If the psychological forces are much more important", then "all complex, adaptive behavior will ultimately be explained in terms of how natural selection shaped the innate aspects of psychology — and culture will have only a proximate role. However, if natural selection acting on cultural variation is important, then it is also an ultimate cause" [80]. Not surprisingly, they come down on the side of the process of natural selection of cultural variants, arguing that it is this force of cultural evolution that has powered the pace of human evolution over the last few hundred thousand years.

Why Richerson and Boyd [2005, 194] think that Lumsden and Wilson's metaphor only gets at half of the story is now clear. If genes keep culture on a leash, then culture "can wander a bit, but if it threatens to get out of hand, its genetic master can bring it to heel." But if, as Richerson and Boyd contend, "heritable cultural variation responds to its own evolutionary dynamic", then it can and often does lead "to the evolution of cultural variants that would not be favored by selection operating on genes. The resulting cultural environments can then affect the evolutionary dynamics of alternative genes. Culture is on a leash, all right, but the dog at the end of the leash is big, smart, and independent. On any given walk,

it is hard to tell who is leading who". This outline account of Richerson and
Boyd's gene-culture coevolutionary theory highlights where and how it contests
human sociobiological, evolutionary psychological, and human behavioral ecolog-
ical explanations of human social behavior. For Richerson and Boyd, modern
maladaptive behavior (e.g., low fertility and thus low inclusive fitness of modern
middle classes) cannot be explained by the "big mistake hypothesis" (from the
gene's point of view). It can, however, be explained as an unavoidable byproduct
of cumulative cultural adaptation. Culture "is adaptive because *populations* can
quickly evolve adaptations to environments for which *individuals* have no special-
purpose, domain-specific, evolved psychological machinery to guide them" [166].
The cost of this capacity for rapid adaptation to variable environments is sys-
tematic maladaptation. Cultural traits that reduce inclusive fitness can spread
precisely because they are transmitted differently from genes.

Richerson and Boyd also used *Not by Genes Alone: How Culture Transformed
Human Evolution* to respond to critics of gene-culture coevolutionary theorizing.
To those who would reject their theory on the grounds that culture cannot be di-
vided into discrete units of inheritance [Bloch, 2000; Bryant, 2004; Hallpike, 1986;
Leach, 1981; Lopreato, 2002] or that, even if such units of inheritance exist, they
do not replicate accurately the way genes do [Sperber, 2000], they reply that their
coevolutionary theory does not require that genetic evolution and cultural evolu-
tion be closely analogous. It only requires that cultural evolution has Darwinian
properties — that it exhibits the variation, competition, inheritance, and the ac-
cumulation of successful cultural modifications over time that they show culture
to exhibit. Genetic evolution and cultural evolution may have their own dynamics
but both are "evolutionary" in the sense that they conform to the logic of natural
selection.[33] To critics who would argue that models of the relationship between
genetic and cultural evolution rely on theory over data [Laland and Brown, 2002],
they point to their use of mathematical models, case studies, and experiments to
test specific predictions (e.g., about classes of maladaptions) derived from their
gene-culture coevolutionary theory. They stress that:

> The theory outlined here predicts what the empirical evidence tells us
> — culture is sometimes adaptive, sometimes maladaptive, and some-
> times neutral. It adds the nuance that what is maladaptive from the
> gene's-eye point of view may result from selection acting on cultural
> variation. Then, genes adapt secondarily to a world with culturally
> evolved institutions, so that genes come to support cultural adapta-
> tions. In a broader sense, human genes have also on average benefited
> from cultural adaptations even though natural selection directly on
> genes never favored large-scale cooperation! The soap opera messiness
> of human life accords well with the idea that multilevel selection has
> built conflict into our instincts and our institutions [244–45].[34]

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[33]For a very different way of arriving at the same conclusion see [Mesoudi, Whiten, and Laland,
2004].

[34]See also [Henrich and Boyd, 1998; 2002; Baum, Richerson, Efferson, and Paciotti, 2004].

This quotation captures how the "evolving pools of cultural and genetic information carried by the human population are partners in a swirling waltz" [192], while emphasizing that each pool responds to its own evolutionary dynamic. Its reference to "multilevel selection" reminds readers that they use a multilevel selection approach to cultural group selection that, following George Price, partitions total gene frequency change in a total population into with-group and between-group components.

As they present it, multilevel selection theory rests on two core arguments. The first argument is that, in both organic and cultural evolution, natural selection can operate simultaneously at different nested levels of the biological hierarchy (e.g., among genes within individuals, among individuals within groups, and among groups within species). The second argument is that, in both organic and cultural evolution, behavioral traits that evolve by group selection can contradict those that evolve by selection acting on lower levels, with the outcome depending on the relative amount of genetic variation within and between groups. For Richerson and Boyd [2005, 203], what is at issue is not whether kin selection, reciprocal altruism, and selfish-gene theory are or are not special cases of the new form of group selection as D. S. Wilson claims. "The real scientific question is what kinds of population structure can produce enough variation between groups so that selection at that level can have an important effect?" They contend that the answer to this question differs for organic and cultural evolution. Selection between large groups of unrelated individuals is not normally an important force in organic evolution because even very small levels of migration reduce the amount of between-group genetic variation to such a low level that group selection is not important. In cultural evolution, by contrast, group selection is an important force precisely because "rapid cultural adaptation led to a huge increase in the amount of behavioral variation among groups" [203-4], largely through the joint operation of moralistic punishment and conformist social learning. This heritable variation between groups + intergroup conflict = cultural group selection and the cultural group selection part of the gene-culture coevolutionary waltz played an important role in the evolution of human ultrasociality. For Richerson and Boyd [2005, 252], then, "nothing about culture makes sense except in the light of evolution".

11   USING EVOLUTIONARY HISTORY TO ANSWER "WHY?" QUESTIONS

All of the evolutionary explanations I have considered thus far use the same way of answering "Why?" questions about ultimate causes. They focus on the selective processes shaping the history of behavioral traits, developing hypotheses about the past or current fitness consequences these traits. Sociologists Alexandra R. Maryanski and Jonathan H. Turner [1992]; see also [Maryanski, 1994; 1998; 2005; Turner, 2001] use a very different approach. They focus on the historical pathways leading to current behavioral traits, answering questions about why humans have innate predispositions that make us more receptive to some patterns of social re-

lations than others in terms of our evolutionary history. By using the concept of phylogeny (i.e. the history of an evolutionary lineage) to anchor their evolutionary theorizing, they seek to supplement "standard social science, macrolevel functionalism" by providing a macrolevel alternative to the "microreductionism" of evolutionary biology, human sociobiology, and evolutionary psychology [Maryanski, 1997, 239].

The first step in arriving at this alternative is reconstructing the primordial hominoid (apes and humans) society and exploring how this ancestral social structure constrained the nature and evolution of hominid (human) social organization. To reconstruct the social organization of the Last Common Ancestor of the hominoid line, Maryanski and Turner map data from long-term field studies of apes onto an existing cladogram that describes the pattern of evolution in the hominoids from the last common hominoid ancestor to humans.[35] They conclude that, compared with most Old World monkeys, the hominoid lineage was predisposed toward strong individualism, autonomy, low sociality, and their social organizational counterparts of weak ties, low-density networks with restricted kin sectors, and mobility in space — in part because of selection against kinship alliances and group continuity over time and in part because of selection for high individuality. They also conclude that "genetic propensities that were adaptive for forest-living apes would be a liability for terrestrial, open-country apes" and, therefore, that "early *hominids* — if they were to survive in a predator-ridden savanna environments — would have to develop more cohesive and tightly knit social structures. The result was the development of human culture" [Maryanski and Turner, 1992, 32].

For Maryanski and Turner, recognizing that culture evolved to construct social ties is important for two very different reasons. The first reason is that it calls into question what they present as the widespread assumption in sociology that sociality is genetically based. For them, "the increasing complexity of hominid social organization was achieved through [individual] selection for a neurobiology that, in turn, could create cultural (rather than genetic) codes for male-female bonding, reciprocity, and other forms of sociality." Groups with these cultural characteristics were more likely to survive and reproduce themselves so that by the time *Homo sapiens sapiens* evolved, "social organization by cultural codes was clearly a more important adaptive process than selection at the genic level" and "selection was operating disproportionately on collectivities" [Maryanski and Turner, 1992, 73, 75]. If, as they contend, cultural codes and group structures are an independent basis for selection and evolution, then the evolution of human societies is primarily the result of cultural and social processes and must be explained in terms of these processes.

The second reason why recognizing that culture evolved to construct social ties is important is that it raises the question of whether culture can "contradict humans' primate legacy for rather weak and fluid social bonds" [Maryanski and

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[35]To see how their approach differs from phylogenetic approaches to cultural evolution, see [Mace and Holden, 2005].

Turner, 1992, 32] and the underlying genetic structures that these patterns of social organization instantiate. The bulk of their 1992 work, *The Social Cage: Human Nature and the Evolution of Society,* examines this question. The *"Human Nature and the Evolution of Society"* part of its title captures both their focus on human social organization and their position that an adequate understanding of changing patterns of human social organization across history cannot ignore the biological basis of human social organization. The *"Social Cage"* part identifies the metaphor they use to explore the ways in which culture constrains humans' primate legacy.

The first human society revolved around hunting and gathering. What distinguishes this stage of evolution is "the fluid and minimal nature of social structure and constraints" [85] and its corollaries, the autonomy of the family, the freedom of individuals, and the preservation of individuality. These distinguishing characteristics are consistent with Maryanski and Turner's view of humans' genetic nature. They thus conclude that at this stage of societal evolution, there was no contradiction between humans' primate legacy and their sociocultural constructions. "But after this period of human evolution, social and genetic structures have often stood in contradiction, and therefore, it is unwise to assume that sociocultural arrangements will reflect biological propensities or that coevolution necessarily involves compatibility between the genic and the cultural" [112] — an assumption that coevolutionary theorists like Richerson and Boyd do not make.

Contradictions between the genic and social levels begin with the transition from hunting and gathering to horticulture. In this evolutionary stage, kinship units were elaborated in ways that structured how economic, political, military, religious, and social activities were carried out. This social cage of kinship did facilitate the survival of the social groups who took up farming and the sedentary lifestyle it demanded. But, as Maryanski and Turner [90] are careful to point out, the cage of kinship also produced "a fundamental tension between humans' primate legacy and their sociocultural constructions". For them, "so-called instincts of humans for 'fighting', 'hierarchy', and 'territory' are not instincts at all, but adaptive sociocultural mechanisms for overcoming the contradictions between human biological propensities and the necessity of increased social organization" [111].

The social cage of kinship becomes less important with the transition to agrarianism and the separation of kinship from the state. During this stage of societal evolution, the new caging structures of power and stratification emerged as products of cultural and social processes. By making agrarian societies the most tyrannical and stratified societies in human histories, these structures, like the kinship structures they displaced, contradicted humans' biological propensities. But Maryanski and Turner contend that the scale and diversity of agrarian systems created opportunities for people to mitigate the constraining effects of these new cages or escape them altogether. The transition to industrial societies, especially capitalist industrial societies, offered ever-expanding opportunities to break out of the social cages of kinship, power, and stratification. In making this claim, Maryanski and Turner acknowledge that the cage of power did not disappear with

the industrial stage of evolution. But even though government gets bigger and more powerful as industrial societies democratize, people living in these societies have "a certain freedom, autonomy, and capacity for individuality not present in traditional agrarian societies" [140]. Industrial societies are thus more consistent with humans' primate legacy than either horticultural or agrarian societies. As they move toward a post-industrial profile, the structure and culture of human societies will become even more compatible with humans' biological nature. "[H]umans are an evolved ape; a primate that has little trouble with weak tie relations, loose and fluid communities, mobility, and fluctuating social structures" [162].

This way of bringing biology into sociology shifts the focus back to social organization and social change on a historical scale — the conventional concerns of evolutionary theorizing in sociology. It is no surprise, then, that Maryanski and Turner pay close attention to links with the works of Hawley, Lenski, and Spencer (as Turner interprets him). Nor is it surprising that they emphasize that their evolutionary theory is intended as an alternative to the "instinct sociology" of Lorenz, Ardrey, Morris, Tiger and Fox, and van den Berghe [1], sociobiology and sociobiology's "new offshoots in behavioral ecology and coevolution" [vi]. The take-home message from their evolutionary theorizing is that sociologists can take seriously the biological basis of human social organization without invoking the "unnecessary and unsubstantiated assumptions" of sociobiology (i.e. genes are the only unit of selection; individuals act so as to maximize their inclusive fitness, where maximize means maximum value — assumptions sociobiologists, human sociobiologists. and human behavioral ecologists do not make) or analogies to biological evolution which "may not hold up" [4]. What sociologists need is a way of assessing societal evolution in the light of the primate legacy humans possess. For them, their phylogenetic approach to social evolution supplies this.

12 UNSETTLED ISSUES IN EVOLUTIONARY THEORIZING IN SOCIOLOGY AND ANTHROPOLOGY

Four things about the controversy about evolutionary theorizing in sociology and anthropology are clear from this historical reconstruction. First, the controversy is less about bringing evolutionary biology into sociology and anthropology than it is about how to bring evolutionary biology in. Some sociologists and anthropologists question the value of evolutionary analogies but, for most, what is at issue is the need for and value of evolutionary explanations. Second, when it comes to constructing and defending evolutionary explanations, sociologists have fallen behind their anthropological counterparts. Three factors that help account for this lag emerged from this historical reconstruction: (1) different disciplinary histories made anthropology more receptive to evolutionary explanations, (2) sociological efforts at evolutionary theorizing are still predominantly theoretical in focus and thus still have too high a ratio of theory to data, and (3) prominent evolutionary sociologists continue to use evolutionary analogies to study social organization

and long-term macrolevel social change. Third, current evolutionary theorizing in sociology and anthropology is a heterogeneous activity and is likely to remain a heterogeneous activity. There are significant differences between those who use analogical reasoning to bring evolutionary biology into sociology and anthropology, and those who ask and answer ultimate questions about human social behavior, social organization, social change, and cultural evolution. There are also significant differences among those who use evolutionary analogies to illuminate these phenomena and among those who offer evolutionary explanations of them. Fourth, evolutionary theorizing in sociology and anthropology is, to borrow Mayr's [1991, 146] description of the evolutionary synthesis, "unfinished business." In different ways and to different degrees, unsettled issues about the value of evolutionary explanations, the role of development in evolution, multilevel selection, and the potential of evolutionary theory to unify the natural sciences and the social sciences continue to fuel the controversy about evolutionary theorizing in sociology and anthropology. My goal in these concluding remarks on this controversy is not to provide even outline accounts of these unsettled issues or competing attempts to resolve them. My goal is simply to identify them and suggest that attempts to resolve these unsettled issues should drive future efforts at evolutionary theorizing in sociology and anthropology.

12.1   The value of evolutionary explanations in sociology and anthropology

The vast majority of sociological and anthropological research asks and answers proximate questions and is thus not concerned with evolutionary explanations. Nor is there any reason why it should be. But the explanatory power of these proximate explanations does not mean that sociologists and anthropologists can ignore the potential value of evolutionary explanations of human social behavior, social organization, social change, and cultural evolution. Ever since ethologist Niko Tinbergen [1963] set out the four questions biologists can ask about animal behavior, evolutionary biologists have stressed the importance of recognizing the "fundamental difference between the immediate causes for something [proximate causes] and the evolutionary causes of that something [ultimate causes]" [Alcock, 2001a, 15].[36] They have also stressed that the study of ultimate causes and proximate causes must go hand in hand [Alcock, 2001a; 2001b; Krebs and Davies, 1997; Mayr, 1961; 1991; 1997]. Defenders of evolutionary explanations in sociology and anthropology extend both lines of argument to the study of human social behavior and its products.

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[36]Attempts to answer proximate questions about how individuals carry out behavioral activities in terms of the hormones that they possess (e.g., [Udry, 2000]) have generated considerable controversy in sociology, in part because some critics have confounded proximate and ultimate causes. See [Freese, Li, and Wade, 2002; Hammond, 2003] for sociological takes on the relevance of other proximate biological causes for understanding how the biological and the social interact.

Some argue for the value of evolutionary explanations on the grounds that they can fill major gaps in modern social theory. Recent developments in sociology, and especially in anthropology, have made evolutionary theorizing much more strongly empirical. This has allowed evolutionary sociologists and anthropologists to defend evolutionary explanations on the grounds that they generate novel predictions and explanations, account for apparently anomalous data by turning them into positive evidence, and make different testable predictions than can eliminate rival hypotheses (both evolutionary and nonevolutionary). They have also shown that, even where these predictions are not supported, developing and testing evolutionary explanations has moved research on topics of sociological and anthropological significance in new directions

For many sociologists and anthropologists, considerably more work will be required to demonstrate the value of evolutionary explanations. A major challenge is to determine empirically when, where, and how ultimate causes matter. But two things are clear from this historical reconstruction. First, understanding more fully the role of ultimate causes and the "Why?" questions they answer is essential for a complete and realistic understanding of human social behavior, social organization, social change, and cultural evolution. Second, improvements in this understanding will come about largely through resolving the ongoing controversies in evolutionary sociology and anthropology that were a focus of my outline accounts of human behavioral ecology, evolutionary psychology, and gene-culture co-evolutionary theories.

12.2   Evolution and development

Like evolutionary biology, evolutionary social science focuses on ultimate questions and answers. But this focus does not mean that evolutionary biologists today ignore how developmental processes constrain and control the adaptive behavior of individuals [Krebs and Davies, 1997; Maynard Smith, 1989]. In 1991 Mayr identified the role of development as one of the frontiers of evolutionary biology that was likely to see the greatest advances in the next ten or twenty years. Accumulating evidence suggests that he was right: "an understanding of evolutionary aspects of development (evo-devo) has advanced rapidly, by synthesizing "proximate and ultimate explanations of biology that have been pursued independently for too long" [Hall, 2000, 178, 177]. Critics of evolutionary social science charge that it has not kept up with these emerging developments in evolutionary biology and, as a result, pays too little attention to the need for a more complete understanding of the interrelationships between developmental systems and evolutionary theory. Linda R. Caporael [2001; Caporael and Brewer; 1995; 2000] has criticized the dominant (inclusive fitness) approach to evolutionary psychology in sociology and anthropology for ignoring the role of developmental systems. In its place she calls for a developmental-evolutionary approach that takes seriously complexities of development, culture, and environment — complexities that she claims "imbuing genes with fictional psychological characteristics (e.g., preferences, intentionality,

agency)" precludes [Caporael, 2001, 611]. Asking "What about development?" focuses attention squarely on an unresolved issue in evolutionary psychology that will take on increased importance in sociology and anthropology, if evolutionary psychology continues to make inroads in these social sciences.

Philosophers William C. Wimsatt and James R. Griesemer (2004:17) have also asked the question "What about development?". Their target, however, is current theorizing about cultural evolution. "Cultural evolution today is as devoid of a developmental perspective as population genetics was in 1970. This is a serious mistake. Development figures centrally in understanding the most important differences between biological and cultural inheritance, and consequently for the difference between biological and cultural evolution." Because culture is acquired developmentally, they contend that an adequate model of cultural evolution must pay close attention to the processes of individual human development.[37] To arrive at such a model, they turn to "one of the hottest research areas in both evolutionary biology and development [Wimsatt, 2001, 220], evolutionary developmental mechanisms,[38] and 'evo-devo' culture". How sociologists and anthropologists will respond to this call to "evo-devo" culture remains to be seen. But one thing is clear from this historical reconstruction: this response will be conditioned by earlier engagements of predictions about and explanations for cultural evolution.

12.3   Multilevel selection

One of the most enduring debates in evolutionary biology centers on the question of which entity or entities are selected in a process of natural selection.[39] It is not surprising, then, that this debate emerged as a central theme of my historical reconstruction of the controversy about evolutionary theorizing in sociology and anthropology. Nor is it surprising that for sociologists and anthropologists, the debate focuses squarely on questions of whether and under what conditions group selection is an important evolutionary force in human social and cultural evolution.

Group selection cannot be ruled out *a priori* as an explanation for the evolution of ecological adaptations or behavior. But, as my historical reconstruction showed, by the late 1960s evolutionary biologists had abandoned the for-the-benefit-of-the-group form of group selection of Emerson and Wynne-Edwards on the grounds

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[37]Lumsden and Wilson [1981a] argued that genetic and cultural evolution are linked by individual development (i.e. epigenesis) but Maynard Smith and Warren [1982, 625] suggest that while their "basic idea, that evolution somehow works through development, is so important and interesting", the "notion of the epigenesis as the gene-culture link turns out, in LW, to be rather loose and empty".

[38]Hall [2003, 493-94] describes evolutionary developmental biology (Evo-Devo) as a discipline that "is concerned, among other things, with discovering and understanding the role of changes in developmental mechanisms in the evolutionary origin of aspects of the phenotype. In a very real sense, Evo-Devo opens the black box between genotype and phenotype, or, more properly, phenotypes as multiple life history stages arise in many organisms form a single genotype".

[39]For recent overviews of the complexities of the debate about units and levels of selection, including links with developmental systems theory see [Okasha, 2003; Lloyd, 2005].

that, except under very stringent conditions (small group size, low rates of migration, high rates of extinction of entire groups), individual advantage would override group advantage. In most cases, ecological adaptations and behavior that benefit the group (in Emerson and Wynne-Edwards' sense) could be explained by selection acting on individuals [Lack, 1966; Maynard Smith, 1964; 1976; 1989; Wiens, 1966; Williams, 1966].

The success of the case against the for-the-benefit-of-the-group form of group selection did not end the debate about group selection, however. A series of mathematical models had been constructed that kept the group selection tradition alive in biology (cf. [Wright, 1945; Maynard Smith, 1964]). Then, in the 1970s, a new approach to group selection was independently developed five times [Charnov and Krebs, 1975; Cohen and Eshel, 1976; Matessi and Jayakar, 1976; Price, 1970; D. S. Wilson, 1975]; see also [D. S. Wilson, 1976; 1977]. These new models helped define a new form of group selection that argues that (1) natural selection can operate simultaneously at different levels of the biological hierarchy (e.g., genes, organisms, groups, demes), (2) a trait may be selectively disadvantageous at the individual level but selectively advantageous at the group level, and (3) that some features of human social groups evolve by increasing the fitness of groups relative to other groups, rather than by increasing the fitness of individuals relative to other individuals within the same group [Okasha, 2003; Sober and Wilson, 1998; D. S. Wilson 2002].

Two things should be, but are not always, made clear in social science discussions of new group selection models in which the targets of selection are subgroups of the population rather than individuals. First, trait-group and intrademic selection models are mathematically equivalent to inclusive fitness models [Boyd and Richerson, 2005; Grafen, 1984, Maynard Smith and Szathmary, 1995; Sober and Wilson, 1998]. These multilevel selection models "are generated from a fitness-accounting scheme that merely produces an alternative picture of the same selective processes described by the inclusive fitness models". The new group selection models can be translated readily into inclusive fitness models and vice versa" [Reeve, 2000, 65-6]. Second, the recent support for group selection, including trait-group or multilevel selection in evolutionary biology and anthropology is thus not a vindication of the for-the-benefit-of-the-group model of Emerson and Wynne-Edwards that evolutionary biologists reject [Alcock, 2001a; 2001b; Reeve, 2000].

My historical reconstruction documented the prevalence of conscious and unconscious group selectionism of the for-the-benefit-of-the-group form in evolutionary sociology long after it had been abandoned by evolutionary biologists. Evolutionary sociologists have paid relatively little attention to the ongoing debate in evolutionary biology about the unit of selection. One result is that they have introduced concepts and theories from evolutionary biology into sociology that have been rejected by evolutionary biologists. For those using analogical reasoning, this may not be problematic if the for-the-benefit-of-the-group form of group selection is the appropriate analogue for human societies. But developments in social theory

since the mid-1980s have shown that it is not. Most social theorists now reject the conception of societies as superorganisms in favor of a relationist conception of society that views individuals as mutually constituting and mutually constituted [Archer, 1982; Bourdieu, 1977; Giddens, 1984; Habermas, 1987; Haines, 1988; Pescosolido and Rubin, 2000; Tilly, 2002].

Abandoning the form of group selection that has dominated sociology may not mean abandoning group selection altogether, however. As Richerson and Boyd [2005, 207] point out, "there is no need for groups to be sharply bounded, individual-like entities. The only requirement is that there are persistent cultural differences between groups, and these differences must affect the groups' competitive ability" (but see [Palmer, Frederickson and Tilley, 1995]). Relationist conceptions of groups (including society) like those now found in sociology are consistent with the conception of group that underpins the new form of group selection in evolutionary biology [Haines, 1987] and gene-culture coevolutionary theorizing in anthropology.[40] Multilevel selection thus forms an important point of departure for developing and testing fully coevolutionary models of social evolution but only if evolutionary sociologists pay close attention to apparently competing answers to questions about the new form of group selection in evolutionary biology and evolutionary anthropology. If inclusive fitness theory and multilevel selection are mathematically equivalent, then do trait-group and intra-demic selection models involve a component of group selection or not [Maynard Smith, 1983; Okasha, 2003; Reeve, 2000; Sober and Wilson, 1998]? Do human groups sometimes qualify as adaptive units in their own right or not [Boehm, 1993; 1997; Caporael, 2001; Sober and Wilson, 1998; Sosis, 2003; Shavit, 2001; D. S. Wilson, 1997; 2002]? Is group selection important in the case of both human genetic and cultural evolution, or just cultural evolution, or neither [Boehm, 1997; Bowles and Gintis, 1998; 2003; Richerson and Boyd, 2005; Soltis, Boyd, and Richerson, 1995; D. S. Wilson, 1997; 2002]? Are the human evolutionary psychological explanations and human behavioral ecological explanations that dominate evolutionary sociology and anthropology grounded in outdated evolutionary biology or not [Caporael, 2001; Caporael and Brewer, 2000; D. S. Wilson, 1999; 2002]? Sustained efforts on the part of sociologists and anthropologists to resolve debates surrounding these questions will help close the gap between evolutionary sociology and evolutionary anthropology, thus moving evolutionary theorizing forward more rapidly in both.

12.4   *Evolutionary theory as unifying theory*

Ever since Spencer constructed his evolutionary explanations, the potential of evolutionary theory to unify the natural sciences and the social sciences has been a recurring theme in the controversy over evolutionary theorizing in sociology and

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[40]Evolutionary sociologist Stephen K. Sanderson [2002, 444] may have overshot the mark when he claimed that the "selection of cooperative social forms occurs at the level of the individual, not the group or society" — unless Sanderson wishes to treat the organisms in a particular (trait) group as part of an individual's selective environment.

anthropology. Two things should be clear from my historical reconstruction of this controversy. First, the verdict is not yet it on the role of evolutionary theory as unifying theory. Second, reaching a verdict will involve resolving unsettled issues in evolutionary theorizing in sociology and anthropology which, in turn, will require a fundamental change in the rules of engaging evolutionary explanations. There can be no doubt that Spencer believed that evolutionary theory could unify not just the social sciences but also the natural sciences and the social sciences. This is what motivated him to write his evolutionary synthesis of biology, psychology, sociology, and ethics and what drove him to finish this evolutionary synthesis despite failing health and financial difficulties. Spencer may turn out to be right but, if he does, then he was right for the wrong reason. Evolutionary explanations do not pose reductionist threats to sociology or anthropology. If we need one, then it will have to be a new evolutionary synthesis.

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HOLISM AND SUPERVENIENCE

Julie Zahle

1 INTRODUCTION

The individualism/holism debate has both an ontological and a methodological dimension. The ontological dispute revolves around two questions: first, do social wholes exist *sui generis*? Second, what is the constitution of social wholes? The methodological disagreement focuses mainly on the question of whether explanations should be provided in terms of individuals, their actions, and so on, or rather in terms of social wholes, their actions, and so on.

Individualists and holists alike have provided different answers to these questions. Accordingly, there are various forms of both ontological and methodological individualism and holism. In the first part of the article, I present a framework for distinguishing between different ontological and methodological positions and give a brief historical overview of the debate since the 1940s. More precisely, the development of the ontological debate is considered, as well as three important phases in the methodological debate: the demise of the dispute about deterministic holism in the 1950s; the birth of the debate on intertheoretic reduction in the 1960s, and the microfoundations debate as it came into prominence in the beginning of the 1980s. Theorists are still engaged in the discussion of intertheoretic reduction and in the microfoundations debate.

In the second part of the article, the debate on reduction is considered in more detail. Here, I explicate a strong argument, based on the notion of supervenience, that is currently being advanced against the possibility of reduction. I show that weakness in this argument leaves open whether reduction is feasible. This means that the outcomes of both the dispute on reduction and the microfoundations debate are still uncertain.

So where should the individualism/holism debate go from here? In order to answer this question, I suggest, the development of parallel discussions within the social sciences should be taken into account. A main trend within the social sciences is relationism. Philosophers might fruitfully clarify and further explore the many issues raised by relationist approaches.

2 THE ONTOLOGICAL DISAGREEMENT

The ontological debate concerns the *sui generis* existence of social wholes or entities such as schools, nations, prisons, societies, churches, etc. Ontological individ-

ualists deny that social wholes exist *sui generis*, whereas ontological holists defend this view.[1] That is, ontological individualists claim that schools, universities, and so on, do not exist in their own right, whereas ontological holists maintain that they do. But what does it mean to say that social wholes exist in their own right? There is no general consensus on this matter in the individualism/holism literature. Theorists may — and do — appeal to different criteria of *sui generis* existence, and they rarely comment on this fact. Here are three commonly used criteria:

> *The composition criterion*: Social wholes exist *sui generis* insofar as they are not merely constellations of individuals, that is, solely made up of individuals.

> *The causal criterion*: Social wholes exist *sui generis* insofar as they are causally efficacious.

> *The instantiation-independence criterion*: Social wholes exist *sui generis* insofar as they exist as some sort of Platonic ideas apart from, or over and above, their instantiations.

Since different criteria may be employed, either singly or in combination, theorists may not only disagree about the *sui generis* existence of social entities relative to a given criterion, they may also hold different views on what to count as criteria of *sui generis* existence. For example, one theorist may subscribe to the criterion of composition while the other endorses the causal criterion. As a consequence, they may pronounce themselves differently on the *sui generis* existence of social entities even though they agree that social wholes are composed of nothing but individuals and are causally efficacious. The theorist who uses the composition criterion will declare himself an ontological individualist on the grounds that social wholes are composed of nothing but individuals. The theorist who appeals to the causal criterion will state that he is an ontological holist since social wholes are causally efficacious. The fact that theorists do not always mean the same thing when they talk about the *sui generis* existence of social entities is a primary reason why the ontological debate may sometimes appear somewhat confused.[2]

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[1]Sometimes the disagreement is presented as one about the existence rather than *sui generis* existence of social wholes. If the debate is phrased in terms of the existence of social wholes, it might invite the misconception that ontological individualists deny that social wholes exist in the sense of individuals coming together to form units or wholes, or that they hold that social wholes are non-existent in the same way as Santa Claus or unicorns. In order not to invite any misconceptions of this kind, I prefer to characterize the dispute as revolving around the *sui generis* existence of social wholes.

[2]Discussions of *sui generis* existence mainly concentrate on social wholes. But the question is also raised in connection with social properties, that is, properties ascribed to social wholes as wholes. Examples of social properties that are ascribed to social entities include belligerence and cohesiveness. Occasionally, the question is also formulated with respect to social facts or structures. Both notions are used in numerous ways. For the present purposes, they may simply be considered as alternative ways of referring to social properties. Thus, a social fact like a high suicide rate and a social structure such as a matrilineal kinship system may both be considered

The ontological debate concerns not only the *sui generis* existence of social wholes, but also their constitution. The latter issue has mainly been addressed from the perspective of what it takes for individuals to constitute a social whole. One suggestion is that individuals constitute a social whole in virtue of sharing some property. For example, if a number of individuals are poor or unemployed, they may be grouped together as social wholes and referred to as "the poor" or "the unemployed." Historically, theorists have not been very concerned with social wholes of this type. Indeed, many theorists do not even consider them true social wholes. Instead, the most common proposal is that individuals form social wholes in virtue of being somehow interrelated. Theorists maintain that individuals are mainly related through their interactions, or they point to individuals' attitudes towards each other as what binds the individuals together into a whole. Needless to say, there are different ways in which to spell out both these proposals. Generally speaking, the constitution of social wholes has received much less attention than the question of whether social entities exist *sui generis*.

2.1 How Ontological Individualism Became Mainstream

In philosophy, ontological holism has mainly been associated with the views of Hegel, Comte, Marx, and their followers.[3] In the 1940s and 1950s, Hayek, Popper, and Watkins criticized their theories in a number of ways. In the course of doing so, they implicitly or explicitly rejected several ways in which social wholes may be said to exist *sui generis*.

First of all, they contended that social wholes are composed of nothing but individuals. They took this to imply that social entities are not sorts of organisms, sometimes equipped with a mind of their own. Further, Hayek offered an analysis of what it takes for individuals to constitute a social whole. He suggested that social wholes are formed by individuals who are interrelated primarily by their attitudes towards each other. Moreover, he held that since these attitudes are unobservable, social entities are unobservable: they only appear as wholes in light of theories about the way observable individuals are interrelated by their unobservable attitudes towards each other [Hayek, 1952, 5].

Secondly, Popper stressed that a nation, for example, does not exist, like a Platonic idea, over and above particular nations. He mainly supported this claim by arguing that the view that social wholes exist apart from their instantiations has unacceptable methodological consequences: it induces social scientists to focus on uncovering the underlying idea of, say, nationhood [Popper, 1964, 29ff]. Instead, Popper claimed, the task of social scientists is to construct and analyze theories

as properties which may be predicated of social wholes such as societies. Among the criteria mentioned above, the causal and the instantiation-independence criteria may be rephrased as criteria of *sui generis* existence with respect to social properties.

[3]Another main proponent of ontological (and methodological) holism is Durkheim. For theorists who mention his ontological holism, see for example Ginsberg [1956, 149ff], and Rosenberg [1988, 118ff].

about the way social wholes are formed by individuals being interrelated by their unobservable attitudes towards each other [Popper, 1964, 136].

Thirdly, Popper, and Watkins following him, strongly opposed the thesis that social wholes are causally efficacious in the sense of being superhuman forces equipped with causal powers that override the causal powers of individuals. This ontological claim may be seen as going together with the historicist view that societies go through stages of development according to deterministic laws of historical development, that is, laws which individuals cannot influence in any way.[4] Popper argued against the possibility of such laws. In order to qualify as a law, he stated, a universal hypothesis must be testable by its new instances. So-called laws of historical development fail to meet this requirement: the development of society is a unique evolutionary process and consequently no new instances of the "laws" may be observed [Popper, 1964, 109]. Popper considered several possible objections to this argument, one being that the development of society is not a unique process. In response, he admitted that certain kinds of events, like the rise of a tyranny, had recurred several times throughout the history of mankind. Still, he insisted, they never occurred as part of the same type of chain of events or evolutionary process. Any theories to the contrary should be seen as a result of the data being manipulated so as to confirm the very idea of history being repetitive [Popper, 1964, 111]. Thus, Popper concluded that there can be no deterministic laws of historical development. Consequently, insofar as the claim that there are deterministic laws of historical development stands and falls together with the view that social wholes have causal powers which override the causal powers of individuals, the latter view may be rejected, too.

In their discussions, Hayek and Popper did not clearly differentiate between the ontological and methodological dimension of the individualism/holism debate (and to this extent the above presentation of their views is somewhat anachronistic). In the 1950s, theorists increasingly began to draw this distinction and emphasize its importance. Watkins' writings are in line with this development. In a later article, he explicitly stated his ontological position as distinct from his methodological stance, while insisting that he had done so all along [Watkins, 1959a, 320]. As to the reaction to Hayek, Popper, and Watkins' ontological points, few, if any, theorists were prepared to defend the view that social wholes exist in their own right according to the above interpretations of the composition, causal, and/or instantiation-independence criterion.

Still, some theorists opposed the ontological individualist position by appealing to different criteria or to alternative understandings of the above criteria. One of the most influential statements of ontological holism was presented by Mandelbaum. He argued that social wholes are not identical with specific individuals having certain thoughts and performing given actions. This may be seen, he claimed, from the fact that it is impossible to translate terms which refer to social wholes into terms which refer to the thoughts and actions of individuals only. What it means to be, say, a bank cannot be captured solely in terms which re-

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[4]It should be emphasized that this is only one possible understanding of historicism.

fer to individual thought and action: "what it means to speak of 'a bank' will involve the use of concepts such as 'legal tender', and 'contract'. Further, what it means to speak of 'a contract' will involve reference to our legal system, and the legal system itself cannot be defined in terms of individual behavior" [1973a, 228]. Insofar as this is the case, Mandelbaum asserted, social wholes exist *sui generis*.[56]

Despite this and other attempts to defend ontological holism, however, it eventually became the minority position. The arguments advanced by Hayek, Popper, and Watkins, combined with further important clarifications of the individualist position, resulted in ontological individualism becoming mainstream.[7]

Today, most theorists, including methodological holists, declare that they are ontological individualists by reference to the composition criterion of *sui generis* existence: social entities do not exist in their own right insofar as they are composed of nothing but individuals. A notable exception to this trend is the position often referred to as social realism, and represented by Bhaskar, among others.[8] Bhaskar agrees that social wholes are made of individuals [Bhaskar, 2000, 30]. However, he does not make use of the composition criterion of *sui generis* existence. Instead, he insists on the existence of social wholes by appeal to the causal criterion: social wholes exist because they are causally efficacious in the sense of having causal powers which emerge from individuals, their actions, and so on.[9] In sum, Bhaskar defends the thesis of ontological holism by appeal to the causal criterion.

It is also worth noting that the constitution of social wholes has recently been taken up in the context of discussions of collective intentionality. The collective intentionality debate mainly focuses on how to understand the ascription of mental states and actions to social wholes. As part of this project, Gilbert has proposed an analysis of what conditions must be met in order for individuals to count as a social whole. She proposes that individuals constitute a social whole or social group if they form a plural subject of, say, a goal, belief, or principle of action [Gilbert, 1990, 10]. One way in which she explicates the idea of plural subjecthood is as a pool of wills dedicated as a unity to a goal, belief, principle of action or the like. Gilbert mainly illustrates her proposal in relation to small social groups such as

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[5]Mandelbaum phrases his argument more broadly as the claim that societal facts are distinct from psychological facts, i.e. facts concerning the actions and thoughts of specific individuals. Societal facts refer to "any facts concerning the forms of organization present in a society" [Mandelbaum, 1973a, 223]. Thus, "societal facts" are not limited to facts about the existence of social wholes. Another aspect of Mandelbaum's argument, as presented by Kincaid, is discussed below in section 5.

[6]Gellner is another proponent of ontological holism, writing around the same time as Mandelbaum; see his [1968]. For an important later defense of ontological holism, see e.g. [Ruben, 1985].

[7]For specifications of the individualist position, see for example [Jarvie, 1972] and [Wisdom, 1970a].

[8]Other proponents of social realism include Archer (see, e.g., her [1995] and [2003]); and Sayer (see, e.g., his [1992] and [2000]). See also part II "Critical naturalism and social science" in Archer et al. eds. [1998] for an overview and discussion of different social realist positions.

[9]The notion of emergence will be discussed further in section 4.

one formed by two people going for a walk together. She makes it clear, however, that her analysis is meant to apply to larger groups or social wholes as well.[10]

In general, the individualism/holism debate has focused much less on issues of ontology, i.e. the question of the *sui generis* existence of social wholes and of their constitution, than on the methodological aspect of the dispute.

3   THE METHODOLOGICAL DISAGREEMENT

The methodological disagreement revolves around the question of what constitutes the proper focus of causal explanations. Roughly speaking, methodological individualists favor explanations centered around individuals, their actions, properties, etc. whereas methodological holists prefer explanations focused on social wholes, their actions, properties, and so on. However, in order to capture the complexities of the debate missed by this simple presentation, it is necessary to distinguish between different versions of both methodological positions. Since theorists may — and do — provide different specifications of both methodological individualism and holism, the methodological dispute may assume several forms.

First of all, it is possible to distinguish between stronger and weaker kinds of methodological individualism and holism. The three most common positions within the philosophical debate are:

> *Strong individualism*: Finished or rock-bottom explanations in the social sciences must always be strict individualist explanations, i.e. refer to individuals, their actions, properties, etc., only.

> *Moderate individualism*: Explanations in the social sciences must always be non-strict individualist explanations, i.e. include reference to individuals, their actions, properties, etc.

> *Temperate holism*: Explanations in the social sciences must sometimes be strict holist explanations, i.e. refer to social wholes as wholes, their actions, properties, etc. only.

Strong and moderate individualism differ in that the former requires that explanations must refer solely to individuals, their actions, properties, etc., whereas the latter only states that they must involve reference to individuals, their actions, etc. Further, by focusing on finished explanations only, the strong position leaves it open that there may be unfinished explanations which refer to social wholes as wholes, their actions, etc. By contrast, the moderate individualist and temperate holist do not make use of a distinction between finished and unfinished explanations and advance their methodological claim in connection with explanations as

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[10]Gilbert provides a more detailed exposition of her position in her [1992]. An alternative analysis of the constitution of social wholes, advanced within the context of the collective intentionality debate, is Tuomela's (see, e.g., his [1995] and [2002]). Other important contributions to the collective intentionality debate in general include [Searle, 1990; 1995] and [Bratman, 1993; 1999]. For a nice overview of the debate, see [Tollefsen, 2004].

such. Finally, the position of the temperate holist conflicts with both individualist positions by holding that strict holist explanations are sometimes appropriate.[11]

Second, methodological individualists and holists alike may also differ by relying on narrower or broader specifications of "individuals, their actions, properties, etc." and of "social wholes as wholes, their actions, properties, etc." For instance, some theorists subscribe to a narrow conception by holding that a methodological individualist is not allowed to refer to relations between individuals. Others endorse a broader notion by allowing reference to such relations.[12]

Third, it is also possible to differentiate between both individualist and holist methodologies according to their views of what an explanation is. For example, a methodological individualist or holist may endorse the view that explanations do not involve laws. Or, he may subscribe to the deductive-nomological (D-N) model of explanation, i.e. the view that explanations assume the form of deductive arguments: the conclusion specifies the event to be explained, whereas the premises contain a statement of a law plus a description of initial conditions.

In sum, methodological individualist and holist positions may vary along at least these three dimensions: they may be stronger or weaker; they may endorse broader and narrower conceptions of what is involved in referring to individuals, social wholes as wholes, etc., and they may rely on different notions of explanation. Consequently, the methodological dispute may assume different forms: methodological individualists and holists may not only disagree about the extent to which explanations must refer to individuals, their actions, etc., or to social wholes as wholes, their actions, etc. Their conflict may be further complicated by the fact that they subscribe to divergent views of what it means to refer to individuals, social wholes as wholes, etc. and of what an explanation is.

In what follows, three important phases in the history of the methodological debate will be considered: the demise of the dispute about deterministic holism in the 1950s; the birth of the discussion of intertheoretic reductionism in the 1960s; and the microfoundations debate as it came into prominence in the beginning of the 1980s. In this connection, the framework just developed will be drawn upon when characterizing key positions and their differences.

Before looking at these three debates, however, two comments are in order. It should be noticed that methodological holism is sometimes associated with the use of functionalist explanations. A functional explanation asserts that the persistence of a social entity or social property may be explained by appeal to its beneficial

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[11] As already indicated, there are other possible versions of both methodological positions. For example, a methodological individualist may claim that explanations (rather than just finished explanations) in the social sciences must always be strict individualist explanations. Similarly, a methodological holist may insist that finished explanations in the social sciences must always be strict holist explanations. And so on. In general, the strength of both positions depends on such factors as: whether the methodological prescription applies to all explanations or to finished ones only; whether holist or individualist explanations must be advanced always or only sometimes; and whether the explanation must be a strict or non-strict explanation.

[12] For discussion of different ways of characterizing methodological individualism in this regard, see for example [Brown, 1973, 137ff; James, 1984, chapter II], and [Lukes, 1973; 1994].

consequences for the larger social whole of which it is a part. For example, a religious institution may be said to persist because it gives rise to social cohesion. Functional explanations have been widely applied within the social sciences. Yet their use has also involved a lot of controversy. Today, most theorists think that functional explanations should be seen as a species of causal explanations. Functional explanations will not be further discussed in the present context.[13]

Further, it should be pointed out that methodological individualism is sometimes associated with the endorsement of liberal values, whereas methodological holism is linked with the espousal of collectivist political ideals. For example, Hayek and Popper drew this connection. They saw liberalism as based upon a commitment to methodological individualism, and collectivism as being underpinned by a commitment to methodological holism. Within the contemporary individualism/holism literature, methodological individualism and holism are typically discussed without reference to political values of any form. Moreover, as argued by Bird, there is no necessary linkage between the choice of methodology and a certain political orientation [Bird, 1999, 55ff]. For both these reasons, the political dimension of the debate will not be considered in the following.[14]

3.1   *The Demise of the Discussion of Deterministic Holism*

The methodological debate was particularly lively in the 1950s. Watkins was one of its most prominent participants.[15] In several papers, he presented a rather detailed exposition of strong methodological individualism that was to become highly influential. Moreover, his position is representative of the individualist opposition to what may be called deterministic holism. Watkins' writings were widely discussed and opposed by, among others, Goldstein.[16] Goldstein's position illustrates why the debate about deterministic holism lost momentum: he refused to defend deterministic holism. Instead, he argued that an alternative temperate methodological holism is preferable to Watkins' strong methodological individualism.

Watkins identified deterministic holism with the use of explanations that appeal to deterministic holist laws. Holist laws are laws about social wholes, their

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[13]See Kincaid (this volume) for a discussion of functional explanations.

[14]See for example, [Hayek, 1949; 1952] and [Popper, 1973a; 1973b]. For a discussion of Hayek's and Popper's views in this respect and of the lack of necessary connection between the choice of methodology and political ideals, see for example [Bird, 1999k, 7ff and 55ff].

[15]Watkins' main publications on the topic are: [Watkins, 1958; 1959a; 1959b; 1973a; 1973b; 1973c; 1976]. A small sample of the books and articles discussing Watkins' position include: [Brodbeck, 1973a; Danto, 1973; Gellner, 1968; Mandelbaum, 1973b; Nagel, 1968; Perry, 1983]; and [Scott, 1973]. A discussion of the development of Watkins' methodological individualism may be found in [Udehn, 2001]. Watkins sees his position as an elaboration of Hayek and Popper's theories. Popper's views have also been developed by Agassi [1973; 1975], Jarvie [1964; 1972] and Wisdom [1970a; 1970b]. For discussions of Hayek's position, see, e.g., [Brodbeck, 1973a; Rudner, 1973; Scott, 1973; Udehn, 2001].

[16]Goldstein's criticisms appear in his [1959; 1973a; 1973b]. An exchange developed between him and Watkins. It should be read in the following order: Watkins [1973a/1952; 1973b/1955]; Goldstein [1973a/1956]; Watkins [1958]; Goldstein [1973b/1958]; Watkins [1959a]; Goldstein [1959]; Watkins [1959b].

properties, and so on. They are deterministic in the sense that individuals cannot prevent or influence what happens in accordance with them. Watkins took it that the deterministic holist usually relies on deterministic holist laws of historical development. Thus, it appears that the typical deterministic holist is a historicist: "This makes holism well-nigh equivalent to historicism, to the idea that a society is impelled along a predetermined route by historical laws which cannot be resisted but which can be discerned by the sociologist" [Watkins, 1973c, 168].

Watkins opposed this position by denying what he took to be its underlying ontological assumption that social wholes have causal powers that override the causal powers of individuals. Without further argument, he simply contended that there are no such superhuman forces at work in history. Individuals "(together with their material resources and environment) are the only causal factors in history" [Watkins, 1959a, 320]. Hence, "no social tendency exists which could not be altered *if* the individuals concerned both wanted to alter it and possessed the appropriate information" [Watkins, 1973c, 169–170], italics in original).[17] Insofar as this is the case, there are no deterministic holist laws and hence it makes no sense to advocate the use of explanations which appeal to such laws. The deterministic holist position collapses. Since Watkins held that the only alternative to deterministic holism is methodological individualism, the latter position wins by default.

Watkins was a strong methodological individualist: he presented methodological individualism as the view that all finished or rock-bottom explanations must be strict individualist explanations. He followed Hayek and Popper in adopting a broad notion of what counts as a strict individualist explanation in the sense of allowing the methodological individualist to refer to relations between individuals.[18] Hence, he stated that a methodological individualist may refer not only to individuals' dispositions, situations, beliefs, physical resources and environment, but also to their interrelations [Watkins, 1973c, 168].[19] Finally, like many of his contemporaries, Watkins subscribed to the D-N model of explanation. He specified that the conclusion of an acceptable explanation consists in a description of an event, whereas the premises contain a statement of individuals' dispositions plus a description of their situation or understanding of their situation. As an example, he offered an explanation of Emperor Constantine's decision to give Pope Silvester extensive temporal rights in Italy. This event, he suggested, may be deduced from,

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[17]Several theorists consider the defense of the autonomy of individuals vis-à-vis their society as a main, if not the main, motivation behind the individualist's opposition to holism. See for example [Dawe, 1970; Fay, 1996; James, 1984; Pettit, 1996].

[18]In numerous places, both Popper and Hayek make it clear that they allow the individualist to refer to relations between individuals. See for example [Popper, 1964, 136] and [1973b, 324 — note 11]). Likewise, see [Hayek, 1949, 6] and his analysis of social wholes as interrelated individuals in his [1952].

[19]Also, Watkins stressed that strict individualist explanations may refer either to particular individuals, their dispositions and so on, or to anonymous individuals characterized in terms of their typical dispositions, typical beliefs etc. Goldstein discusses Watkins' distinction to this effect in numerous places. Ryan draws a somewhat similar distinction between actual and typical individuals [1970, 176ff]. For discussions of Ryan's distinction, see Hylland and Bridgstock [1974] and Bridgstock and Hylland [1978].

among other things, a specification of the Emperor's disposition to subordinate all rival powers to himself, together with a description of the Emperor's acknowledgment that Christianity could be tamed as an official religion of the Empire [Watkins, 1973c, 178]. In sum, Watkins was a strong individualist; he subscribed to a broad notion of what it means to refer to individuals, their actions, etc.; and he endorsed the D-N model of explanation.

Goldstein raised a number of objections to Watkins' position. One of Goldstein's main complaints was that Watkins assumed that methodological holism must be based on the ontological claim that there are superhuman forces at work in history. Goldstein agreed that this ontological position is untenable. But he pointed out that a methodological holist need not hold such a strong view. That is, a methodological holist may perfectly well advocate the use of strict holist explanations that do not appeal to deterministic holist laws. Consequently, methodological holism as such cannot be rejected on the grounds that it relies on the dubious assumption that there are superhuman forces. As Goldstein puts it: "If non-individualistic social science does not commit untoward ontological sins, the methodological individualists are required to find better grounds for its rejection" [Goldstein, 1973b, 281]. Thus, it appears that Watkins' argument is incomplete: it fails to show that strong methodological individualism should be favored over other forms of methodological holism.

Goldstein continued by arguing that there are situations in which strict holist explanations that do not rely on deterministic laws are preferable to strict individualist ones. To illustrate this point, he considered the explanation of the prosperity of the Huguenots in 17th century France by appeal to their disposition to return a large proportion of their profits to their businesses. As it stands, Goldstein claimed, this explanation needs to be supplemented by an account of the social context in which this disposition was exercised; otherwise it is unclear why, say, the Huguenots developed and acted on the disposition to plough money back into their businesses [Goldstein, 1973b, 284]. An account to this effect might include a description of the kind of business enterprises that flourished at the time; of the kind of business enterprises in which Huguenots were typically involved; and of the extent to which the governments of the European countries were prone to interfere in economic affairs. Goldstein's key point was that this characterization of the social context does not include any mention of individuals, and what is more, "no need is felt for the psychological characteristics of anybody" [Goldstein, 1973b, 284]. As such, this specification of a social context exemplifies a situation in which there is no reason to prefer an account that focuses on individuals, their actions, and so on. Thus, Goldstein concluded that Watkins' strong methodological individualism should be rejected in favor of a temperate methodological holism that does not appeal to deterministic holist laws.

The exchange between Watkins and Goldstein may be seen as marking a shift in the methodological dispute: Watkins took it that the methodological individualist must defend his position against the deterministic holist. Goldstein made it clear that this was no longer necessary; the "new" opponent was the methodological

holist who does not appeal to deterministic holist laws. For some reason, Watkins never replied to this new challenge as presented by Goldstein. Other theorists have done so, however. The debate since then has transpired between methodological individualism and a methodological holism which does not rely on deterministic holist laws. The next two phases in the history of the methodological debate both illustrate this development.

3.2   The Birth of the Discussion of Intertheoretic Reduction

In the 1950s the question of reduction was being widely discussed within the philosophy of science. Different accounts of reduction were suggested. Among these, Nagel's model of intertheoretic reduction became extremely influential, particularly after the publication of his *The Structure of Science* in 1961. The book further spurred on the general debate about reduction.[20]

During the same period, theorists engaged in the individualism/holism debate also became increasingly concerned with reduction. In this context too, Nagel's model of intertheoretic reduction came to occupy a dominant position. Gradually, theorists began mainly to refer to his account when discussing reduction. Moreover, many suggested that the disagreement between methodological individualists and holists should be formulated as a dispute about the possibility of reduction according to Nagel's model. In this manner, the reductive version of the dispute was born: the reductive individualist maintains that holist laws may and should be reduced to individualist laws in accordance with Nagel's model of intertheoretic reduction. The anti-reductive holist denies the possibility and/or desirability of intertheoretic reduction.[21]

The proposal to rephrase the methodological debate as a dispute about reduction is acceptable to a theorist who grants the following two assumptions: that Nagel's model of reduction should be endorsed, and that the holist uses strict holist explanations which appeal to holist laws, i.e. laws about social wholes, their actions, etc., while the individualist uses strict individualist explanations which appeal to individualist laws, i.e. laws about individuals, their actions, etc. From the perspective of this theorist, the methodological dispute may be decided by

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[20]See also Nagel [1949], and his [1979] where he responds to criticisms. Alternative accounts of reduction have been suggested by for example [Oppenheim and Putnam, 1958; Feyerabend, 1962; Suppes, 1967; Wimsatt, 1974]. Critical elaborations of Nagel's model have been advanced by for example [Schaffner, 1967; 1993] and [Sklar, 1967]. For an overview of the development of the debate on reduction, see [Bickle, 2000] and [Wimsatt, 1979]. For a systematic overview of different approaches to reduction, see [Sakar, 1992].

[21]Nagel himself considers the reduction of holist to individualist theories in his [1961]. Theorists who refer to Nagel's account include [Bhargava, 1992; Brown, 1973; Danto, 1973; James, 1984; Kincaid, 1994; 1996; 1997; Lessnoff, 1974; Little, 1991; Martin, 1972; Macdonald and Pettit, 1981; Nozick, 1977; Sawyer, 2002; 2003]. It is noteworthy that theorists who present Nagel's account in more detail propose rather different interpretations of his requirements of connectability. Among social scientists, Homans explicitly refers to Nagel when he states that he is in favor of reduction understood as deduction [Homans, 1967]. A few theorists refer to [Bergmann, 1957] rather than Nagel when they discuss reduction. They include [Brodbeck, 1973b; Hummell and Opp, 1968; Addis, 1975].

determining whether holist laws are reducible to individualist ones. If holist laws are thus reducible, they may be substituted by individualist laws. This means that all explanations appealing to holist laws may be replaced by explanations involving individualist laws. Thus, the actual reduction of holist to individualist laws will amount to a vindication of strong individualism by showing that it is possible to use strict individualist explanations alone. Conversely, the actual failure to carry the reduction through in connection with all laws will demonstrate that strict holist explanations must sometimes be used (i.e. in cases where the holist laws are irreducible). This would establish the correctness of temperate holism. The point is now that a theorist may equally well reject one or both of the above assumptions. Insofar as he does so, he will also resist the suggestion to formulate the methodological disagreement as a dispute about reduction. From the viewpoint of this theorist, the outcome of the methodological dispute will not hinge upon the possibility of intertheoretic reduction.[22]

Both in the general literature on reduction and in the individualism/holism literature, there are different interpretations of Nagel's account. For present purposes, the following widely-used interpretation will be adopted. A theory may be considered a collection of statements typically in the form of laws. To reduce one theory to another, two conditions must be met:

1. The condition of connectability: The predicates/terms/type descriptions (T2s) that only figure in the to-be-reduced theory must be linked, on a one-to-one basis, via bridge laws or reductive definitions to the predicates/terms/type descriptions (T1s) of the reducing theory. The bridge laws may be stated in the form of bi-conditionals: $T1 \leftrightarrow T2$. The bridge laws express that the linked predicates are nomologically co-extensive, that is, have the same reference by law.[23,24]

2. The conditions of derivability: The to-be-reduced theory must be deduced from, and in this sense explained by, the reducing theory plus the bridge laws.

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[22]The theorist who does not accept Nagel's model of reduction will obviously oppose the reformulation of the methodological debate as a debate about the possibility of reduction in accordance with Nagel's model. But so will a theorist who denies that the holist's explanations appeal to holist laws and the individualist's explanations to individualist laws. From the perspective of this theorist, the reduction of holist to individualist laws will have no bearing upon the possibility of dispensing with strict holist explanations.

[23]It should be noticed that there are different interpretations of bridge laws. Some claim that they should be interpreted as one-way conditionals. See, e.g., [Richardson, 1979; James, 1984, chapter I; Danto, 1973]. The majority view is that bridge laws are bi-conditionals that link terms on a one-to-one basis. Some theorists take these bi-conditionals to express identity relations; others adopt the weaker relation of nomic co-extensiveness. Here, I shall adopt the weaker notion. But it should be emphasized that this choice does not make any difference to the following discussion.

[24]For a discussion of the reasons why bi-conditionals may not be disjunctive, i.e. assume the following form $T2 \leftrightarrow T1_1 \lor T1_2 \lor T1_3$, see [Fodor, 1995; 1997; Kim, 1992; Owens, 1990; Seager, 1991].

Now, in order to apply Nagel's model of reduction to the individualism/holism debate, holist theories must be distinguished from individualist theories. Accordingly, holist theories may be characterized as containing holist predicates which refer to social wholes as wholes, their actions, properties etc. alone. These holist predicates do not occur in individualist theories. The latter make use of individualist type descriptions, referring only to individuals, their actions, properties, etc. The reductive individualist must start by providing reductive definitions of the holist terms in the holist laws to-be-reduced. For instance, if the holist predicate "school" occurs in the holist law, then "school" must be reductively defined by showing that it is co-extensive with a single individualist type description. Once this is done, the reductive individualist must show that the holist laws may be deduced from the reductive definitions plus laws about individuals. Both these conditions must be met in order to reduce holist to individualist theories. More precisely, the reductive individualist must provide enough examples of cases in which both requirements are fulfilled to make it reasonable to expect that they may always be met. The anti-reductive holist's task is much easier: all he needs to show is that at least one of the conditions may not be met. To this end he may simply provide a single example of a situation in which either the condition of connectability or derivability cannot be met, or even better, point to feature(s) of, say, holist terms that show why intertheoretic reduction is likely to be impossible in general.

The reductive individualist's insistence on the possibility and desirability of intertheoretic reduction is sometimes associated with the reductionist program, also called the unity of science view.[25] The program states that the world is hierarchically organized: the elementary physical particles are at the bottom of this hierarchy. These particles form wholes in the form of atoms, which in turn constitute molecules, and so on. Social wholes are typically placed at the top of this hierarchy; they are the most complex wholes. The different sciences may be categorized according to the kind of wholes they focus on. At one end of the spectrum is physics, which is concerned with elementary physical particles. Its laws are the most fundamental and general. At the other end are the social sciences concerned with advancing the least general laws, i.e. laws about social wholes. The reductionist program requires that laws about the more complex objects be reduced to laws about their constituent parts until the basic laws of the elementary particles are finally reached. If a reduction along these lines were carried out, it would show how all the laws of the different sciences are interconnected or unified by ultimately being reducible to physics. Proponents of the unity of science view think that science should have as a goal the realization of the program of reduction. The claim that laws about social wholes should be reduced to laws about individuals is completely in line with this program. Thus, some theorists argue that reductive individualism should be seen as the application of the reductionist program to the social sciences [Kincaid, 1997, 1; Little, 1991, 191].

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[25]The classic statement of this view is Oppenheim and Putnam [1958]. For discussions and criticisms of the view, see [Causey, 1977; Kim, 1997; 2002; Poland, 1994].

The discussion of the reduction of holist laws to individualist ones has mainly focused on the possibility rather than desirability of reduction. Moreover, anti-reductive holists have primarily tried to show that reduction is highly unlikely or simply not feasible because of the difficulties related to meeting the requirement of connectability. The debate has continued on and off since its birth in the 1960s. It has mainly been promoted and kept alive by theorists who are highly sceptical about the possibility of reduction. The most recent development within the debate is no exception to this trend: a powerful argument, based on the notion of supervenience, is currently being advanced against reductive individualism. This argument will be critically examined after the presentation of the microfoundations debate.

3.3   When the Microfoundations Debate Gained Prominence

Within philosophy, the microfoundations debate became prevalent around the beginning of the 1980s when theorists like Roemer, Elster, and Little defended the micro-individualist demand for microfoundations in connection with Marxist social theorizing. Their writings gave rise to a lot of discussion of this requirement, not only as set forth within a Marxist context, but also as a general thesis about proper social theorizing.[26]

The general microfoundations debate concerns whether holist causal claims must be supplemented by accounts of underlying individual-level mechanisms. The micro-individualist maintains that such supplementary accounts must always be provided; the macro-holist denies this. Holist causal claims are ones in which both the cause and the effect are described in holist terms. An example of a holist causal statement is "the high rate of unemployment caused the war to break out." The macro-holist holds that this claim is fine just as it stands. By contrast, the micro-individualist maintains that it must be specified how the high rate of unemployment caused individuals to adopt certain beliefs, act in certain ways, etc. that in turn led to the outbreak of the war.[27]

There are different ways of spelling out the micro-individualist requirement. According to one suggestion, an account of individual-level mechanisms consists in a description of the chain of events, at the level of individuals, that link the cause and effect described in holist terms. More precisely, the account must specify

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[26]Roemer's writings in support of micro-individualism include his [1981; 1982; 1986]. Elster advances his micro-individualist position in numerous places. See for example, his [1982; 1983; 1985; 1986; 1989a; 1989b; 1998]. With respect to Little, see, e.g., his [1986; 1991; 1998]. For critical discussions of Elster's micro-individualism both in general and as applied to Marxist theorizing, see, e.g., [Taylor, 1986; Wood, 1986; Meikle, 1986; Slaughter, 1986], all in "Symposium on Elster's 'Making sense of Marx"' [1986]. See also [Weldes, 1989; Sensatt, 1988; Kincaid, 1996, 179ff; 1997, 25ff]. Critical discussions of Little's position may be found in [Roth, 1995; Steel, 2004].

[27]Micro-individualists mainly present and advance their demand for microfoundations in connection with holist causal claims. They often add, however, that the requirement applies to holist functional claims too. For a discussion of the micro-individualist requirement in connection with functional claims, see Kincaid (this volume).

the laws or law-like regularities that govern the transitions between these events [Little, 1991, 15]. Thus stated, the micro-individualist requirement leaves it open how these law-like regularities at the level of individuals should be phrased. Many micro-individualists base their formulations on rational choice models. Thus, they state regularities in the form of how rational individuals would choose to act in light of their beliefs about their situation, their goals, and so on. Micro-individualists hold different views as to how precise the accounts of individual-level mechanisms must be. Some micro-individualists hold that exact specifications of the underlying mechanisms must be provided; others argue that it is sufficient to have a rough idea of how this would go.

As already indicated, Elster and Little are two main proponents of micro-individualism. In slightly different forms, they have both advanced what have become two standard arguments in support of this position. The first argument appeals to a specific conception of explanations: the mechanism view. According to this view, to "explain is to provide a *mechanism*, to open up the black box and show the nuts and bolts, the cogs and wheels" [Elster, 1985, 5]. That is, causal claims on their own do not count as explanations; to qualify as such they must be accompanied by an account of the underlying causal mechanisms. The micro-individualist requirement, Elster argues, may be justified by appeal to this conception of an explanation: holist causal claims are in need of microfoundations because only thus supplemented do they count as explanations.[28]

The second argument points to the role of mechanisms in connection with the confirmation of holist causal claims. Little states that simply pointing to a correlation between two events described in holist terms does not establish that they are related as cause and effect. The reason is that the existence of a correlation is compatible with it being spurious. The only way, or at least the best way, to exclude this possibility, Little maintains, is to consider the underlying individual-level mechanisms. If no possible mechanism linking the two events described in holist terms may be suggested, then the correlation may be considered spurious [Little, 1991, 25]. Moreover, by having at least a rough theory about the individual-level mechanism, "we also have a theoretical basis for judging that the correlation is genuinely causal rather than spurious" [Little, 1991, 178]. So, Little concludes

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[28]Apart from Elster and Little, other proponents of the mechanism view in one form or another include: [Merton, 1968; Boudon, 1990; 1998; Coleman, 1986; 1987; 1990; Stinchcombe, 1991; Hedström and Swedberg, 1998]. Hedström and Swedberg's [1998] as well as Mayntz [2004] provide a nice overview of the debate. Hedström and Swedberg's essay is the introduction to a collection of essays on the topic. Mayntz's article is only one of several articles in a special issue on the mechanism view in the journal *Philosophy of the Social Sciences*. It should be emphasized that proponents of the mechanism view of explanation need not be micro-individualists. They may hold that holist causal claims may equally well be supplemented by accounts of causal mechanisms located at the level of, say, smaller social wholes rather than individuals. For example, the mechanisms leading from a bad economy to war may be specified in terms of events at the level of smaller social wholes such as firms. Thus, contrary to what Elster implies, the mechanism view can only be used to justify the micro-individualist requirement insofar as some independent reason is given for holding that the causal mechanisms must be located at the level of individuals.

that the micro-individualist requirement is supported by the fact that pointing to individual-level mechanisms is the best, if not the only, way to confirm holist causal claims.

On this basis, the micro-individualist may not only be characterized as subscribing to a particular view of explanation, viz. the mechanism view. Also, he may be classified as a moderate methodological individualist: he insists that all explanations must include reference to individuals, their actions, etc. Finally, the micro-individualist operates with a broad conception of what it means to refer to individuals, their actions, etc. by allowing reference to relations between individuals [Elster, 1985, 6].[29]

The micro-individualist position has not gone unchallenged. In fact, both of the above arguments have been disputed by Kincaid. First of all, he criticizes the mechanism view of explanation. He claims that we advance causal claims all the time, both in scientific and everyday contexts, and that we regard these claims as genuinely explanatory even though the underlying mechanisms are ignored [Kincaid, 1997, 28]. For example, the fact that a flying ball hit the window is typically considered a good explanation of why it broke even in the absence of an account of the window-breaking mechanism. For this reason, Kincaid asserts, the mechanism view should be rejected, and hence it can no longer serve as justification for the micro-individualist requirement: "reference to individuals is not necessary for social explanation" [Kincaid, 1997, 47]. Secondly, Kincaid contends that the identification of underlying mechanisms is not the only way to rule out spurious correlations. Another option is to control for all alternative possible causes of the effect described in holist terms [Kincaid, 1996, 180]. Since this method is available, Kincaid argues, it follows that there is no basis for holding that the best or only way to exclude the possibility of spurious correlation is to point to underlying mechanisms. Thus, he concludes that the micro-individualist requirement cannot be justified by pointing to the role of individual-level accounts in connection with the confirmation of holist causal claims.

A micro-individualist may try to counter Kincaid's criticisms by challenging Kincaid's assumption that an explanation is what we call an explanation in everyday and scientific contexts. Similarly, he might question whether it is really possible to check for *all* possible alternative causes. On a more general note, too, the debate does not end here. Several other arguments have been advanced both in support of, and against, the micro-individualist position. The microfoundations

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[29]Notice too that the micro-individualist is not a reductive individualist. The micro-individualist does not state that holist descriptions, such as those figuring in holist causal claims, must be reductively defined in individualist terms. Nor does the micro-individualist think that holist laws must be deduced from individualist ones. It is true that Elster, for example, sometimes describes his micro-individualism as a form of reductionism [Elster, 1985, 5]. But this should not be taken to mean reductionism as Nagel conceives it. Elster considers micro-individualism to be a form of reductionism simply because it requires that events described in holist terms may and should be accounted for in terms of individuals, their actions, and so on (see, e.g., [Elster, 1985, 5; 1989b, 195]).

debate is still an ongoing dispute within philosophy of social science and at this point there is no clear consensus as to the outcome of the debate.

4   HOLISM AND SUPERVENIENCE

The most important development within the current debate on reduction is the introduction of the notion of supervenience and the advancement of the argument from multiple realization.

In the 1980s, the notion of supervenience began to appear within the individualism/holism debate.[30,31] There are several ways to characterize this notion. For present purposes, a rough characterization, adapted to its use within the individualism/holism debate, will do. Social entities, their properties, actions, etc. may be said to supervene upon individuals, their actions, and so on, insofar as: (1) there can be no difference at the level of social wholes, their properties, actions, etc., unless there is also a difference at the level of individuals, their properties, actions, and so on; (2) individuals, their actions, etc. fix or determine what kinds of social wholes, properties, etc. are instantiated. Thus stated, the notion of supervenience refers to an ontological dependency relation. Supervenience may also be specified as a descriptive dependency relation. Holist predicates supervening upon individualist type descriptions imply that: (1) if the holist predicate "applies in one case but not in another, there must be some associated individualistic predicates that apply in the first case but not in the second, or vice versa" [Macdonald and Pettit, 1981, 119]; and (2) the application of the holist predicate is fixed or guaranteed by the application of some associated individualist predicates. Most theorists take the notion of supervenience to express both an ontological and descriptive dependency relation. Hence, they formulate their points in one way or the other (and I will do the same). Moreover, they maintain that *all* social entities, holist properties, actions, and so on supervene upon individuals, their actions etc., and that *all* holist predicates supervene upon type descriptions of individuals, their actions, and so on. Finally, notice that two other dependency relations are also currently in use: the notions of realization and emergence. They may be taken to express either a similar or a different, typically stronger, dependency relation. Within the philosophic individualism/holism debate, most theorists use supervenience and realization interchangeably: they talk about schools, say, being either supervenient upon, or alternatively, realized by, individuals, their actions, and so on. The notion of emergence is less commonly used. Henceforth, I will follow the trend and refer to supervenience and realization only.[32]

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[30]The notion of supervenience first gained currency within philosophy of mind around the beginning of the 1970s. For discussions of the history of the notion and different ways in which the notion may be specified, see, e.g., [Horgan, 1993; Kim, 1984; Teller, 1984].

[31]Within philosophy of social science, theorists who refer to the notion of supervenience include: [Bhargava, 1992; Bohman, 1993; Currie, 1984; Kincaid, 1994; 1996; 1997; Little, 1991; Mcdonald and Pettit, 1981; Mellor, 1982; Pettit, 1996; Ruben, 1985; Sawyer, 2002; 2003; Levine et al, 1987]. I draw on their accounts in the following characterization of supervenience.

[32]For a discussion of the relationship between the notions of supervenience, realization, and

The notion of supervenience has not only received attention as a convenient way to conceptualize the dependency of "the social upon the individual." Its introduction into the debate should also, and mainly, be seen as related to the fact that it paves the way for the application of the argument from multiple realization. The argument is currently considered to be *the* argument against intertheoretic reduction. It has mainly been developed within philosophy of mind, where it is used to argue that theories containing psychological predicates cannot be reduced to theories stated in purely physical terms.[33] The structure of the argument is easy to generalize. Thus, it is perhaps not surprising that several theorists have proposed that the argument may also be used to show that theories containing holist predicates cannot be reduced to theories making use of individualist type descriptions only. In this manner, it is claimed, the argument vindicates the anti-reductive holist position.[34]

The line of reasoning supporting this conclusion proceeds by first noting the fact that holist terms supervening upon individualist ones ensure that a holist type description of a particular event may always be replaced by a corresponding individualist predicate. It does not guarantee, however, that the application of the same holist term to *several* particular events may necessarily be substituted by a *single* individualist type description. Or, differently put, the specification of supervenience is compatible with multiple realization, that is, with a holist term being supervenient upon multiple different individualist type descriptions. Next, it is asserted that multiple realization is in fact highly likely to occur. This claim is typically supported by pointing to social predicates that are taken as being obviously supervenient upon multiple different individualist type descriptions. For instance, Kincaid lists several holist predicates such as "revolution", "bureaucracy", and "peer group" and remarks that "any number of different relations between individuals, individual psychological states, beliefs, etc. could realize the referent of these terms" [Kincaid, 1994, 500]. In a similar vein, Sawyer states that "'being a church' could be realized in disjunctive [and hence multiple] ways in different cultures and social groups" [Sawyer, 2002, 550].

Some theorists go further. They provide a general reason in support of multiple realization in the form of the claim that social whole predicates, i.e. holist predicates that refer to social wholes as wholes, are functionally defined. There are two ways in which to spell out this idea. Little suggests that social whole terms are *functional system definable*: they may be defined by describing how the individu-

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emergence, see [Kim, 1997]. For discussions of the notion of emergence, see, e.g., [Beckermann, et al., 1992; Crane, 2001].

[33]The argument is typically credited to Putnam and Fodor. See, e.g., [Putnam, 1975] and [Fodor, 1965; 1968; 1995]. For an overview of the development and discussion of the argument from multiple realization, see [Bickle, 1998]. Among others, Sober observes that the argument is currently *the* argument against reductionism: "If there is now a received view as to why reductionism is wrong, it is the multiple realizability argument" [Sober, 199, 542].

[34]The argument from multiple realization against reductionism within the social sciences has been advanced by [Currie, 1984; Fodor, 1994; Kincaid, 1994; 1996; 1997; Levine et al., 1987; Little, 1991; Ruben, 1985; Sawyer, 2002; 2003]. In the following, I am relying on their presentations of the argument.

als who constitute a given type of social whole are interconnected by their causal or functional roles vis-à-vis each other. For example, "bureaucracy" may be specified as "a hierarchical social organization in which office holders perform tasks in accord with plans established by a centralized decisionmaker" [Little, 1991, 194]. Moreover, he claims, individuals who realize a given type of social whole may be motivated in different ways to perform their functional roles. They may be induced to perform their tasks by sticks over carrots, carrots over sticks, and so on. As a result, a social whole term is also likely to supervene upon multiple type descriptions of individuals' motives.

Differently from Little, Kincaid proposes that social whole predicates are *functional state definable*: "If necessary and sufficient conditions can be given for these social predicates, it will generally be by means of their function vis-à-vis other social institutions and events — much as psychological states might be defined in terms of their functional role in a cognitive system" [Kincaid, 1997, 33-34]. That is, a social whole predicate may be defined by describing the causal or functional role of the corresponding social entity vis-à-vis other social wholes. Moreover, Kincaid points out, a given type of social entity may be realized by constellations of individuals whose internal organization differ. This means that a social whole predicate is likely to supervene upon multiple type descriptions of internally organized constellations of individuals.

Regardless of how the claim of multiple realization is supported, the upshot is the same. Intertheoretic reduction requires that holist terms be linked up with individualist ones on a one-to-one basis. The argument from multiple realization shows that many holist predicates will each have to be linked up with multiple individualist type descriptions. Consequently, the condition of connectability cannot be met. Reductive individualism fails.

In this fashion, the introduction of the notion of supervenience may be argued to have as a result the settlement of the dispute about reduction in favor of anti-reductive holism.

5   A DISCUSSION OF THE ARGUMENT FROM MULTIPLE REALIZATION

It is clear that the argument from multiple realization is a very strong argument against the possibility of intertheoretic reduction. Nonetheless, I think that it is possible for a reductive individualist to challenge the argument. The reductive individualist may argue that once no unreasonable descriptive constraints are imposed upon his position, it is no longer obvious that he will be unable to come up with reductive definitions. For this reason, the possibility of intertheoretic reduction should not be considered highly unlikely but rather an open question.[35]

In order to establish the point, it is useful to specify two kinds of descriptive constraints that may have a bearing upon the possibility of providing reductive

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[35]The following discussion draws upon my [2003]. Some of the points made in connection with both Little's and Kincaid's position are spelled out in more detail there.

definitions. First of all, the prospect of reductive definitions may depend upon what kinds of descriptions are available to the reductive individualist. The fewer the kinds of descriptions the reductive individualist is allowed to use, the more difficult it will be to provide reductive definitions. Or to put it the other way round, the less limited the available descriptive resources, the easier it will be for the reductive individualist to come up with a reductive definition. Secondly, whether reductive definitions are feasible (given the descriptions available to the reductive individualist) may depend on their descriptive focus. For example, the definition of a social whole predicate might either, say, focus on the interactions among the individuals who constitute the social whole, or on the interactions between these individuals and individuals who are not part of the social whole. It may be that a reductive definition may only be offered if the former, but not the latter, descriptive focus is adopted. Once this is clarified, it appears that there are two situations in which a reductive individualist may reject the claim that holist predicates are highly likely to supervene upon multiple individualist type descriptions. The argument from multiple realization may be discarded if it relies on an unreasonably narrow conception of the kinds of available descriptions. Further, it may be dismissed as inconclusive if it presupposes that the reductive individualist adopts one descriptive focus when others are available. In this case, the argument only shows that relative to this one descriptive focus, reductive definitions are highly unlikely. In both situations, the reductive individualist should follow up this point by showing, by way of example, that once the unreasonable descriptive constraints are lifted, it is no longer highly unlikely that reductive definitions may be provided.

The effectiveness of this individualist strategy to handle claims of multiple realization may be illustrated in connection with Little and Kincaid's arguments. As outlined above, Little maintains that social whole terms are functional system definable, that is, they may be specified in terms of how individuals who constitute a given type of social whole are interconnected by their causal or functional roles vis-à-vis each other. The anti-reductive holist advances these functional system definitions. The reductive individualist is said to describe individuals' motives. Consequently, he is highly likely to run into the problem of multiple realization: individuals may have different types of motives to perform their roles.

In response to the argument, the reductive individualist should question the crucial presupposition of the argument that he is not allowed to describe individuals' roles vis-à-vis each other. Little does not really justify this assumption except by stipulating that the reductive individualist is not allowed to describe individuals' interrelations and, by implication, their roles vis-à-vis each other. But is this reasonable? At least since Hayek, Popper, and Watkins, individualists have defined their position as allowing reference to relations between individuals. From this perspective, Little's stipulation seems unacceptable: the ban on any reference to relatins between individuals reflects a long-abandoned narrow conception of what kinds of descriptions are available to the individualist. Hence, the reductive individualist should reject it. Insofar as he does so, there is no longer any

obvious reason why he should abstain from describing individuals and their inter-relations in the form of the roles they perform vis-à-vis each other. This means that the reductive individualist may advance functional system definitions as his own reductive definitions. For example, he may appropriate Little's definition of a bureaucracy and state that "bureaucracy" applies if and only if the definition "constellation of individuals who are hierarchically organized so that officeholders perform tasks in accord with plans established by a centralized decision maker" applies. In a similar vein, he may propose that "prison" may be reductively defined as "a constellation of individuals such that some are prisoners, while the others are prison guards and prison personnel more generally" and so on. In sum, Little's argument from multiple realization fails: once a broader and more reasonable conception of the kinds of descriptions available to the reductive individualist is adopted, it no longer appears to be highly unlikely that reductive definitions may be advanced.

This conclusion, however, may be a bit too quick. The above examples of functional system definitions make use of role predicates such as officeholder, prison guard and prisoner to describe individuals' roles vis-à-vis each other. There is a widely cited argument against the individualist's use of such role predicates. The argument is typically credited to Mandelbaum (as part of his discussion already mentioned in 2.1) and/or to Lukes.[36],[37] Kincaid formulates it as follows: "Predicates such as teacher, employee, inmate, soldier, or citizen do refer to individuals, but it is reasonable to believe they implicitly involve social terminology as well. To have true statements employing these role predicates, we must also have true statements about social entities, for there are presumably no inmates without prisons, a judicial system, laws and norms, and no teachers without schools. Applying any of these role predicates to someone seems to presuppose or entail a host of further facts about the social institutions that give them meaning. Elimination of social predicates thus becomes quite unlikely" [Kincaid, 1997, 35]. That is, role predicates can only be defined by reference to social wholes, and the only way to refer to social wholes is by way of social whole predicates. In this manner, the use of role predicates presupposes the use of holist predicates. Since the reductive individualist is not allowed to use holist predicates, role predicates are unfit for individualist use.[38]

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[36] See [Mandelbaum, 1973a; Lukes, 1973; 1994]. Reference to, and discussion of, the argument may for example be found in [Bhargava, 1992; Danto, 1973; Gellner, 1968; Goldstein, 1973b; 1959; James, 1984; Lessnoff, 1974, 79-80; Little, 1991; Kincaid, 1995; 1997; Martin, 1972; Quinton, 1975; Watkins, 1973c].

[37] Many holists only use the argument to show that the individualist cannot make use of role predicates. Yet, it is also sometimes formulated to include institutional action descriptions, like cashing a check and voting. This seems right since the same considerations speaking against the use of role predicates apply to institutional action descriptions. For the sake of simplicity, I will focus on role predicates only. It should be emphasized, however, that the objection raised below also applies to the argument when formulated in terms of institutional action descriptions.

[38] Often the argument is formulated as follows: properties, such as being a bank teller, presuppose the existence of social wholes, and hence the individualist is prevented from mentioning such properties [James, 1984, 38]. As it stands, this argument does not have much bite. An

The most straightforward reply to this argument is simply to point out that the anti-reductive holist begs the question. The reductive individualist thinks that social wholes may be described in purely individualist terms; this is the basis for the claim that holist predicates may be matched up on a one-to-one basis with corresponding individualist type descriptions. For this reason, the anti-reductive holist cannot expect the reductive individualist to grant the premise that the only way to refer to social wholes is by way of social whole predicates. By making this assumption, the holist's argument against the individualist use of role predicates presupposes that the basic issue of contention, i.e. whether reductive definitions of holist predicates are possible, has already been settled in favor of the holist. Since this is obviously not the case, and since *prima facie* the reductive individualist has no problems with referring to individuals in terms of their roles vis-à-vis each other, the argument may be rejected without further ado. This means that the above conclusion stands: Little's argument from multiple realization may be dismissed because it wrongly presupposes that the reductive individualist is not allowed to describe individuals' roles vis-à-vis each other. The reductive individualist may propose functional system definitions as his reductive definitions.[39]

A similar line of reasoning may be applied to Kincaid's argument from multiple realization. Kincaid maintains that the anti-reductive holist advances functional state definitions: social whole predicates may be defined by specifying the corresponding social whole's functional role vis-à-vis other social wholes. The reductive individualist defines social whole predicates by reference to individuals' internal organization. As a result he is said to run into the problem of multiple realization since multiple types of organizations of individuals may realize the same type of social whole. In response to the argument, the reductive individualist should point out that Kincaid assumes that the reductive individualist does not have access to role predicates. As just shown, Kincaid's argument to this effect is untenable. This means that the reductive individualist may use role predicates. Hence, specifications of individuals' internal organization may assume the form of functional

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individualist who denies the (*sui generis*) existence of social wholes may mean to deny that social wholes are something more than mere constellations of individuals; that social wholes are causally efficacious, and so on. It is unclear why the property of being a bank teller entails the existence of social wholes in any of the senses typically denied by the individualist. In order to make the argument plausible, it must be shown what kind of existence of social wholes is presupposed by properties such a being a bank teller.

[39]It should be emphasized that the discussion of what kinds of descriptions are available to the individualist does not end here. What about descriptions of individuals' intentional states such as "Individuals believe that the government should lower taxes?" And what about descriptions like "student in medical school?" In both cases, holist terms (viz. government and medical school) figure in the description of individuals. Some theorists stress that individualists are only prevented from using holist predicates that *refer* to social wholes, their properties and so on. On this basis, they argue that when "we speak about institutions in the characterization of agent attitudes we are not referring in the strict sense to those institutions themselves, but rather to the attitudes" [McDonald and Pettit, 1981, 128], see also [Mellor, 1982, 66] for a similar point). In connection with the second example, Nagel holds that "student in medical school" should be considered as a single individualist predicate since it refers to an individual [Nagel, 1968, 535-536].

system definitions, that is, descriptions of individuals' roles vis-à-vis each other. Kincaid's argument from multiple realization should only be accepted if it is impossible to define social whole predicates by way of functional system definitions. As the above examples of functional system definitions illustrate, this is far from obviously the case. Hence, once the reductive individualist is not denied access to role predicates, it is no longer clear that Kincaid's claim of multiple realization holds up.

There is also a second way in which the reductive individualist may challenge Kincaid's argument. The argument presupposes that the reductive individualist's descriptive focus is the internal organization of individuals who compose a given type of social whole. Another descriptive focus is also available: the reductive individualist might equally well try to describe social wholes' functional role in individualist terms. That is, instead of providing functional system definitions, a reductive individualist might insist on advancing individualist functional state definitions as his reductive definitions. A definition along these lines states that a given social whole predicate applies whenever an individualist specification of the functional role of a constellation of individuals (whatever their internal organization) vis-à-vis other constellations of individuals applies.

Kincaid provides a single example of what he means by a functional role ascribed to a social whole. A state, he suggests, is "at least in part, the entity which has more control over organized violence than any other institution" [Kincaid, 1996, 164]. This example suggests that a social whole's functional role vis-à-vis other social wholes should be understood in a purely comparative sense: the state has more control over organized violence than any other social whole. Moreover, in light of the example, it is reasonable to hold that a social whole's role consists, at least in part, in what it does — e.g. the state *controls* organized violence. Thus, in order to provide an individualist specification of a social whole's functional role, the reductive individualist must describe the actions ascribed to social wholes in individualist terms. Kincaid holds that actions ascribed to social wholes supervene upon actions performed by individuals. Consequently, the reductive individualist may simply specify the individual action(s) upon which the action ascribed to the social entity supervene. So, for example, it may be proposed that the state's "controlling organized violence" may be rendered as "appropriate individuals may order the use or restraint of organized violence." On this basis, "state" may then be partially reductively defined as "a constellation of individuals (whatever their internal organization) among whom appropriate individuals may order the use or restraint of organized violence and this to a higher degree than any other individual(s) who are part of other constellations of individuals." In this manner, it appears that Kincaid's argument from multiple realization is inconclusive: it does not take into account the ability of the reductive individualist to adopt another descriptive focus than the one presupposed by his argument. It is not obvious that the reductive individualist will run into the problem of multiple realization when adopting this alternative descriptive focus.

The discussion of Little and Kincaid's positions shows that even if it is granted that social whole predicates are either functional system or functional state definable, it does not follow that the reductive individualist will run into the problem of multiple realization. In both cases, the claims of multiple realization were evaded since they presuppose unreasonable descriptive constraints imposed upon the reductive individualist. As soon as these unreasonable constraints were rejected, the reductive individualist was able to present reductive definitions of Little and Kincaid's supposed examples of social whole predicates said to supervene upon multiple types of individualist predicates. There is no guarantee, of course, that all examples of multiple realization may be dismissed on the grounds that they rely on unreasonable descriptive constraints being imposed upon the reductive individualist: this must be determined by going through the examples one by one.[40] Similarly, whether the reductive individualist will always manage to come up with reductive definitions of holist predicates will have to be decided on a case-by-case basis. Still, the successful employment of the individualist strategy in connection with Little and Kincaid's argument makes it reasonable to conclude that it is far from clear that reductive individualism may be discarded by appeal to the argument from multiple realization. The failure of reductive individualism is not highly likely, but rather an open question.

6   WHERE SHOULD THE DEBATE GO FROM HERE?

Within the current individualism/holism debate, there is as yet no general consensus as to the outcome of the microfoundations debate. Above, it turned out that the dispute about intertheoretic reduction should be seen as undecided too. Despite appearances to the contrary, the argument from multiple realization fails to vindicate anti-reductive holism. Should theorists further pursue one or both of these debates? Or perhaps change focus altogether? In order to answer these questions, parallel discussions and their development within the social sciences should be taken into account. Preferably, the philosophical debate should not only be of interest to philosophers, but also of relevance to working social scientists in the sense of addressing issues which have a bearing on current social scientific theorizing.

Social scientists rarely understand themselves as engaged in the individualism/holism debate. Instead, they talk about the *micro/macro debate* and the *agency/structure debate*. The micro/macro and structure/agency debates address

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[40]So far it has been suggested that the reductive individualist should try to handle claims of multiple realization by arguing that they presuppose a too narrow conception of the kinds of descriptions available to him and/or that they have one descriptive focus when others are available. It may be noticed that there is also a third way in which an individualist's may try to rebut claims of multiple realization. He may argue that the claims presuppose that the individualist reductive definitions are very detailed and that it is because the definitions are held at so detailed a level that he will need to link up holist predicates with several rather than a single more general individualist definition. That is, if less detailed definitions are advanced, the problem of multiple realization may not occur.

the same basic ontological and methodological issues as the individualism/holism debate. Further, both debates frequently neatly line up with the individualism/holism debate in the sense that "the micro level" and "agents" are used to refer to individuals, while "the macro level" and "structures" are taken to refer to social entities, their properties, and so on. Still, this is not always the case. For example, the micro-level is sometimes taken to include smaller units of individuals like families or firms, just as smaller social wholes are considered as agents. The micro-macro formulation of the debate is most common within the American tradition, whereas the agency/structure formulation is typically used in the European tradition. For the sake of simplicity, I will use the latter formulation only.

Before the 1980s, there was a tendency among social scientists to take either agents or structures as their units of analysis.[41] Then in the 1980s, many social scientists began to argue that such one-sided approaches should be abandoned and that attention should instead be directed towards the linkages or relations between agents and structures. There are important differences as to how social scientists think this idea should be worked out in more detail. Still, in their discussions, two kinds of linkages are commonly mentioned. First and foremost, most theorists stress how agents and structures are causally interrelated: causal interaction in both directions takes place between agents and structures. Second, many theorists hold that agents and structures are related in the sense that structures emerge from agents. The general concern with linkage within the social sciences is often referred to as relationism [Ritzer and Gindoff, 1994, 5].[42]

This development within the social sciences may be related to the philosophical individualism/holism debate. It may be specified to what extent the debates on reduction and microfoundations share the social scientific focus on linkage. The use of emergence within social theorizing parallels the use of supervenience within the philosophical debate. Though both notions may be spelled out in different ways, they are, as pointed out in section 4, closely related. The current debate on reduction appeals to the notion of supervenience, yet it does not really take an interest in this type of linkage except in its capacity to pave the way for the argument from multiple realization. Nor does it focus on the causal interaction between individuals and social wholes. In short, the main question around which the debate on reduction revolves is not really of concern to relationists. As Bohman puts it: "[t]he proper theoretical question is not how to reduce one [theory] to the other but how they are linked and interconnected: theoretical debates are no longer about re-

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[41] Standard examples of theories or schools which focus on agents include symbolic interactionism as represented by Herbert Blumer, George C. Homan's exchange theory, and ethnomethodology as associated with Harold Garfinkel. By contrast, Talcott Parsons' structural functionalism, Ralf Dahrendorf's conflict theory, Peter Blau's and Bruce Mayhew's structuralism are standard examples of approaches which focus on structures. For a classification along these lines, see [Ritzer, 1996, 490].

[42] Theorists who may be classified as relationalists include George Ritzer, Jeffrey Alexander, Norbert Wiley, James Coleman, Pierre Bourdieu, Anthony Giddens, and Margaret Archer. For an overview over different relationalist approaches, see [Ritzer, 1996]. Also, for a now classic collection of articles on micro-macro relationism, see [Alexander, et al., 1987]. For a discussion of Bourdieu's and Giddens' practice theories, see Rouse (this volume).

duction, but 'linkage'' [Bohman, 1993, 149]. The microfoundations debate is more in line with current social theorizing. In fact, the micro-individualist position may be seen as exemplifying a relationist approach. Some micro-individualists propose that social wholes, their properties, etc. supervene upon individuals, their actions, and so on. More importantly, they all assign a central role to causal linkages insofar as they require that holist causal claims must always be supplemented by an account of the way in which social wholes, their actions, etc., cause individuals to act, believe etc., in certain ways, which in turn cause changes at the level of social wholes.[43]

If the philosophical debate is to reflect and deal with questions of significance to working social scientists, it should further explore the issues raised by relationist approaches. How do different ways of drawing distinctions between agency and structure relate to each other? How do the different relationist approaches compare? How should this emphasis or insistence upon linkage be justified? How may the idea of structures being emergent (or supervenient) be further spelled out? How exactly do structures causally influence agents, and vice versa? And if structures are said to be emergent, how is the notion of emergent causation to be understood? These are at least some of the questions which philosophers of social science might fruitfully consider.

ACKNOWLEDGEMENTS

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[43]For a further discussion of micro-individualism and related approaches, see Little (this volume).

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LEVELS OF THE SOCIAL

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1   THE FRAME

We can characterize "the social" from the concrete level of individuals in specific relations, to the global structures and institutions that constitute the modern world system, with many stops in between. Social phenomena can be analyzed through a wide variety of contrasts: individual versus institutional, face-to-face versus anonymous, local versus distant, economic versus political, experiential versus structural, immediate versus nested, and many other dimensions of contrast. Do these distinctions represent different *levels* of social phenomena? Are there distinguishable levels of organization within the domain of the social — perhaps from "close to the actor" to "distant", simple to complex, or from concrete to abstract? How do familiar objects of social science investigation like systems of norms, social networks, local social units, families, labor organizations, practices, organizations, institutions, and political or economic structures fit into these questions of level within our conception of the social? And how about causation? Can we assert causal connections from one level to another? Do high-level social structures have causal powers? Do they have effects on local behavior and local institutions? The task for this chapter is to consider how to think rigorously about these levels within the social and within social scientific conceptualizing.

The issue of "levels" within the social can be formulated from several complementary perspectives:

- *Ontology:* what are social entities composed of?

- *Explanation*: do social explanations need to "reduce" to facts about the actions of individuals?

- *Causation*: do "higher-level" social entities have causal powers?

- *Inquiry*: at what level should (a given style of) social inquiry focus its efforts at descriptive and explanatory investigation?

- *Description*: are there "level" requirements or constraints on social description?

- *Generalization*: are there higher-level "types" of social entities that recur in different historical and social settings?

The issues discussed here are important in their own right; but they are even more important because social scientific research needs some fresh thinking about the definition of the object of social scientific inquiry and the nature of social scientific explanation. Too many areas of social science research are motivated by bad analogies with the natural sciences; too many social scientists make facile assumptions about "social structures" in analogy with "physical structures"; and too many work on the assumption that the goal of social science research should be the discovery of generalizations across types of social phenomena: wars, revolutions, regime types, cities, or classes of people.[1] Clear thinking about these fundamentals — the nature of the social; the degree of hierarchy that may exist among social phenomena or structures; and the relations between structured human agency and social outcomes — can help frame our thinking more effectively as we formulate social science research objectives.[2]

This may seem to be a "tired" question, invoking old debates about methodological individualism and holism.[3] I would like to frame the issues here in ways that open new and more fruitful insights. The social sciences have to some extent settled into stereotyped assumptions about the methods and character of social science knowledge. We need to seek out a methodology and ontology that are well suited to the intellectual challenges of the social sciences, given what we know about the social realm. We often make the mistake of reification of social phenomena, and we sometimes go in for a naive naturalism that offers bad analogies with the ordering of "natural" phenomena. Fresh thinking about the subject matter and ontology of the social can allow us to formulate more insightful forms of social research and theory.[4]

The general topic of social levels has been addressed from the perspective of several philosophical theories about social facts. The simplest view is the theory of methodological individualism: the view that social facts and assertions must be reducible in principle to facts and assertions about individuals. Central proponents of methodological individualism include the Austrian school of economics, many economists, and some political scientists.[5] Methodological individualism is one version of a requirement of "inter-level reductionism" that has also been important in biology and psychology; reductionists hold that patterns or laws at a higher

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[1]Concerns about the most basic assumptions underlying contemporary social science research have been raised by George and Bennett [2005], Lieberson [1987], McAdam, Tarrow, and Tilly [2001], Rabinow [2003], Steinmetz [2004; 2005], Adams, Clemens, and Orloff [2004], and numerous contributors to McDonald [1996] and Mahoney and Rueschemeyer [2003].

[2]Lieberson offers equally deep criticisms of the effort to model social science research on the natural sciences; [Lieberson, 1987].

[3]For a historical treatment and analysis of methodological holism and individualism, see Zahle, this volume.

[4]Fresh approaches to the topic of relations across levels of social life have been offered in the past decade. Essays in Alexander [1987] provide a broad discussion of these issues, as do several contributions in Lichbach and Zuckerman [1997], McDonald [1996], and Mahoney and Rueschemeyer [2003].

[5]See Weber [1968, 13], Popper [1957], Watkins [1968] for primary expressions of the position. Lukes [1973] and Miller [1978] offer philosophical criticisms of the position. For further discussion, see Zahle, this volume.

level of organization must be reduced to a set of laws at the lower level (for example, descriptions of the workings of the human cognitive system should be reduced to facts about neurophysiological organization of the brain). On this approach, we should replace higher-level concepts with lower-level concepts, and we should explain higher-level outcomes as the result of the workings of lower-level processes. The theory of supervenience offers a related but less restrictive view regarding social ontology and explanation: social entities are dependent upon facts about individuals, but it is not necessary to reduce statements at the social level to equivalent statements couched in the language of individuals.[6] The theory of supervenience may be summarized under the slogan: Properties at level A supervene upon properties at level B if and only if there is no difference at level A without a difference at level B. Each of these theories gives some form of primacy to facts about individuals, deriving from the obvious truth that social phenomena are constituted by the actions and mental states of individuals.

There is another perspective on social life that doubts the availability or legitimacy of higher-level social structures, under the slogan of pursuing "local knowledge". This approach is most associated with the discipline of anthropology, but some historians and sociologists also adopt the perspective.[7] On this approach, the task of social research is to discover the features of local life and interaction that are found in a given location. There are no higher-level social facts or structures; there is only the ensemble of local relationships and actors in direct interaction with each other. This approach characterizes the work of social scientists who pursue highly localistic studies: local histories, local ethnographies, and local sociological studies. Institutions are invoked only to the extent that local actors perform roles in these institutions.

Opposed to this group of individual-centered views of social entities and explanations is a family of views that assert primacy or independence for social entities and structures. Durkheim's social holism is an instance of this approach; Durkheim asserts that there are social forces and conditions that exert their influence independent from the individual's states of mind. Individuals are influenced by large social facts rather than determining large social facts [Durkheim, 1964]. This approach implies that social properties are "emergent": they are qualitatively distinct from the properties of individuals, and they emerge only at a

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[6]See Yaegwon Kim's exposition of this concept [Kim, 1984; 1993; 2005].

[7]Clifford Geertz makes extensive use of this phrase [Geertz, 1983], but many other social investigators share elements of the perspective. See, for example, James Scott's treatment of local knowledge in *Seeing Like a State* [Scott, 1998]. Marcus and Fischer represent the ethnographic preference for localism in these terms:

> "A jeweler's-eye view of the world is thus urgently needed, and this is precisely where the strength and attractiveness of cultural anthropology reside at the moment. ... Anthropology's distinctive method of research, ethnography, has long been focused precisely on problems of the recording, interpretation, and description of closely observed social and cultural processes" [Marcus and Fischer, 1986, 15].

But Marcus and Fischer go on to argue for the need to "represent the embedding of richly described local cultural worlds in larger impersonal systems of political economy" (77).

certain level of population or complexity. Structuralism asserts that social structures have causal powers that are independent from the actions and states of mind of individuals; social causation is in some sense autonomous from the states and actions of individuals. Social structures are thought to be persistent entities with stable causal properties over time. Examples of enduring social structures might include the absolutist state [Anderson, 1974], the Protestant ethic [Weber, 1930], the modern world system [Wallerstein, 1974], or global trading regimes [Bhagwati, 2004]. Some versions of Marxism depend on this view [Althusser and Balibar, 1970; Hindess and Hirst, 1975], and intimations of this assumption are found in the historical sociologies of Skocpol [1979], Tilly [1984], and Wallerstein [1974]. Holist and structuralist theorists maintain the autonomy and independence of social constructs and structures; if they recognize the role that individuals play in "embodying" the structure, they insist on the interchangeability and causal insignificance of these individuals. (A sand dune consists of trillions of grains of sand; but no individual grain of sand influences the shape of the dune.)

There is a third perspective on the question of social levels that is more compelling than either of these extremes of individualism and holism. I refer to the third perspective as "methodological localism", and it is designed to capture the elements of truth involved in both individualism and structuralism. This perspective affirms that there are large social structures and facts that influence social outcomes, but it insists that these structures are only possible insofar as they are embodied in the actions and states of socially constructed individuals. With individualism, the moderate position embraces the point that individuals are the bearers of social structures and causes. There is no such thing as an autonomous social force; rather, all social properties and effects are conveyed through the individuals who constitute a population at a time. Against individualism, however, methodological localism affirms the "social-ness" of social actors. ML denies the possibility or desirability of characterizing the individual pre-socially. Instead, the individual is understood as a socially constituted actor, affected by large current social facts such as value systems, social structures, extended social networks, and the like. In other words, ML denies the possibility of reductionism from the level of the social to the level of a population of non-social individuals; rather, the individual is constituted by social facts, and the social facts are constituted by the current characteristics of the persons who make them up. Furthermore, ML affirms the existence of social constructs beyond the purview of the individual actor or group. Political institutions exist, and they are embodied in the actions and states of officials, citizens, criminals, and opportunistic others. These institutions have real effects on individual behavior and on social processes and outcomes — always mediated through the structured circumstances of agency of the myriad participants in these institutions and the affected society. This perspective emphasizes the contingency of social processes, the mutability of social structures over space and time, and the variability of human social systems (norms, urban arrangements, social practices, etc.).

The "localism" that is part of the ML position brings social explanation back to the individual, in that it affirms that we must be able to offer "microfoundations" for the pathways by which these socially constituted individuals are influenced by distant social circumstances. There is no action at a distance in social life; instead, individuals have the values that they have, the styles of reasoning, the funds of factual and causal beliefs, etc., as a result of the structured experiences of development that they have undergone as children and adults. On this perspective, large social facts and structures do indeed exist; but their causal properties are entirely defined by the current states of psychology, norm, and action of the individuals who currently exist. Systems of norms and bodies of knowledge exist — but only insofar as individuals (and material traces) embody and transmit them. So when we assert that a given social structure causes a given outcome, we need to be able to specify the local pathways through which individual actors embody this causal process. That is, we need to be able to provide an account of the causal mechanisms that convey social effects.

This approach has numerous intellectual advantages: it avoids the reification of the social that is characteristic of holism and structuralism, it abjures social "action at a distance", and it establishes the intellectual basis for understanding the non-availability of strong laws of nature among social phenomena. It is possible to offer numerous examples of social research underway today that illustrate the perspective of methodological localism; in fact, as I will argue below, almost all rigorous social theorizing and research can be accommodated to the assumptions of methodological localism. But a particularly strong example is to be found in the literature associated with the "new institutionalism" — a body of work that attempts to locate the social effects of particular institutional arrangements [Brinton and Nee, 1998]. The efforts to identify the causal mechanisms associated with popular politics and mobilization in the work of Charles Tilly and his colleagues represent another good example [McAdam, Tarrow, and Tilly, 2001]. And in fact, most of the works by comparativist researchers who are sometimes characterized as structuralist are in fact compatible with the approach of methodological localism, including Skocpol and Tilly.[8]

2   AN EXAMPLE: A FARMING VILLAGE

We may begin our inquiry into the question of social levels with a stylized example. Imagine a Chinese farming village consisting of several hundred families. These thousand persons have a dense set of face-to-face social relationships with each other: familial, religious, economic, political, and agricultural. They participate in labor-sharing practices with each other, they gossip about each other, they

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[8]Examples of theories and analyses contained in current comparative and historical social science research may be found in McDonald [1996] and Mahoney and Rueschemeyer [2003]. Many of these examples illustrate the fecundity of the approach to social analysis that emphasizes the "socially constructed individual within a concrete set of social relations" as the molecule of social action.

participate in rituals of life and death together, they buy and sell products from each other, they possess religious and ethnic identities, they sometimes mobilize as bandit gangs or militia organizations in times of stress. These persons also have long-distance relationships with other persons (urban kin, for example) and with governmental institutions (the tax system, political registration system, legal system). Their daily lives are affected by regional, national, and international institutions — grain markets, transportation systems, mass media, institutions of party and state, the institutions of the global trading system. Some are followers of heterodox religious movements with regional and national scope (perhaps the Falun Gong or White Lotus societies). Their life circumstances are affected by both local relationships (a generous landlord, an over-zealous official) and by regional or national networks and institutions (governmental, economic, or cultural). So the social reality of the village is multiple and multi-layered: multidimensional, local, distant, personal, anonymous . . .

Social science investigators can bring a series of questions into play when they become interested in the village. They may seek to describe some aspects of social life in the village: "What is the standard of living of smallholding peasants and how has it changed in the past 20 years?" They may ask questions about the lived experience of persons in the village: "How do relations of class and patriarchy affect the daily experience of villagers?"; "How have attitudes towards citizenship and equality changed over the past 20 years?" They will ask causal questions: "How has the rising level of private Chinese investment affected labor opportunities in the village?"; "How has China's reproductive policy affected the demography of the village?" They may ask comparative questions: "How do conditions in this village compare to other villages in other regions in China (or in other parts of the country or the continent)?" They may ask questions concerning the links between the village and more distant places: "What is the nature of long-distance credit networks connecting the village to distant cities?" "How are party cadres disciplined, motivated, and controlled by the party bureaucracy?"

So reflection on the nature of social phenomena and social science can begin in this microcosm without excessive narrowing. Within this example we can immediately see topics of research for practitioners of all the traditional social science disciplines: ethnography, economics, economic history, political science, geography, demography, sociology, and neo-institutionalism. And we can identify different aspects of local life and experience that perhaps correspond to the disciplines: agriculture, trade, religion, social networks, family, inequality, education and mass media. These are "zones" of social activity that can be singled out for systematic study and inquiry by specific disciplines. We can note as well that these segments of social activity are not sharply segregated; so when the farmer cultivates or harvests, he or she may be simultaneously acting economically, culturally, cooperatively, religiously, and fiscally.

There are two stylized forms of isolation that are *not* found in this description. There are no "socially unadorned atomized individuals" in this story; rather, these villagers are fully engaged in a dense network of social relationships, connections,

and constraints. But likewise, the village itself is no isolated locus of social activity, but is instead linked inextricably to more distant social forces, institutions, and organizations (government, trading system, religious networks, globalization, off-shore communities, etc.).

This example suggests several important clues about the layers, levels, and strata of social phenomena. It alerts us to questions about social ontology — the variety of social entities to which we refer in social analysis. The example refers to a variety of social formations and systems at a variety of levels (region, administrative hierarchy, system nestedness, . . .): the multiple social relationships, institutions, networks, and structures within which people live and act. These social formations are complex agglomerations, bringing together the actions and purposes of large numbers of other persons. The social formation is often embedded within a material structure — road networks, government buildings, collections of published regulations and laws. Further, the example provides a microcosm for an understanding of structure and agency; individuals living and acting according to their own purposes, within a set of constraints and conditions that influence their choices. Third, the example draws our attention to the poles of stability and dynamism that so commonly characterize social life: institutions, practices, and traditions that persist and constrain agents, but that change over time as a result of both local and contextual developments.

The example invites us to ask about the composition of social constructions — what they are made up of. What does the "county government" consist in? What constitutes the "periodic market system" for produce, grain, and other commodities? How is the "provincial government and administration" composed, and how does it exert influence in the village? The example also invites us to ask about the mechanisms of social causation that are invoked in the example: how distant social formations exert influence affairs locally — how "the state" influences settlement patterns and collects taxes; how the White Lotus movement influences local people; or how international grain markets influence the local standard of living.

Several extreme, and incorrect, points of view might come forward in response to this example. First, it might be maintained that "all social phenomena are face-to-face and local". On this perspective, the social reality represented in the example is exhausted by a description of the local, lived social relationships that occur among the villagers, and the sporadic contact they have with outsiders. However, this approach is plainly incorrect, in that the village is lodged within a larger political, economic, and natural environment, and these supra-local factors influence and constrain the choices that are made by persons in the village.[9] Moreover, the networks of relationships within which villagers operate extend far beyond the village: to the region, the nation, and to off-shore Chinese communities, for example. So "ultra-localism" is a bad description of the nature of the

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[9]This point is made forcefully by Jean Comaroff in her excellent ethnography of the Tshidi of South Africa [Comaroff, 1985]; see also [Marcus and Fischer, 1986, 77 ff.].

social. Regional, national and international factors permeate and influence social life and agency in the village.

At the other extreme, some might maintain that "social structures" exert an impersonal and pervasive causal power essentially independent from the agency of participants; so local affairs are controlled or programmed by the needs and imperatives of the state, national economic development, or the global economy. This approach is a form of reification — the attribution of causal powers to entities without an understanding of the mechanisms through which those powers are expressed. We need to know how the state succeeds in imposing policies, collecting taxes, and maintaining order at the local level, and this requires an analysis of the connective institutions and organizations through which the state's will is expressed, transmitted, transformed, and deployed locally. So "ultra-structuralism" is also a bad description of the nature of the social.[10]

Our example gives a textured illustration of what we mean by the question of "levels of the social": there are social relations and structures invoked in this example that are intuitively "lower-level", intermediate, and "higher-level"; there is composition of social institutions (local village political authority nested within provincial or national political institutions); and there are distant social structures that influence outcomes and actions within the village. So our challenge is to shed some light on how these levels are knit together, how actions and circumstances at one locus have effects at other loci, how subordinate institutions are governed or regulated by higher-level institutions, and whether there are grounds for distinguishing across levels of the social, even provisionally and approximately. In short, we are led to ask how the large and loose network of forms of social organization and causal influence hang together.

It is with respect to this fluid domain of social phenomena, things, structures, agents, ideas, and organizations, that we can ask the question: Are there levels of the social? It is a truism that social items are composed of individuals and facts about individuals. Does this point have implications for the issue of the levels of social phenomena? Are social formations arrayed in some hierarchical fashion, with so All done 16.5.06me items being "higher level" than others? And what, if anything, can we say about the causal processes that tie these social processes and formations together?

3   WHAT IS THE "SOCIAL WORLD"?

Let us advance our exploration of our topic by asking about the constitution of social entities, and considering whether there are features of the constitution of the social that conform to the idea that some social facts are lower or higher than others.   What do we mean by "the social world"? What are "actors", "social behavior", "social groups", "social life", "social organization", "social structure",

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[10]The insights of the "new institutionalism" represent a response to exactly this concern: an account of the mechanisms through which distant structures and organizations succeed or fail in influencing local behavior [Powell and DiMaggio, 1991; Brinton and Nee, 1998].

or "society"? These are inherently open-ended questions, because the "social" itself is open-ended. But we can begin with a few simple premises. The social realm includes *persons* involved in intensional relationships with each other; persons whose beliefs, values, purposes, and ways of thinking have been shaped by exposure to social relationships and institutions (prior and ongoing); and persons whose actions have effects on the actions and mental states of other persons. Persons are socially constituted. They engage in social acts, and they have effects on social outcomes through their action and inaction. Second, the social includes *organizations and institutions* (governments, armies, labor unions, bandit gangs), which can be described more concretely (the persons who constitute the corners and connectors of the organization) or more abstractly (the rules and modes of functioning that characterize the organization). The social includes as well *social structures* (states, trading systems, international regimes); systems of *ideas, practices, norms, and values*; and large *factors* of human interaction (race, gender, class, sexual identity). And the social culminates in groups of individuals who possess *population characteristics*: for example, income, life expectancy, occupation, religion, degree of solidarity . . .

This account begins with the socially constituted person. Human beings are subjective, intentional, and relational agents. They interact with other persons in ways that involve competition and cooperation. They form relationships, enmities, alliances, and networks; they compose institutions and organizations. They create material embodiments that reflect and affect human intentionality. They acquire beliefs, norms, practices, and worldviews, and they socialize their children, their friends, and others with whom they interact. Some of the products of human social interaction are short-lived and local (indigenous fishing practices); others are long-duration but local (oral traditions, stories, and jokes); and yet others are built up into social organizations of great geographical scope and extended duration (states, trade routes, knowledge systems). But always we have individual agents interacting with other agents, making use of resources (material and social), and pursuing their goals, desires, and impulses.

To start, then, we may say that the social has to do with the phenomena and patterns that result from agents' behavior when that behavior is oriented to, or generated by, the actions, intentions, constraints, and states of other agents. The social has to do with *social development* — the construction of the agent through processes of socialization — and *social action, interaction, and aggregation*: behavior, choice, and agency in which the actor considers the effects, actions, purposes, and states of other agents. This definition directly encompasses features of behavior such as cooperation, competition, altruism, and aggression. It includes teams, friends, opponents, enemies, and competitors. Is there any human action that is purely non-social? We might think of Robinson Crusoe, but this is not a real human circumstance. And we might think of ordinary, every-day self-affecting actions — eating, drinking, smoking, reading, or gardening. But it is hard to say that these actions are purely pre- or non-social, since our tastes and preferences are socially formed, we learn to garden from others, our actions are constrained

by law and custom, and the use of language is inherently social. We might say that these actions are socially situated, constructed, or constrained, even if they are not oriented toward the actions of others. So almost all human action is social: socially oriented, socially embedded, or socially constructed (through socialization and education).

So far we have emphasized the socially situated individual. But social action takes place within spaces that are themselves socially structured by the actions and purposes of others, by property, by prejudice, by law and custom, and by systems of knowledge. So our account needs to identify the local social environments through which action is structured and projected: the inter-personal network, the system of rules, the social institution. The social thus has to do with the behaviorally, cognitively, and materially embodied reality of *social institutions*. An institution, we might say, is an embodied set of rules, incentives, and opportunities that have the potential of influencing agents' choices and behavior.[11] An institution is a complex of socially embodied powers, limitations, and opportunities within which individuals pursue their lives and goals. A property system, a legal system, and a professional baseball league all represent examples of institutions.[12] Institutions have effects that are in varying degrees independent from the individual or "larger" than the individual. Each of these social entities is embodied in the social states of a number of actors: their beliefs, intentions, reasoning, and histories. Actors perform their actions within the context of social frameworks represented as rules, institutions, and organizations, and their actions and dispositions embody the causal effectiveness of those frameworks. And institutions influence individuals by offering incentives and constraints on their actions, by framing the knowledge and information on the basis of which they choose, and by conveying sets of normative commitments (ethical, religious, interpersonal) that influence individual action.

These social institutions are thought to be more impersonal, more independent from specific individuals, and more sustained over time, than the fleeting patterns of everyday social face-to-face interaction. But persistent social systems and structures also depend on the social states of actors: their beliefs, memories, purposes, patterns of reasoning, and face-to-face social relationships. The world trading system *circa* 1850 depended on actors (sea captains, port officials, banking officers, sailors, company directors), organizations (the East India trading company, treaty organizations, the British Navy), and states designed to permit certain actors to bring about their purposes and to constrain the behavior of countless others. Institutions are embodied in the social individuals who make up the population and in the material artifacts that commonly represent some of the traces of the institution. It is legitimate to assert the existence of spatially and temporally

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[11] "Institutions are the humanly devised constraints that structure human interaction. They are made up of formal constraints (for example, rules, laws, constitutions), informal constraints (for example, norms of behavior, conventions, self-imposed codes of conduct), and their enforcement characteristics. Together they define the incentive structure of societies and, specifically, economies" [North, 1998, 247].

[12] See [Brinton and Nee, 1998; North, 1990; Ensminger, 1992], and [Knight, 1992] for recent expositions of the new institutionalism in the social sciences.

extended structures and institutions, providing that we keep clearly in mind the sorts of individual-level activities and mechanisms that give these structures their coherence and stability over time.

What can we say about the ontological and causal status of social entities beyond the scope of acquaintance of the situated individual agent — structures, organizations, networks, or social systems? Social entities are less tangible than physical entities, so the issue of characterizing and identifying a set of social conjunctions as an enduring and extended "thing" is more challenging. Two large sets of questions confront us about a given social entity (for example, a structure or system). First, what are the social "threads" that suffice to unify a range of social actors, institutions, and places into a single unified historical entity (that is, what are the criteria of identity for a "single social entity")? Is "China during the Han Dynasty" one unified social formation; or is it a congeries of semi-independent regional cultures, economies, and social orders? Is "the Chinese imperial state" a single historical entity over the 4000 years of Chinese political history? And second, at what level of description is it credible that we can re-identify "the same" institutions or practices in separate historical formations? Is there some quality of "state-ness" that is possessed by the French absolutist state, the Chinese imperial system, and the Indian polity?

In view of these questions, what can we say about the topic, "what things exist in the social realm?" Here I will describe a social ontology that falls closer to the spare end of the spectrum. On this approach, what exists is the socially constructed individual, within a congeries of concrete social relations and institutions. The socially constructed individual possesses beliefs, norms, opportunities, powers, and capacities. These features are socially constructed in a perfectly ordinary sense: the individual has acquired his or her beliefs, norms, powers, and desires through social contact with other individuals and institutions, and the powers and constraints that define the domain of choice for the individual are largely constituted by social institutions (property systems, legal systems, educational systems, organizations, and the like).[13]

On this approach, a given society may be said to consist of specific social, economic, and political institutions, mentalities and systems of beliefs and values, and higher level structures that are composed of these institutions, practices, and mentalities. All of these social factors are constituted by the set of agents who populate them at a given time. Agents act within the context of these structures; and their actions both reproduce and modify the structure. At any given time, agents are acting in ways that affect future states of the system while being prompted or constrained by existing structures and mentalities; and agents are being shaped by these structures and mentalities in ways that influence their future actions. Finally, the social formation is subject to "exogenous" influences: climate change, war, natural events (disastrous or favorable), and the appearance of singular and exceptional individuals.

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[13]Hacking [1999] offers a critique of misuses of the concept of social construction. This use is not vulnerable to his criticisms, however.

Inevitably, social organizations at any level are constituted by the individuals who participate in them and whose behavior and ideas are influenced by them; sub-systems and organizations through which the actions of the organization are implemented; and the material traces through which the policies, memories, and acts of decision are imposed on the environment (buildings, archives, roads, etc.). The organization exists and serves as a valid object of scientific study. The task for the social scientist is to uncover the institutions, rules, incentives, prohibitions, and enforcement mechanisms through which the behavior of sub-systems and agents is regulated. All features of the organization are embodied in the actors and institutional arrangements that carry the organization at a given time.

Within this formulation we can characterize the level of a given social unit by tracing out its location within a broader set of organizations and institutions, and its downward relationships to agents (as participants, subjects, and entrepreneurs). At each point we are invited to ask the question: What are the social mechanisms through which this institution or organization exerts influence on other organizations and on agents' behavior?

How do statistical facts about human populations fit into this analysis of the social and of social research and explanation? Groups of people present distributions with respect to various characteristics: wealth, height, propensity to make charitable gifts, life expectancy, and so on. So, facts about groups are normally statistical statements about the distribution of one or more properties across a population. The population of interest can be singled out in a variety of ways: by location, by nationality or race, or by occupation, for example. And we can use the methods of social measurement and observation to estimate the distribution of the variable across the population (survey research, household studies, self-reports, direct observation, etc.). As a result, it is an important part of social scientific knowledge to provide description and analysis of the distribution of properties across populations of people.

Observation of one variable across a population permits us to make descriptive statements about this population, for example, "the average income in metropolitan Detroit is $22,500, with a bottom decile of $12,500, a top decile of $225,000, and a Gini coefficient of .55." Once we have estimated multiple variables for the population, we can look for meaningful statistical differences in the distribution of one variable across the population when differentiated by a second variable: "Male workers in Cleveland earn salaries, adjusted by skill and seniority, that are 25% greater than female workers in Cleveland", or "College educated workers earn 20% more than non-college-educated workers in Cleveland". And we can make comparisons across populations: Detroit workers versus Cleveland workers, family size in the U.S. north versus the south ... Finally, we can test our data for correlations, statistical associations, meaningful differences in descriptive statistics (means, medians), and regression characteristics.

On the ontology being advanced here, higher-level entities such as "the Canadian federal state" are the sum of the constellations of socially situated individuals and institutions that exist at a given time. Social institutions and organizations

come together to constitute complexes of institutions that we denote as "state", "military regime", "market", "family", and other medium- and large-scale structures. Higher-level structures indeed exist, but they supervene upon individuals and lower-level institutions. It is reasonable, on this approach, to affirm the existence of social structures like "the seventeenth-century French absolutist state", "the American industrial system", or "the Soviet military system", if we note carefully the subordinate ontological status that these higher-level structures have. These social entities exist in the particular concrete forms that make them up in a particular time and place: the institutions that create rules, powers, and opportunities; the assignment of powers and restrictions to particular officers; the material factors and objects that embody various elements of these systems; the assumptions and values that individuals bring to their interactions with these institutions, and the like. In all instances the social entity is constituted by the social constructed individuals who make it up, through their beliefs, values, interests, actions, prohibitions, and powers.

These higher-level organizations and institutions constitute larger systems that can be termed "political", "economic", "demographic". But the latter set of terms — "state", "market", "economic sphere", "religion" — should be regarded as nominal and provisional rather than essential. The French state, the British state, and the Indian polity all exist, but "the state as such" does not. Institutional configuration is plastic in its development and relatively sticky in operation. We can regard specific social formations as constituting distinctive regimes: distinctive and interlocking systems of institutions, norms, and groups that persist over time and through which agents pursue their goals. Moreover, given well-known processes of social feedback and selection, institutional settings will come over time to be adjusted so as to constitute a coherent system of institutions for accomplishing the social purposes of the society in question.

The mutability and variety of social institutions — and therefore the inappropriateness of an essentialist view of "capitalism", "city", or "clientelism" — follows from a universal feature of human social agency. At any given time, agents are presented with a repertoire of available institutions and variants (along the lines of Tilly's [1986] point about a repertoire of strategies of collective action or Bourdieu's [1977] analysis of social practice). The content of the repertoire is historically specific, reflecting the examples that are currently available and that are available through historical memory. And the repertoire of institutional choices for Chinese decision makers was significantly different from that available in early modern Europe [Wong, 1997].[14]

So the ontology that I defend comes down to socially constituted agents within social relations and institutions, possessing a set of material needs and purposes and a set of norms, beliefs, and goals that constitute the ground of their agency. These institutions convey individuals to the accomplishment of their purposes and

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[14] "Despite the importance assigned by many scholars to the role of institutions in structuring political life, the issue of how these institutions are themselves shaped and reconfigured over time has not received the attention it is due" [Thelen, 2003, 208].

embody various forms of power, production, and reproduction. And these institutions and practices in turn form larger configurations of institutions, practices, and organizations that we refer to as "states", "economic systems", "demographic regimes", and the like. It is then an empirical and contingent discovery when we discern important commonalities among the institutions of several distinct social formations — for example, similar systems of land tenure or systems of revenue extraction.

This ontology gives central focus to the relationship between structure and agency. Agents constitute structures; and agents are in turn constituted by structures. How can this apparent circularity be interpreted? The relationship between structures and agents is one of ongoing mutual influence, within and across generations. Agents constitute structures through their beliefs, preferences, and actions. Their actions are to some extent "channelized" by the incentives and disincentives created by existing institutional arrangements; more deeply, agents themselves are shaped through their educational and social development — processes that are themselves richly informed by the workings of institutions and organizations. Finally, individuals have the ability to change institutions — either dramatically through leadership or slowly through many acts of anonymous opportunism.

The key to the looseness of social organization to which we have referred frequently derives from the human ability to imagine new forms of social interaction, to innovate socially and collectively, to defect from social expectations. As a result, we get differential degrees of fit between individual action and "structures", "institutions", and "norms". There is a regular propensity to "morphing" of higher-level structures. Agents create institutions, they support institutions, they conform their behavior to the incentives and inhibitions created by institutions, they defy or quietly defect from norms, they act opportunistically or on principle ... So the hard question is not "Do institutions and structures exercise autonomous and supra-individual causal primacy?" since we know that they do not. Instead, the question is "To what extent and through what sorts of mechanisms do structures and institutions exert causal influence on individuals and other structures?"

A coherent social ontology can now be formulated: individuals in social relations exist. Individuals in social relations constitute institutions that exist (that is, persist and maintain their properties for extended periods of time). Configurations of institutions form higher-level complexes that we describe as large social structures: political systems, economic systems, cultural systems. And these higher-level structures too possess the qualities of persistence and continuity over significant periods (and surviving the comings and goings of the individuals who constitute them at a specific time) that permit us to say that they exist as durable social entities. This ontology is counterpart to the "microfoundations" theory described below. It places the level of "thing"-ness in the social realm close to the level of individuals in social relations and practices.

4   CAUSAL MECHANISMS AND MICROFOUNDATIONS

Let us turn now to the second large question involved in investigating the levels of social organization: how does social causation function across the breadth, depth, and layers of social life? I maintain that social explanation requires discovery of the underlying causal mechanisms that give rise to outcomes of interest.[15] Social mechanisms are concrete social processes in which a set of social conditions, constraints, or circumstances combine to bring about a given outcome. On this approach, social explanation does not take the form of the inductive discovery of laws. The generalizations that are discovered in the course of social scientific research are subordinate to the more fundamental search for causal mechanisms and pathways in individual outcomes and sets of outcomes. This approach also casts some doubt on the search for generalizable theories across numerous societies; it looks instead for specific causal variation. The approach emphasizes variety, contingency, and the availability of alternative pathways leading to an outcome, rather than expecting to find a small number of common patterns of development or change. The contingency of particular pathways derives from several factors, including the local circumstances of individual agency and the across-case variation in the specifics of institutional arrangements — giving rise to significant variation in higher-level processes and outcomes.[16]

In *Varieties of Social Explanation* [Little, 1991] I argue that the central idea of causal ascription is the idea of a causal mechanism: to assert that A causes B is to assert that there is a set of causal mechanisms such that A in the context of typical causal fields brings about B (or increases the probability of the occurrence of B). A causal mechanism is a series of events or processes that lead from the explanans to the explanandum [Little, 1991, 15].[17] This approach may be called "causal realism", since it rests on the assumption that there are real causal powers underlying causal relations.[18] This approach places central focus on the idea of

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[15]Important recent exponents of the centrality of causal mechanisms in social explanation include Hedström and Swedberg [1998a], McAdam, Tarrow, and Tilly [2001], and George and Bennett [2005]. One specific interpretation of the idea of a social mechanism is formulated by Tyler Cowen: "I interpret social mechanisms . . . as rational-choice accounts of how a specified combination of preferences and constraints can give rise to more complex social outcomes" [Cowen, 1998, 125]. The account offered in this article is not limited to rational choice mechanisms, however.

[16]McAdam *et al.* describe their approach to the study of social contention in these terms: "We employ mechanisms and processes as our workhorses of explanation, episodes as our workhorses of description. We therefore make a bet on how the social world works: that big structures and sequences never repeat themselves, but result from differing combinations and sequences of mechanisms with very general scope. Even within a single episode, we will find multiform, changing, and self-constructing actors, identities, forms of action and interaction." [McAdam, Tarrow, and Tilly, 2001, 30]

[17]Jon Elster offers a similar approach to social explanation. See particularly [Elster, 1989b; 1989a].

[18]I make the case for this view at greater length in *Varieties of Social Explanation* [Little, 1991, chapter 2]. Richard Miller has advocated a similar conception of social explanation; he writes that "an adequate explanation is a true description of underlying causal factors sufficient to bring about the phenomenon in question" [Miller, 1991, 755].

a causal mechanism. To identify a causal relation between two kinds of events or conditions, we need to identify the typical causal mechanisms through which the first kind brings about the second kind. Finally, I argue for a microfoundational approach to social causation. The causal properties of social entities derive from the structured circumstances of agency of the individuals who make up social entities — institutions, organizations, states, economies, and the like. This idea will be more fully articulated below.

What is the nature of the causal relations among structures and entities that make up the social world? What sorts of mechanisms are available to substantiate causal claims such as "population pressure causes technological innovation", "sharecropping causes technological stagnation in agriculture", or "limited transport and communication technology causes infeudation of political power"? What are the causal mechanisms through which social practices, ideologies and systems of social belief are transmitted? How are structures and practices instantiated or embodied, and how are they transmitted and maintained? Do causal claims need to be generalizable? How do historians identify and justify causal hypotheses?

The general answers I offer flow from a very simple perspective. Social structures and institutions have causal properties and effects that play an important role within historical change (the social causation thesis). They exercise their causal powers through their influence on individual actions, beliefs, values, and choices (the microfoundations thesis). Structures are themselves influenced by individuals, so social causation and agency represent an ongoing iterative process (the agency-structure thesis). And hypotheses concerning social and historical causation can be rigorously formulated, criticized, and defended using a variety of tools: case-study methodology, comparative study, statistical study, and application of social theory.[19]

Causal realism gives central place to the mechanisms that mediate between cause and effect. What can we say about the mechanisms that mediate social causation? Once we recognize that social phenomena are not governed by strict causal laws akin to some views of the physical sciences, we are forced to ask ourselves the question: What sorts of processes might constitute the metaphysics of social causation? I argue for a *microfoundational* approach to social causation: the causal properties of social entities — institutions, organizations, states, economies, and the like — derive from the structured circumstances of agency of the individuals who make up those entities, and from nothing else [Little, 1989]. There are no causal powers at work within the domain of the social that do not proceed through structured individual agency.

The microfoundations thesis holds that an assertion of an explanatory relationship at the social level (causal, functional, structural) must be supplemented by two things: knowledge about what it is about the local circumstances of the typical individual that leads him to act in such a way as to bring about this relationship; and knowledge of the aggregative processes that lead from individual actions of

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[19]See [Little, 1998, chapters 10 and 11] for exposition of these ideas.

that sort to an explanatory social relationship of this sort.[20] So, if we are interested in analysis of the causal properties of states and governments, we need to arrive at an analysis of the institutions and constrained patterns of individual behavior through which the state's purposes are effected. We need to raise questions such as these: How do states exercise influence throughout society? What are the institutional embodiments at lower levels that secure the impact of law, taxation, conscription, contract enforcement, and other central elements of state behavior?[21] And if we are interested in analyzing the causal role that systems of norms play in social behavior, we need to discover some of the specific institutional practices through which individuals come to embrace a given set of norms.[22]

How does this approach to social causation connect to causal reasoning in the quantitative social sciences? It was noted above that statistical description and analysis of populations are crucial parts of social scientific knowledge. Our interest in the statistical measurement of variables within populations takes two forms. First, a description of the distribution of a variable across the population is an important form of factual knowledge about the life circumstances of the people in this group. Referring to the "typical" peasant in Uttar Pradesh is unsatisfactory; we are much better served by studies that provide information about the range of incomes, land tenancy, taxation, citizenship rights, and health that characterize the rural population as a whole in Uttar Pradesh. Statistical data allow us to describe the group with a degree of precision — inexact but informative. Second, statistical analysis permits us to identify variables that may be causally linked: smoking and cancer, gender and income, race and health status. So, statistical analysis can start us on the way of inquiry and explanation.

The literature on causal reasoning on the basis of statistical evidence is very large, but some points are clear.[23]

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[20]We may refer to explanations of this type as "aggregative explanations". An aggregative explanation is one that provides an account of a social mechanism that conveys multiple individual patterns of activity and demonstrates the collective or macro-level consequence of these actions. Thomas Schelling's *Micromotives and Macrobehavior* [Schelling, 1978] provides a developed treatment and numerous examples of this model of social explanation.

[21]An excellent recent example of historical analysis of Chinese local politics illustrates the value of this microfoundational approach: "But the villages were not totally out of the government's reach; nor was the subcounty administration necessarily chaotic, inefficient, and open to malfeasance. In fact, during most of the imperial times, the state was able to extract enough taxes to meet its normal needs and maintain social order in most of the country. What made this possible was a wide variety of informal institutions in local communities that grew out of the interaction between government demands and local initiatives to carry out day-to-day governmental functions" [Li, 2005, 1].

[22]"Explanations of social norms must do more than merely acknowledge the constraining effects of normative rules on social action. Such explanations must address the process that culminates in the establishment of one of these rules as the common norm in a community. One of the keys to the establishment of a new norm is the ability of those who seek to change norms to enforce compliance with the new norm" [Knight and Ensminger, 1998, 105].

[23]Recent insightful discussions of statistical causal reasoning include Salmon [1998], Simon [1971], Woodward [1995], Cartwright [1995], Goertz and Starr [2003], Humphreys [1986], Lieberson [1987], and Pearl [2000], and many of the contributions in McKim and Turner [1997]. For further discussion, see Woodward, this volume.

- Statistical associations and correlations across variables do not by themselves establish causation.

- Strength of association or values of regression coefficients do not allow us to estimate the probable strength of the hypothesized causal influence.

The point to be emphasized here is that the overall knowledge framework of quantitative social science is in principle compatible with the microfoundational approach to social causation. This is so for several reasons. First, much of the most compelling analysis of causation in quantitative social science supports the requirement of seeking out credible and testable hypotheses about the causal mechanisms that link the associated variables. So this observation takes us in the direction of causal realism. And second, the causal mechanisms that might link "gender and profession" to "lifetime salary earnings" must themselves be expressed through local circumstances impinging on individuals. What are the circumstances of education, business practice, and legal environment in the context of which the typical female is conveyed into an occupational trajectory resulting in a reduced lifetime earnings expectation? So, a causal explanation based on statistical findings requires a theory of the underlying causal mechanisms, and these causal mechanisms must themselves exercise their causal powers through influences on the socially situated individual actor.

What the microfoundational approach precludes is the assertion of explanations that begin and end with statistical generalizations about populations, and it precludes the idea that a statistical law is itself a causal factor in social outcomes. The law of gravity may be said to be a causal law; the association between gender and income is a result, not a cause—a phenomenal rather than a guiding regularity [Little, 1998, chap. 12].

If this microfoundational view is correct, then there is no such thing as autonomous social causation. There are no social causal mechanisms that do not supervene upon the structured choices and behavior of individuals.[24] The mechanisms through which social causation is mediated turn on the structured circumstances of choice of intentional agents, and nothing else. This is not equivalent to methodological individualism or reductionism because it admits that social arrangements and circumstances affect individual action. For it is entirely likely that a microfoundational account of the determinants of individual action will include reference to social relations, norms, structures, cognitive frameworks, etc. This means that social science research that sheds light on the individual-level mechanisms through which social phenomena emerge have a foundational place within the social sciences: rational choice theory, theory of institutions and organizations, public choice theory, analytical Marxism, or social psychology. What

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[24]Hedström and Swedberg endorse this position in their exposition of social mechanisms: "A corollary to this principle states that there exist no such things as "macro-level mechanisms"; macro-level entities or events are always linked to one another via combinations of situational mechanisms, action-formation mechanisms, and transformational mechanisms" [Hedström and Swedberg, 1998b, 24].

these fields have in common is a commitment to providing microfoundations for social explanations.

On the microfoundational approach, the causal capacities of social entities are to be explained in terms of the structuring of incentives and opportunities for agents. The causal powers or capacities of a social entity inhere in its power to affect individuals' behavior through incentives, preference-formation, belief-acquisition, or powers and opportunities. The micro-mechanism that conveys cause to effect is supplied by an account of the actions of agents with specific goals, beliefs, and powers. Social entities can exert their influence, then, in several possible ways.

- They can alter the incentives presented to individuals.

- They can alter the preferences of individuals.

- They can alter the beliefs of individuals. (constraints on knowledge; ideology)

- They can alter the powers or opportunities available to individuals.

Plausible examples of institutions, structures, or practices that have causal properties might include:

- Forms of labor organization: family farming, wage labor, co-operative labor

- Surplus extraction systems and property systems: taxation, interest, rent, corvée labor

- Institutions of village governance: elites, village councils

- Commercialization: exchange, markets, prices, subsistence cash crops, systems of transportation and communication

- Organized social violence: banditry, piracy, local militias

- Extra-local political organizations: court, military, taxation, law

In each instance it is straightforward to sketch out the sorts of microfoundations that would be needed in order to discern the causal powers of the institution: the direction of individual behavior within these arrangements and the aggregate patterns of social change that are likely to result. The result of this line of thought is that institutions have effects on individual behavior (incentives, constraints, indoctrination, preference formation), which in turn produce aggregate social outcomes.

Social causal ascriptions thus depend on common characteristics of agents (*e.g.* the central axioms of rational choice theory, or other theories of practical cognition and choice). I would assert, then, that the rock-bottom causal stories — the governing regularities for the social sciences — are stories about the characteristics of typical human agents within specific institutional settings. The causal

powers of a particular social institution — a conscription system, a revenue system, a system of democratic legislation — derive from the incentives, powers, and knowledge that these institutions provide for participants. Social entities thus possess causal powers in a derivative sense: they possess characteristics that affect individuals' behavior in simple, widespread ways. Given features of the common constitution and circumstances of individuals, such alterations at the social level produce regularities of behavior at the individual level that eventuate in new social circumstances.

Consider a few examples of plausible social-causal explanations. Transport systems have the causal capacity to influence patterns of settlement; settlements arise and grow at hubs of the transport system. Why so? It is not a brute fact, representing a bare correlation of the two factors. Instead, it is the understandable result of a fuller description of the way that commerce and settlement interact. Agents have an interest in settling in places where they can market and gain income. The transport system is the structure through which economic activity flows. Proximity to the transport system is economically desirable for agents. They can expect rising density of demand for their services and supply of the things they need. So when a new transport possibility emerges — extension of a rail line, steamer traffic farther up a river, or a new shipping technique that permits cheap transportation to offshore islands — we can expect a new pattern of settlement to emerge as well. Consider, for a second example, Robert Klitgaard's treatment of efforts to reduce corruption within the Philippine Bureau of Internal Revenue [Klitgaard, 1988]. The key to these reforms was implementation of better means of collecting information about corruption. This innovation had a substantial effect on the probability of detection of corrupt officials, which in turn had the effect of deterring corrupt practices. This institutional arrangement has the causal power to reduce corruption because it creates a set of incentives and powers in individuals that lead to anti-corruption behavior.[25]

An example of social explanation that illustrates the importance of disaggregating social processes onto underlying conjunctions of agency and structure, and the contingency of the social causal processes that result, is found in a large literature on the study of social movements. The literature on "political opportunity structures" emphasizes the contingency of mobilization of social movements depending on the array of opportunities that exist at a given time. Sidney Tarrow summarizes the approach in these terms: "Rather than focus on some supposedly universal cause of collective action, writers in this tradition examine political structures as incentives to the formation of social movements" [Tarrow, 1996, 41]. The openness to contingency characteristic of this approach parallels the approach to contentious politics offered in [McAdam, Tarrow, and Tilly, 2001].

Is there such a thing as "macro-macro" causation? Yes, but only as mediated through "micro-foundations" at the level of structured human agency. State institutions affect economic variables such as "levels of investment," "levels of

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[25]Similar examples of arguments about the logic of power relations in pre-modern societies may be found in Mann [1986].

unemployment," or "infant mortality rates". But large institutions only wield causal powers by changing the opportunities, incentives, powers, and constraints that confront agents. So the hard question is not "Do institutions and structures exercise autonomous and supra-individual causal primacy?", since we know that they do not. Instead, the question, is "To what extent and through what sorts of mechanisms do structures and institutions exert causal influence on individuals and other structures?"

Here, then, we can come to several conclusions. Social entities exercise causal powers through their capacity to affect the choices and behavior of the individuals who make up these entities, and through no other avenue. And social processes should be expected to demonstrate a significant level of contingency, path-dependency, and variability — give the multiple types of causal mechanisms, institutional variations, and features of individual agency that come together to bring about a given outcome.

5   METHODOLOGICAL LOCALISM

The strands of thought offered to this point have led us to a distinctive position on the question of the composition of the social world and the nature of social explanation — the questions of the "levels of the social" and "levels of social explanation". I refer to this emerging position as "methodological localism", in deliberate contrast to both methodological individualism and methodological holism. This position gives both ontological and explanatory primacy to the socially situated actor within a set of proximate social relationships. This position seems most compelling on ontological and explanatory grounds, and it is strongly supportive of research in many fields of the social sciences. According to methodological localism, the social is constituted by socially situated individuals, nested within social relations and institutions that have only an intermediate degree of persistence and permanence.   We disposed of the construct of the "pre-social" individual in the opening paragraphs of this article, and we disposed of the idea of autonomous structural causation through our discussion of the requirement of providing microfoundations for social explanations. What we are left with, then, is a social ontology that emphasizes contingency and impermanence in the social structures and institutions that exist at a certain time. And we are left with a theory of social causation that works through the idea of purposive human action within structured circumstances of choice. The circumstances that constrain human choice, finally, are themselves largely constituted by the social states of other individuals, who embody the characteristics, opportunities, and limitations of existing institutions.

Pervasive and common features of individual agency — common beliefs, ethical or normative motives, systems of norms, religious commitments, practical skills — are conveyed to the individual through specific local institutions and practices and embodied in the "practical cognitive" psychology of the individual. And it is an important challenge for social science research, including historical and

comparative research, to identify the specifics and the variations that define the institutions that transmit ideas, values, and practices.

This approach suggests an answer to the question of "levels of the social". It suggests that the lowest level of the social is the socially situated actor within a local set of embodied social relations. The socially situated individual finds herself within a concrete set of social relationships, networks, and institutions. This complex serves to socialize and provide incentives, as well as to constrain. The approach of methodological localism supports as well the reality that institutions often have extra-local scope, geographically, demographically, and administratively. So, we can legitimately describe institutions with broader scope as being "higher-level" institutions. Recognition of the requirement of microfoundations, however, requires us to pay attention to the mechanisms through which the distant actors within the higher-level institution are constrained and induced to play their institutionally-defined roles. And likewise, as we rise to higher levels of geographical and population scope, as well as administrative complexity, we can legitimately identify "global" or "world-systems" institutions — with exactly the same requirement that we be prepared to identify the institutional and material arrangements through which the actors who constitute the global institution are led to play their parts in the maintenance of the institution. (The imperfection of these mechanisms explains the plasticity of institutions over time, as opportunism permits the modification of institutional arrangements over time for the extra-institutional purposes of powerful actors.)

This approach suggests six large areas of focus for social science research:

"What makes the individual tick?" What makes individual agents behave as they do? Here we need accounts of the mechanisms of choice and action at the level of the individual. This area of research is purposively eclectic, including performative action, rational action, impulse, theories of the emotions, theories of the self, theories of identity. What are the main features of individual choice, motivation, reasoning, and preference? How do emotions, rational preferences, practical commitments, and other forms of agency influence the individual's deliberations and actions?

"How are individuals formed and constituted?" This approach gives deep importance to learning more about how individuals are formed and constituted. Here we need better accounts of social development, the acquisition of preferences, worldview, and moral frameworks, among the many other determinants of individual agency and action. What are the social institutions and influences through which individuals acquire norms, preferences, and ways of thinking? How do individuals develop cognitively, affectively, and socially?

"What are the institutional and organizational factors that motivate and constrain individuals' choices?" What are the systems of incentives and constraints that govern individuals' choices in particular settings? Institutions are systems of incentives and constraints, embodying formal and informal constraints.

They constitute systems of norms, embodied in the actions and expectations of others, which induce and enforce the institutional requirements.

"How do individual agents' actions aggregate to higher-level social patterns?" The social sciences offer a host of models demonstrating how individual agents' actions are aggregated to more collective levels. Here we need theories of institutions, markets, social mechanisms aggregating individual actions, microeconomics, game theory ... What patterns of social activity can be inferred as the aggregate consequence of deliberate human choice within the settings of the institutions that can be identified as the social context of action?

"What is the distribution of a given set of characteristics across a specified social population?" Individuals have properties, and populations have distributions of these properties. The task of quantitative social science, most broadly, is to measure and analyze the distributions and associations of variables across one or more populations or groups. It is a crucial part of social science research to conduct the studies that permit observation and measurement of some of the social characteristics that are of greatest interest to us, for the purpose of explanation, diagnosis, and policy.

"How do macro-level social structures influence other macro-level social structures?" The first several topics focus on micro- to macro-relationships (how features of individual agents have effects on the structures and institutions that they embody) and on macro- to micro-relationships (how embodied social structures affect the behavior and agency of the individuals who fall within them). There is also the question of macro- to macro-relationships; for example, how does a certain configuration of political institutions create effects in the educational institutions of a society? The perspective of methodological localism has implications for this category of question as well; it implies that we need to find the individual-level circumstances that convey the effects from the first configuration of macro-structures to the consequences at the level of the second set of macro-structures.

Social psychology and behavioral research are intended to shed light on the first two areas of inquiry. The new institutionalism, local ethnographies, and theories of organizational behavior shed light on the third question. And much of social science theory, devoted to the discovery of unexpected consequences, focuses on the fourth question: the results of prisoners' dilemmas and collective action problems, the aggregation of preferences represented by "micromotives and macrobehavior" analysis [Schelling, 1978], and much of economic theory and game theory. Quantitative social science focuses on the fifth type of question — what we might describe as the ultimate outcomes of the variety of social processes and individual choices described in the first several categories of question. And, significantly, the hypotheses about social causation that are suggested by the findings of quantitative research are best tested and supported by seeking out the causal mechanisms that can be uncovered through analysis of local circumstances, institutions, and the multiple factors that influence individual choices and lead to aggregate outcomes.

These six areas of focus combine to offer upward and downward social influence. Social institutions and facts influence agents directly, by constraining their choices; and indirectly, by influencing their cognitive and practical developmental process. And agents' actions in turn influence institutions and higher-level social structures, both by embodying the properties of those institutions at a given time (serving as a policeman or teacher, for example) and by opportunistically changing the workings of the institution (as a tax avoider, for example). And, strikingly, great portions of existing social science research can be accommodated within the scope of these research questions.

The approach of methodological localism gives rise to several cautions about assertions concerning higher-level structures and patterns. First, asserting facts about higher-level processes requires that we give an account of the "microfoundations" through which these processes come about. Second, socially situated individuals — individuals with social properties who exist in social relations and social institutions — are the "molecule" of social phenomena. This entails that all social entities (structures, institutions, rules, worldviews, etc.) supervene upon individuals. Third, social institutions and structures are plastic over time and space, given that they are maintained and modified by independent agents. This is, in fact, an important insight into the nature of the social. The complexity and looseness of the relation between levels that we find in human affairs is intrinsic to the nature of social life. Fourth, some degree of stability inheres in institutions as a result of the fact that individuals are to some extent "interchangeable". There is "multiple realizability" in the achievement of institutional effects. Fifth, higher-level entities exercise causal properties solely through the individuals who constitute them at a given time. And finally, it is an important social fact that individuals are in turn constrained by the (supervening) institutions within which they exist — and which they constitute. Macro entities exercise causal properties through the individuals who constitute them at a given time. This is a "social" fact, in that individuals are constrained by the (supervening) institutions within which they exist.

6   CONCLUSIONS: LEVELS AND LAYERS WITHIN THE SOCIAL

Several central points have emerged from this discussion. First, it is scientifically important for social scientists to arrive at a more adequate understanding of the social ontology that underlies their work, and such an ontology can be reasonably simple. The socially constituted agent within a set of social relations and institutions provides us a rich basis for characterizing social phenomena, and permits us to hypothesize higher-level structures and institutions as well. This approach to social ontology focuses on the level of the socially situated individual. Individuals exist; specific institutional arrangements exist; and specific ensembles of institutions exist. Here we proceed from the existential circumstances of human social life: agency, need, social relations, knowledge, power, worldview, and local opportunism.

Second, higher-level social structures exist, but they have their properties solely in virtue of the specific practices, rules, and arrangements that constitute them at a time and within a group of people. Higher-level structures are composed of the individuals, networks, and sub-institutions that coordinate and constrain the actions of persons throughout the scope of the social structure. The social sciences therefore need to exercise caution against "reification" of abstract structures. Social entities supervene upon individuals; they have no independent existence. Institutions and organizations exist at a range of levels, from the local to the global; but they are embodied in the same way, regardless of scale or scope. Individuals occupy positions of service and decision-making: they have instruments of persuasion, communication, and coercion; they are subject to incentives, opportunities, and penalties; and they act in ways that are sometimes coordinated and sometimes self-serving.

Third, macro-social entities exercise causal properties through the individuals who constitute them at a given time. Social entities convey causal properties through their effects, direct and indirect, on individuals and agency. Individuals act according to a set of beliefs, values, and preferences that have been socially constructed, and their actions in turn preserve or modify the institutions and norms within the context of which they live and act. This amounts to recognizing that there is a requirement of "reduction" within the social sciences, but this requirement is not onerous. It is the "microfoundations" or the "social mechanism" requirement. Explanations that refer to the effects of social structures must be accompanied with a schematic account of the mechanisms through which they bring about the putative effects at the level of locally-situated individual behavior.

Fourth, social structures and institutions are plastic over time and space. We need to exercise great caution in postulating high-level abstract structures that recur across instances, *e.g.*, state, mode of production, protestant ethic, Islam. Social institutions, structures, and practices "morph" over time in response to opportunism and power by the participants. It is therefore an important area of social and historical research to document the variations of structures, institutions, practices, and systems of values and belief across space and time, and to identify the social mechanisms through which these differences are caused. But symmetrically, social entities persist beyond the particular individuals who make them up at a given time, because of identifiable processes of social reproduction. Social structures, institutions, and practices have a surprising degree of stability and "stickiness" over generations. So we need to be able to offer an account of some of the social mechanisms through which this stability over time and generational cohort is achieved.

Fifth, it is highly significant that each of the major research frameworks in the social sciences finds a place within this treatment of methodological localism. Comparative research provides many of the tools necessary for probing the nature of institutional settings that define the social context of agency. Qualitative research sheds significant light on the nature of the "socially constituted actor". And quantitative research allows the social sciences to describe and analyze the social

patterns that result from these various kinds of social mechanism and individual agency. A preference for a single research methodology ought not drive the agenda of social science research. Instead, researchers should adapt their methodologies to the specific demands of the research questions they want to investigate and the complex social constructions that they would like to better understand. And this means that the approaches of qualitative, comparative, and quantitative social science are complementary to one another rather than incompatible alternatives.

Finally, I maintain that much existing social science research and theory is already consistent with "methodological localism". Researchers and theorists in many of the areas of the social sciences can be understood to be providing insight into one or another of the "nexuses" presented by the socially-situated individual. Virtually all the examples I can think of from the social sciences can be recast in these terms. Differences in disciplines are, in large part, the result of choices about which locus within the "socially situated individual" ontology to focus: development, action, relationship, network, or institution. Moreover, when theories deviate from this conception, they are all too often fall into fallacious thinking: reification, essentialism, functionalism, teleological thinking, blind structuralism, social "action at a distance", and methodological purism.

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RATIONAL CHOICE

Alessandro Pizzorno

1 RATIONAL CHOICE THEORY AMONG OTHER THEORIES OF SOCIAL ACTION

Rational choice theory (RCT) can be considered as a more or less formal application of the idea of instrumental rationality — that is, of the widely held idea in which an action is considered rational when, given certain ends, and given that he possesses determined beliefs and means, the subject chooses the most suitable means for achieving his ends. In this sense RCT appears to be an application of economic logic to all social actions. At first glance then, it could be seen as the absorption of the idea of *homo sociologicus* into that of *homo economicus. Homo economicus* is thought to be an active subject, not a passive one being governed wholly by impersonal forces beyond his control, in the way that *Homo sociologicus* is often perceived to be. One perspective sees man in a rational light, while the other thrusts upon him a causal model. The struggle between these two paradigms can be neatly summarized as "rationality versus causality" [Bhargava, 1992]. Yet in reality this distinction between the subject of action as studied by economists, and that studied by sociologists, despite it being referred to in many authors' work, remains generic and thus highly misleading.

A slightly different and more research-oriented typology has been proposed by Siegwart Lindenberg, who distinguishes between three types of theories [Lindenberg, 1996]. The first type is traditional sociological theories, that are "dominated by the idea that social facts, be they institutions (like language or school system) or structures (like social classes) impose themselves on the individual.... the vehicles of imposition are role expectations and processes of socialization in which the individual learns to want to conform to these expectations" [1996, 148–9]. The second type is "choice-centered theories" (such as RCT), in which the main task is to show that the theory can be applied to a certain phenomenon, which may already have been considered by different theories, rather than to advance knowledge in an unexplored field. Here the aim is to map the influence of changing constraints on behavior, rather than to specify the nature of the expected utility. The third type is "subject-centered" theories, where the central focus is the subject matter itself, embedded in an ongoing field of inquiry. This type of theory seeks to establish how constraints on action have shaped the outcome of a process, thus pre-supposing a range of potential outcomes, the actual outcome being determined by the complex inter-actions between choice and constraint.

But if we want to situate RCT among other more comprehensive theories of social action, we should devise a typology which can effectively distinguish between theories that are intentionalist and outcome-limited on the one hand, and those that are contextualist and reception-oriented on the other.

The intentionalist type considers that the explanation of actions is reason-oriented. Propositional attitudes, i.e. propositions defining beliefs and desires of the agent, are deemed to establish the cause of the action. Rationality is predicated on intentionality. Agents are considered to be perfectly transparent to observers (or interpreters), and the language of the observer is perfectly transparent to the audience asking for an explanation. This is the typical case of economic science. The meaning of an action is defined by the intention of the agent, and is reflected in the outcome. Intentions are therefore considered to be internal states of mind, which occur prior to expressing themselves in behavior. The agent must always be aware of his intention and, if asked, would claim them as his own. The process of preference formation preceding the choice, as well as the chain of outcomes that follow the action, are not taken into consideration by this model. It is less a theory of social action than a theory of decision, intended to explain situations where an agent's choice does not affect, nor is affected by, that of other individuals.

On the other side, we have theories of social action which define the meaning of an act not through reference to the intentions of the agent, but through the process by which this act comes to be understood by the participants in the social context where it takes place. In other words, the meaning of an action is what those to whom the action is directed at understand it to be. The observer-interpreter who needs to explain the action should therefore organize his interpretation on the basis of a reconstruction of the understanding which was formed in the social context. Rationality does not bear on the intention of an agent, but rather on the reception of the action among the participants. Think of a religious rite. Participation in it can be considered rational not because the agent intends to maximize some utility through this behavior, but because the action takes place in a situation where participants understand certain acts as being reasonable, predictable and meaningful in the light of shared beliefs. Or take artistic work. Its understanding does not follow from establishing the intention of the artist in creating it, but by the variable determination of its meaning by its audiences. Here rationality is the expression of the competence of an agent in testifying or in creating the meaning.

2 INSTRUMENTAL RATIONALITY

David Hume was the first important modern philosopher to state that the task of reason is to evaluate the means necessary for reaching some given end [Hume, 1951]. He excluded every rational judgement concerning ends, observing that when we come to ends, reason should be silent. "Reason is, and ought to be the slave of the passion, can never pretend to any other office than to serve and obey them" [1951, 415]. This definition went on to become common in both everyday language and in the vocabularies of those disciplines dealing with theories

of social action. The "ends" would receive various names other than the Humean term of "passions"; like "interests," "utility" or "preferences." But it became commonly accepted that when considering the judgement of rationality, it was essentially means, and the information and the beliefs when using them, to which theories referred. Since, however, limiting the idea of rationality to the choice of means posed several difficulties, theoretical investigation then turned to the task of determining the foundations of other, more comprehensive conceptions of rationality. After reviewing the doubts and objections raised by the use of the instrumental concept of rationality, I shall examine other possible ways of defining the rationality of a social action. I shall mention "procedural rationality," which refers to subjects considered to be acting rationally insofar as they follow prescribed rules. Finally, I shall assess whether it is possible to ascribe the notion of rationality to the ends pursued by subjects of action, and not merely to the means chosen, therefore abandoning the path so strictly defined by Hume, examining instead the type of rationality that is sometimes called "axiologic" (inspired by values), sometimes "expressive," or "cognitive," but which I shall in this text call "identity-oriented rationality."

3   RATIONAL CHOICE THEORY AS A NORMATIVE THEORY

Rational Choice Theory (RCT), in its instrumental guise, presents itself first and foremost as a *normative* theory aiming to indicate which means to choose (i.e. which means in a given situation may be considered rational) in order to reach certain ends. In other words, RCT assumes that a rational agent expects certain consequences to flow from his actions; calculates the probability of the occurrence of these consequences; assesses these consequences in terms of their utility on the basis of his initial preferences; and chooses the action which will provide him with the highest expected utility. The notion of maximisation (of some magnitude) is the central assumption of this theory.

RCT, however, is also a theory with *descriptive* ambitions. In this case, the observer using the theory should assume that the actions he seeks to explain may be attributed to rational agents, who given certain beliefs, will choose the best means for reaching their end. "A feature of such a theory" — as Donald Davidson so pithily puts it — "is that what it is designed to explain — ordinal preferences or choice among options — is relatively open to observation, while the explanatory mechanism, which involved degree of beliefs and cardinal values, is not taken to be observable. The evident problem is that what is known (ordinal or simple preferences) is the result of two unknowns: degree of belief and relative strength of preference" [2004, 153]. Thus, in a descriptive use of RCT, the observer who intends to explain the behaviour of an agent should be able to acquire knowledge of a) the preferences the agent wants to satisfy; and b) the beliefs the agent holds about the best means to employ in order to obtain them.

This descriptive structure of RCT takes the form of a proposition [L] familiar in folk-psychology [Rosenberg, 1988]. The proposition runs as follows: [L] given

any person X, if X desires D, and believes that A is a way of getting D under the given circumstances, then X will perform A. However, [L] implies a strong *ceteris paribus* clause. To take a typical example, let us consider the micro-economic theory of consumption. This theory assumes perfect information on available choices and their consequences, the restrictions under which the consumer acts, and in addition to all this, a running order of the preferences that lead the consumer to one particular choice.

Let us now suppose that the problem of how X's preferences are formed may be overlooked, and instead seek to explain the course of action by applying [L]. Firstly, we must determine the initial conditions of our explanation. In other words we must establish which beliefs and desires lead X to perform an action. A simple empirical observation of the action in question is not sufficient to enlighten us on this matter. A first hand physical description of a fact can often leave us in doubt as to what we are dealing with. We see a person running: is she late or is she jogging? We see a person's eye twitch: is she blinking or is she winking? We see one person hit another: is it a punishment or are they playing?

To discover the intentions behind these actions, we may ask, experiment or observe. Will this be enough to allow us to reconstruct the intentions behind the action? Hume says: "Ask a man why he exercises: he will answer, because he desires to keep his health." It is not enough. We should also try to discover what beliefs the agent holds as to the efficiency of the means chosen in order to obtain his desire. Once this information has been obtained, however, [L] as it stands is no longer useful. We would in fact be left with a theory of action that is no longer falsifiable. "Whenever a person does something that looks utterly irrational, given the beliefs and desires we have attributed to him, the reasonable thing to do is to change our estimate of his beliefs and desires" [Rosenberg, 1988, 46]. Following this logic, all types of action appear to be uniformly instrumentally rational.

However, it is not so much the impossibility of falsifying the propositions arising from this structure that renders them useless, as the practical difficulties that arise in applying the necessary auxiliary hypotheses. When we use [L] in our everyday relations, we do so because we are able to add such auxiliary hypotheses. In other words we are able to make plausible conjectures concerning the beliefs and intentions of the people we observe, thus providing common sense explanations about the nature of their behavior and the kind of advantage they seek. We are able to do this because each time we engage in this process we put local theories into operation. These allow us to reconstruct the desires and beliefs of the participants because we know them well, or because we know what role they occupy, or because we have other local common sense hypotheses concerning their usual way of behaving. All things considered, this is sufficient for us to imagine that we are to deal with some minimal degree of rationality. These are the cases in which we accept the validity of Friedrich Hayek's comment: "to recognize something as a mind is to recognize it as something similar to our own mind" [1952, 135]. If all our relations were limited to such exchanges, we would need to delve no further. But we cannot assume that the observer, much less the audience to whom the

observer must relate his interpretation of the action, are in a situation in which it is possible to employ the "concurrence of minds" hypothesis.

The extreme consequences of a position which maintains that a rational explanation of action should be limited to a normative reconstruction are clearly reached in Davidson [2004]. His points are the following: rationality is a central, and irreplaceable, feature of the intentional. Explanations using reasons (reason-explanation) are explanations in terms of propositional attitudes. A belief plus a desire can explain an action "only if there is something desirable about the action to be explained" [2004, 151]. Therefore normativeness appears as a primitive aspect of rationality. It follows that a descriptive explanation in terms of rational choice is only possible when the audience is composed of persons assumed to reason in the same way as the agent, be they participants in the same social situation or not. This also explains why the most innovative analysis with an RCT orientation deals with situations where the nature of the preference of the agents is manifest, such as in the case of political elites [Tsebelis, 1990].

4 CRITIQUES OF RCT

RCT has undergone different kinds of critique. These range from critiques concerning its philosophical conception of morality, to critiques of methodological import. RCT has been accused of moral scepticism, to the extent that only means-ends relationships are considered to constitute social action, and in that the theory is concerned only with means, not with ends. It would follow that instrumental rationality is not capable of explaining the morality of a social system. The fact that deviance is a rather rare phenomenon indicates that there should be some internal orientation towards moral behaviour. But a theory purporting to limit itself to the efficacy of means seems unable to detect the origin of the observance of moral norms (see Section **7**).

What lies behind these critiques is the awareness that it is not sufficient to link the rationality of an action to the intention of the agent, without referring to the socio-cultural system from which an action receives its meaning. The meaning of an action should instead be linked to the social system within which it takes place. It is indeed easy to demonstrate that a theory based on instrumental rationality is not qualified to explain the activity of producing public goods (as is the case when an individual action must be connected to a specific social system). Nor is it qualified to explain "weakness of the will" (as is the case when an individual act should be connected to the inter-temporal aspect of the system of the person). Nor does instrumental rationality seem qualified to secure the transparency of the agent to his interpreters (and this is needed to place individual acts within a system of interpersonal relations).

4.1

Most damaging is the observation that no form of instrumentally orientated action can account for the formation of public goods. Theories of democracy, like those of Downs [1957], indicated how voting cannot be considered rational behaviour (given the unlikelihood that a single vote may have any discernible effect on the outcome of an election). Mancur Olson's collective action theory also showed that the formation of groups, associations, or movements could not be explained instrumentally, that is, by deriving the goals of the individual participant from the goals of the group, especially when his participation is not decisive in attaining the outcome. To explain participation, a different theory is needed. A rigorous demonstration of the impossibility of an individualistic instrumental production of public goods was reached in the game theoretical description of one-shot Prisoners' Dilemma [Olson, 1965]. An attempt to find a solution to this dilemma makes recourse to the so-called Prisoners' Dilemma Supergame, when the game is played for a certain number of shots. It seems verified that in this game a tacit accord emerges among the players, who end up with a cooperative solution [Axelrod, 1984; Taylor, 1987]. No theory seems to explain this empirical result. A possible theory would obviously need to refer to notions of trust, solidarity, or concern the individual has for long term ends — notions which are at odds with a theory of instrumental rationality. Similarly, in his attempt to find a partial solution to the problem of collective action, Elster mentions situations like the presence of an appropriate number of participants; their enjoyment when they feel to be amongst others; and other notions extrinsic to the individualistic model [Elster, 1986].

An element seems common to all these proposals to overcome the limits of reason-explanation based on instrumental rationality: awareness that the meaning of the choice cannot simply be derived from propositional attitudes of the agent, such as desire and beliefs, and that a new component seems needed in a theory of action.

4.2

Akrasia, or weakness of the will, is traditionally seen as a form of conflict within the self. It is the struggle either of passions against reason or of short term preferences against some long term interest of the person. In slightly more general terms, Davidson defines the standard case of akrasia as one in which the agent knows what he is doing, knows that it is not for the best, and knows why [Davidson, 2004]. He continues by saying that in such cases, the agent acknowledges his own irrationality. According to Davidson, the intentionalist position states that the intention of an action is its cause, so that all mental acts require reason-explanation. Yet if actions are caused by intentions, how is it possible that there are actions unexplainable by reasons? Davidson's solution is to the effect that in these cases the mind must be considered "partitioned." "Indeed, if we are going to explain rationality at all, it seems that we must assume that the mind can be partitioned into quasi-independent structures that interact. The idea is that if

parts of the mind are to some degree independent, we can understand how they are able to harbour inconsistencies" [2004, 181]. In other words, while certain acts are such that they can be explained by reasons, other acts are effects of one part of the mind acting as an external agent on another part, so that the latter behaves not for reasons, but as if under external constraint. Thus the principle of the causal function of intention is sustained. This explanation is not merely a sophisticated re-statement (circumscribed to cases of weakness of the will) of the Humean idea that reason is the slave of passions. Davidson underlines that the Humean idea of the battle between reason and passion implies that the conflict takes place within the same division of the mind. This would not only constitute a logical contradiction, but would also imply that the principle by which all action must be explained by reason, and therefore understood by the agent himself, would thereby be lost.

4.3

As for the explanation in terms of a conflict between the present and the future self, we have to imagine that some idea of a long term life plan exists in the mind of the agent, and that in certain circumstances the agent finds it difficult to sacrifice some short term desire that he knows to go against the precept of that plan. This would make it possible to avoid an analysis which is merely framed in terms of introspection. Indeed, a socially scientific explanation needs observation in order to decide about the existence of a state of affairs, and no observation allows us to verify whether some conflict exists within the mind between passions and reason; or between short term desires and long term goals. The only workable assumption is that the mind of the observer works in the same way as the mind of the agent. But if we have knowledge of an agent's declarations about the existence of a life plan, or if we observe repentant behaviour by the agent following his choice, then we can deduce that the agent imagines himself as being composed by two persons. One being the self whose activity is that of "judging", the other is the self whose activity is that of "choosing": So that if the agent desires to take the drug, smoke the cigarette, eat fattening food (or indulge in other short term fancies), he will act accordingly, but then later repent. In this view, we are then led to design not only two separate selves (or one self which appears partitioned), but a self occupied with the activity of *choosing*, and another directed to the activity of *judging* the choices of the first. As the definition of akrasia says: "an agent's will is weak if he intentionally acts counter to his best judgement" [Davidson, 2004, 201]. The notion of judgement is central here when 'judging' and 'choosing' are in conflict. Both parts of the mind respond to reasons, but one, in certain circumstances, is weaker than the other (See Section **11**: Conclusions).

4.4

We have mentioned that RCT clouds the transparency of the agent to the interpreter, both when the latter is a participant in a situation, and when he is a scientific observer. The circumstance that the model excludes from consideration, the process of preference formation, leaves the interpreter with the task of collecting data about the imaginable preferences which governed the intention of the agent. This seems an easy task when the knowledge of the agent is secured by familiarity with languages and cultures. But as soon as the intention of the other person appears opaque, the problem of a (radical) interpretation arises.

4.5

Consumer theory in economics adopts the Samuelson theory of 'revealed preferences' [1948], where *a posteriori* knowledge of the already expressed preferences suffices. It is a theory, however, which has often been criticised. Notable among these critics is Amartya Sen, who maintains that in order to be able to interpret an individual's preference ordering, one must know what those objects mean for the individual concerned [Sen, 1982]. In the most recent "mainstream" reply to these critiques, Dowding acknowledges (even by refuting certain analytical passages of Sen's demonstration) that understanding choice requires the attribution of some specific identity to the agent; and that it is the fact that we are describing a politician, or a bureaucrat, or a voter, that allows us to use "an intuitive explanation of the sort of preferences assumed for the player" [Dowding, 2002, 280]. This is an important concession, because it implies that the identification of the social position of the agent-consumer, and some knowledge of the nature of that position, is needed to establish the rationality of the choice. In any case, it is also generally admitted that "revealed preference theory" can only be used, if ever, for the analysis of mass behaviour affecting the movement of goods, services, people and prices, and not for the explanation of individual cases.

The situation is more difficult if the observer is asked to proceed to a more radical interpretation, and wants to understand an agent on the basis of evidence that does not presuppose any detailed knowledge of his thought or of the meaning of his words. That is, when the environment within which the agent acts is in large part ignored by the observer or by the participants. The solution that philosophers like Quine [1953] and Davidson [1984] propose is to apply a "principle of charity" (see also [Ludwig, 2004]). In other words, the operation of understanding an agent should always aim at demonstrating the potential rationality of his action, but this should be made possible only by reconstructing the context (or social situation) within which the actions took place.

5   THE PHILOSOPHICAL CONSTRAINTS OF METHODOLOGICAL INDIVIDUALISM

5.1

The main difficulties of RCT are due to the philosophical background on which it is based, namely methodological individualism (MI). There exists a scholarly debate as to whether or not the roots of RCT lie in the philosophical mediaeval school of nominalism. Popper and Mises (even with some qualifications), and many others with them, tend to accept this derivation. The school of nominalism maintained that only individuals exist, and that *universal*, or collective concepts, are only constructs of the individual mind [Udehn, 2001]. However, nominalists were interested in answering ontological questions about the reality of things, not to propose a method for the social sciences. Social sciences cannot pretend to give ontological judgments, but only an analysis of the nature of the relationship between social events and social situations. Moreover, the standard contemporary view of the proceeding of scientific theories, the so-called "holistic view" [Quine, 1953], is considered valid for both natural and social sciences, and implies that we cannot use pre-theoretical concepts. Individual as well as collective entities can both be used if a theory requires them. As such, both can be considered constructs of the mind of the theorist. This makes Hayek's saying to the effect that "the wholes as such are never given to our observation, but are without exception constructions of our minds" [1953], true but irrelevant, because the same can be said of individual entities. The circumstances in which one sees individuals gesturing does not provide any evidence in itself if there are no names with which to define those gestures. Names are something that only a theory can furnish. In other words, one can use both the concept of the individual, and the concept of collective bodies, according to the requirement of the theory needed to explain certain events. Moreover, one can say that a social science audience is probably not interested in establishing whether or not individuals as such exist, and it is sufficient for them to establish that it is the juridical order that executes the criminal, and not the hangman. Besides, the same Mises (author of the former caricature), in another passage observes that "every form of society is operative in the action of individuals aiming at definite ends. What would a German national character be that did not find expression in the Germanism of the individuals" (Mises, quoted in [Udehn, 2001, 109]). This statement implies that there is a difference between German individuals and French (or other) ones. How does one explain difference if not as a consequence of the fact that the presence of diverse collective bodies (or systems of roles) make individuals, in certain collectivities, behave differently to others? The very statement that Germanism exists (but not "Germany"), or that Germany is formed by a collection of individuals, forgets not only that individuals in Germany change at every moment (while "Germany," in our conception, does not change accordingly), but that every proposition in which we use the term "Germany" cannot have the same meaning if replaced by

"German individuals." If we call those individuals "German citizens," this would refer not to a set of individuals considered in their totality, but to roles defined by their membership to a collective entity.

5.2

In other words, a collective entity as such is not made by individuals, but by a set of relationships between social positions that this or that individual may alternatively fill. What makes collective entities different from one another is the nature of the relationships which link those social positions. It follows that the object of the social sciences is neither the observation of individuals as such, nor of some collective entity, but those situations in which certain subjects of action (which can be natural or juridical persons) enter into a relationship such that the outcome is amenable to receiving a meaning that is understood by the participants in that situation. By ascertaining the meanings that the participants mutually give to their actions, the observer will then become capable to offer an interpretation of that situation in terms which his audience will understand.

Indeed, if we take the etymological significance of the word 'individual' as being an *indivisible* entity, we see that what social sciences may call "individual" is actually formed by a set of very *divisible* entities. Subjects of action exist to the extent that they are capable of specific social actions, that is, to the extent that they are part of a situation which, directly or indirectly, defines the meaning of their action. As Weber puts it, when someone opens an umbrella when it rains, he takes a rational decision, but his action is not a *social action*. But if he opens his umbrella to cover another person, that act will be considered *social*. From the beginning of his existence, what is ontologically known as a human individual is in fact defined by his name and surname, or by other labels intended to situate him in a social setting. He is in turn defined by his actions in specific relationships with other subjects of action. The child is defined by his relationship with his parents; a student by that with his teacher; a friend by his friends; a lover by his lover; an employee by his organisation, and so on. In each of these situations, the individual will only partially (not totally) use his entitlement as a subject of action, i.e. his capacity of participating in a social situation.

6   THE OPENING OF THE BLACK BOX

One of the main, and most insistent, tenets of methodological individualism (MI) is the need to penetrate what is called "the black box." This expression refers critically to those types of theories that are incapable of performing the opening of the box, like functionalism, certain types of historicism, certain aspects of Marxian methodology, quantitative correlation analysis, and similar approaches. These theories appear to explain actions by causes which are external to the subject of action, as if assuming that these subjects receive impulses existing in a context around them, and then they pass directly to action. By so doing, these theories

ignore the black box, which is located between the impulses and the external action, and where the intention to act manifests itself and constitutes the very cause of the action. The program of MI, however, is not clear about the nature of what could be found in that black box. Is it an act explainable by reasons, or may it also be explained by irrational impulses? Even with Popper, the black box could be filled by individual intentions, but these will necessarily depend on relations with social groups since, as a matter of logic, thoughts or intentions are dependent on the existence of social groups. Moreover, why stop the reduction at mental concepts of an individual, and not descend to his genetic structure?

6.1   The Historical Significance of MI

According to Mises, the development of MI, which supplanted the older concept of *realism* or *universalism,* can be considered the Copernican revolution of the social sciences [Udehn, 2001]. Today it would be difficult to agree with this sweeping and emphatic judgement.

   The fortune of MI must rather be understood as a counter reaction to a succession of philosophical schools that in the 19th century tried to confront changes in western society, and proposed to furnish a theoretical basis both for a general view of society, and for the new disciplines that wanted to study society empirically. The emergence of representative regimes, of individual rights, of absolute rights of private property, the speed of urbanisation, and of the market oriented society, required an understanding of how the individual, seen as the protagonist of all those changes, could be understood as the builder of the new institutional structures. Political economy, and the more radically successive neo-classical school of marginalism, tried to capture this new vision of society.

   But these disciplines chose to ignore too many social phenomena. The growing importance of national communities, the reconstitution (even within urban society) of groups, associations, political parties and new forms of interpersonal relations, the changing but lasting centrality of the family — all these were new phenomena that needed specific theoretical concepts to be described and explained. Sociology and other specialised sciences, plus other new forms of historical methods, organised as new scientific disciplines, attempted to deal with all these phenomena. Historicism, mainly of German influence, was interested in the phenomenon of the macro-analysis of historical change. Hermeneutics (starting with Friedrich Schleiermacher) tried to introduce both subjective and psychological interpretations of historical cases, and linguistic interpretations of interpersonal relations [Udehn, 2001]. For this new way of examining social situations, a theory of individual action was not of central interest. Individual action was meaningful only when situated within historical contexts. The analysis of the rationality of the singular action was not confronted. Even Marxism, in spite of its economic foundations, aimed only secondarily to propose a social theory, and meant rather to give a general theory of the phases of social change, where the individual had only a secondary role.

On the other side, the new discipline of sociology oriented its interest rather towards family relations, communities, and other specific types of organisation. Small group analysis, interpersonal relations, the distribution of a population in terms of social stratification, and the social paths and strategies for social mobility, were at the heart of these disciplines. The idea of dividing the analysis of the human person according to two conceptions of man, one oriented to the study of *homo economicus*, and the other oriented to the study of *homo sociologicus*, emerged out of this situation. Both schools had imperialistic ambitions [Radnizky and Bernholz, 1987]. The followers of the first position were mainly the economists, but also in certain respects the Marxists. Forms of special disciplines were proposed, like praxeology (from Mises) to work towards the economic interpretation of traditionally non-economic situations. Thus the army supporting the *homo economicus* tried to control the old territory of the social sciences.

Sociology on the other hand (plus anthropology and other similar disciplines of the social sciences) maintained that a theory of action should be embedded in some general theory of the social system. But the difficulty of reaching an accord on the validity of a unique conception of the social system led to the fragmentation of several fields of research.

7   NEO-INSTITUTIONALISM AS AN ORGANIC RESPONSE TO RCT

According to North [1990], the real objection to RCT is the neglect of the role of institutions: "once one introduces institutions into the model a necessary corollary is a recognition that instrumental rationality is not the correct behavioral assumption" [1990, 358]. Indeed, among the theories opposed to RCT, neo-institutionalism (NI) has revealed itself as the most coherent and the one most able in aggregating scholarly activity. Its criticism of RCT gathers most of those previously directed against it. Collecting the essential points here will serve to better delineate RCT-specific positions in the current methodological debate. I will examine three clusters of problems.

Where the theory of individualism begins from the idea that individuals enter into social life with their preferences already formed, and in acting out those individual preferences they constitute the institutions of society, NI assumes instead that to discover the origin of the formation of preferences we should look at institutions first. In other words, NI assumes that individuals may enter into relations with one another only on the basis of the positions they occupy in institutions. The agent is not seen as entering the model in possession of his preferences already formed exogenously, but as the player of one or more roles corresponding to the positions he covers in the institutions. He is thus oriented to follow interests arising from these roles. The question, in everyday English, posed to a person in order to understand the meaning of one of his decisions "In what capacity are you doing or saying this?" is an expression which precisely reflects the idea that a choice is a function of a person's interpretation of the role (capacity) he is occupying.

Since the roles a person fill are numerous, and the choices that express the

wishes of one or another role can be incompatible, there are situations where a person must choose one path or another, or build bridges upon which the different wishes may become compatible. When the roles assigned by the family, political career, productive activity, or group of friends inspires conflicting choices, then from the normative point of view the subject of action should seek to follow some form of meta-preferences; or as others call it, a "life plan" [Frankfurt, 1971].

These inconsistencies, we should note, occur not only among possible choices when desires arise simultaneously, but also, as we have seen, when immediate desires contradict desires concerning how the agent would want the outcome of his current choice to be judged in a future time. As a consequence of these assumptions, the formation of preferences should be considered endogenous as well as exogenous to the model, because people can change their preferences during the development of the reciprocal relationships into which they enter. In addition, the orientation of the person to the action is not considered as necessarily based on the utility the relation may yield, but indeed to the completion of a requirement (duty) that marks the role the person is filling. It may be that the choice of the role upon whose basis those choices are organised was originally inspired by consideration of the utility of the person in the long term, but this evidently does not count for the specific series of choices of the incumbent of a role. To take account of these kinds of choices we should resort to a different notion, which NI calls "appropriateness" [March and Olsen, 1989].

We may indeed ask ourselves if, with the concept of the person who derives the reasons for his actions from the roles he occupies in the social structure, we are not heading for an "oversocialized conception of man" [Wrong, 1961]. In this case, the consequence would be a diminution of the ideal of the person's dignity, and of the sovereignty that we imagine the individual, as a unitary whole, must exercise over himself and his choices. More specifically, it appears that in this way we are questioning the concepts of individual rights and responsibilities. In reality, individual rights as we know them affect individuals that occupy precise roles (such as that of citizen), and individual responsibility belongs to an individual insofar as he acts within an institution (organisational loyalty) or insofar as he enters into a system of interpersonal relations. Moreover, the relationship between the structure of a self and its capacity to perform autonomous action depends on his playing among the different identities that are recognised to the individual by the multiplicity of his belonging. In other words, an individual may refuse to accept the obligation of a particular role when his position in other roles allows him the possibility of grounding his autonomy on the strength of other recognitions. This may take the form of other circles to which he currently belongs, or to which he belonged in the past, or to which he desires to belong in the future. Autonomy does not come to an individual by birth, but by the multiplicity of relationships with others, thus allowing him the possibility to refer to more than one circle of recognition.

All this implies that a theory of social action cannot be limited to the notions defined by the trajectory: preference — choice — utility maximisation. Real choices

are linked together in a combination of actions that, inside an institution, continue to have effects. The course of action will be altered, and so initial preferences may be reversed. In this sense, the old institutionalist theory already conceived of consumption as a form of social communication [Heap, 1989]. In this case too it is indicated that the choice of a consumer does not necessarily have to ignore the choices of other consumers, as the principles of market equilibrium would require. Moreover, choices contain a temporal dimension, in the sense that the subject who makes a choice he considers rational is inevitably obliged to take account of the preferences he himself will hold at the moment the outcome is obtained. As March and Olsen put it: "Since the consequences of interest are to be realized in the future, it is not necessary to anticipate what will happen, but how the decision maker will feel about those outcomes when they are experienced" [1989, 7]. We can sum up by stating that the institutional critique of RCT converges with other critiques in emphasising its exclusion of consideration of time, and the judgement of others, in defining the rationality of choice.

8 THE ALTERNATIVE CHOICE — NORMS

A similar conclusion can be found if we analyse the core concept which is behind the position of neo-institutionalism, the concept of a "norm." Norms can enter a theory of rationality in two major ways. a) They can act as a form of constraint (such as when they are sanctioned by the use of force). From the point of view of individual choice, this fact does not present logical difficulties. Norms can be a type of constraint like others which shape the opportunity set of the agent. The difficulty is rather in considering the formation of this type of constraint at the societal level, and of the sanctions that it implies. Sanctions are a form of non-excludable public good, and therefore they represent a cost that it is irrational for an individual to incur, since the advantages of living in a society guaranteed by good norms can be enjoyed even by someone not participating in the production of the good itself. As with all forms of production of public goods, RCT is not able to give an explanation of how this could take place [Buchanan, 1985]. b) They can be preference forming. The concept of the internalisation of values, of feelings of shame by the violator of a norm, and other forms of internal consequences, refer to the possibility that the observance of norms, and therefore the preference for behaving morally, derive from a situation in which the agent enters into a relationship with others which is aimed at defining his identity. As Parsons puts it "to act in conformity with a norm becomes a need-disposition in the actor's own personality structure" [1951, 37]. But since norms are being internalised as a consequence of some anticipated judgement of our behaviour by other persons (real or imagined), and of the sanctions which can be expected, the preferences thus formed imply that the agent considers that the outcomes of his choices have a value which depends on the judgement of a circle of others. This is just another way to confirm that the process of choice needs to be understood by including some judgement by others.

   A similar conclusion is reached when norms are seen operating in a situation "when the socially defined right to control the action is not held by the actor but by others" [Coleman, 1990, 243]. This may be rephrased as indicating that when a person chooses, he has in mind the judgement that another person will give of the outcome of this choice.

9   SYMBOLIC UTILITY

If the above limitations to RCT demonstrate that a theory of social action must inevitably include the presence of another subject in the preference formation, a similar conclusion is reached when the critical analysis is dealing with the concept of utility. According to Nozick, the concept of utility is built so that it also includes the pursuance of symbolic goods. To the familiar type of utility Nozick adds "a component concerning the way an action fits into a person's self image and is self expressive" [1993, 48]. Moreover, he conceives the agent as oriented to perform actions which "symbolise[s] his being a certain kind of person and his having a certain image of himself of being of that kind" [1993, 49]. An agent, that is, can be conceived as capable of organising his action by following a series of principles, thanks to which he can classify his conduct. A certain act is then considered as standing for all the other acts of that class, and therefore symbolising them. By forging this connection, that is, by adopting a certain principle, "the (dis)utility of an act becomes the (dis)utility of performing all the acts of that class....doing that action this time will lead us to expect that we will continue to repeat it" [1993, 49]. This situation is typically represented by acts meant to save face, keep promises, pay a visit to a sick person, return a gift, accept a challenge to a duel, as well as other cases when honour or trustworthiness (or a similar connection with other persons) are at stake. That is, cases which marks the identity of a person. With reference to game theoretical situations, this means "that our responses to the prisoner's dilemma are governed, in part, by our view of the kind of person we wish to be and the kinds of ways we wish to relate to others" [1993, 57]. The rationality of an act thus includes a reference to the effects of this act on the self-image of the agent, as well as to the judgement others will give of his identity. The same general idea is at the origins of the bargaining tactic called side betting, which involves committing oneself to a course of action by putting one's own future credibility at stake [Schelling, 1960]. And in a slightly different form, the fact that choice includes a judgement of the actor of himself, is alluded to in the concept of diagnostic reward. According to Prelec and Bodner, choices bring about "two types of reward (or utility): 'causal rewards', that flow directly from the consequences of choice...and 'diagnostic rewards', consisting in the pleasure or pain derived from learning something positive or negative about one's internal state, disposition, ability, or future prospects" [2003, 274]. In all these cases, the utility of a choice, and therefore the idea of rationality, enters in relation with the self-image of the agent, and his relationship with other persons in a social situation.

10   DISREGARDING TIME

From the foregoing analysis, it has clearly emerged that a theory grounded on a notion of rationality, which takes the propositional attitudes of the agent as being the *cause* of the action, is led to ignore two essential dimensions of social action, which can be classified as vertical and horizontal dimensions. The vertical dimension refers to the awareness, in the act of choice, of the duration of the action in time. The horizontal dimension refers to the awareness of the impact of other subjects in defining the meaning of the desired outcome. The reality of an action indeed is not concluded with the satisfaction of a desire, but continues to receive meaning by the judgement which others make of the outcome. For human beings, to take decisions is to necessarily deal with the organisation of time beyond the immediate present, up to and until the actual death of the person [Searle, 2001]. As Searle puts it, "Death, one may say, is the horizon of human rationality" [2001, 3]. In this sense, an understanding of the choices of a person will imply a definition of his identity.

We can distinguish between two theoretical directions that have sought to incorporate the question of time into RCT analysis. The first gave rise to a theory meant to determine how an agent makes a choice by discounting its future utility. In this type of theory, choice involves a trade-off between costs and benefits, occurring at different times [Shane *et al.*, 2003]. Interest in this theme emerged already in the eighteenth century, with the first discussions on the discipline of political economy, thanks to the Scottish economist John Rae. But the first model of discounted utility (DU) was proposed by Paul Samuelson in 1937. DU assumes a unitary discount rate that applies to all acts of consumption. Individuals are assumed to express a single rate of time preference, which they use to discount the value of delayed events. DU was proposed by Samuelson with manifest reservation, and has generally been considered deficient by the literature that has tried to apply it. In any case, its assumption limits its application to forms of utility measurable in money, or in goods exchangeable with money. It does not consider consumption where the value of the choice is predicated upon the judgement of people other than the agent.

This is a situation dealt with by the second theoretical direction that has emerged, which confronts the potential discrepancy in the mind of the agent between the long and short term consequences of his choice (as the phenomenon of akrasia exemplifies). The ideological and moral effects of this discrepancy are numerous. As an illustrative example, we can take the central tenet of liberal ideology, which states that everyone is the best judge of one's own interest, and confront this with the paternalistic principle that some superior authority has the duty to define what the "true" (long term) interests of a certain individual is, in order to prevent him making choices which will be contrary to that interest. While representative institutions, at least apparently, operate on the first principle, institutions of the welfare state (as well as various professional deontologies) imply that the true interests of an individual manifest themselves in the long term, and

therefore are better taken care of by some expert authority capable of knowing how to define them. The individual agent also confronts a similar predicament when he makes his choice, being uncertain as to whether the decision must be taken as responding to his immediate desires, or by considering the "true" interest of his future self.

The idea of two (or more) selves of an individual, a concept which is present in varying forms in classical (as well as oriental) antiquity, is taken up again to make logical sense of this type of predicament. A specialised discipline has been developed, entitled "egonomics," which brings together economists, philosophers and psychologists. Thomas Schelling is the main exponent of this line of research. His models are mainly devoted towards explaining the strategies of an individual in attempting to reach "self-command," like the strategy of pre-commitment, when a person uses stratagems to prevent himself from taking choices that he judges will be against his well considered interest [Schelling, 1978; 1984]. The case of Ulysses and the Sirens was made famous, to this effect, by a brilliant essay of Jon Elster, which typified the strategy of pre-commitment, wherein a "straight" self controls a "wayward" self by enjoining an external agent not to obey what the latter self will later command [Elster, 1979]. A similar example is that of an obstetrician being asked by a patient to withhold anaesthesia during delivery. Does the woman ignore the pain she will undergo? Should the doctor obey the earlier command of the woman unaware of the pain, or the later imploration of the woman in pain? But then suppose that the woman had delivered previously, and was well acquainted with the pain. Should now the doctor obey the previous demand to withhold the anaesthetic, or should the doctor comply with the later demands of the same woman in pain? [Schelling, 1984].

Within the literature of egonomics, there are attempts to give a formal definition of the structure of the two selves that enter in that model. For instance, Thaler and Shefrin [1981] conceive of the difference between the two selves as corresponding to the relationship between a principal and an agent. The principal (the "planning self"), is the bearer of true interests, while the agent (the "doing" self), is in charge of daily decisions An earlier and well-known attempt to deal with the same problem is by Harry Frankfurt [1971], who attempts to define the problem in terms of preferences and meta-preferences. Frankfurt differentiates desires as belonging to either a first or a second order. Desires of the first order are those which bring persons to make choices in order to satisfy their needs. While Frankfurt recognises that this is a form of volition that human beings share with other animals, he goes on to observe that "it seems to be a peculiar characteristic of humans. . . that they are able to form. . . 'second-order' desires" [1971, 6]. This is linked to a desire to be distinguishable from one another in their preferences and purposes. This reflective self-evaluation is manifested only in humans, by the formation of second-order desires, and is essential to becoming a free person, that is, a rational agent. Rationality is therefore not generically defined by its links to intentionality, but by the specific intention to make the choices that are functional to the process of constitution of the agent as a distinct person.

Above all, this lack of a temporal dimension manifests itself in situations where the subject decides on a particular course of action, while still suspecting that when the results of his action are clear, he will evaluate them with criteria different to those he used when making the choice. We label such situations as exposed to "value uncertainty." This refers to changes which do not depend on the subject itself, but rather concern the judgement that the outcome of his choice will receive from other people. In envisaging his choice in this manner, the agent has been led into a process of preference formation that depends on the judgement that other subjects give of the outcome.

11   DISREGARDING OTHER PEOPLE

The concept of "value uncertainty" allows us to assess the limitations that hinder the use of RCT, arising due to its failure to consider the judgement of other subjects as being relevant to the behaviour of an agent. Value uncertainty is present when the value that an agent pursues in his choice depends on the judgement that a definite group of people give on an acquired benefit. It could be an entitlement, or a badge of honour, or a diploma, or any other good whose value is such only because a certain group of people recognise it. The concept of "positional good" comes closest to this idea [Hirsch, 1976; Heap, 1989]. The presence of others thereby contributes to our own personal processes of preference formation. Not in the sense that others may influence our will (this may happen in many ways, but does not concern the function of choice), but rather in the sense that even if we were completely free to make the choice that appears most convenient to us at that moment, we are aware that the value deriving from that choice will be subject to the uncertainty of the changeable judgement of other people. In other words, this value will be determined by the relation that is established, in making that particular choice, with a value system that the agent controls only to the extent that he keeps his relations with some collective entity. This entity we may call a "circle of recognition," because the identity of the agent, and its modification in time, depend on its recognition.

It may be useful to clarify the above by referring to two notions we have already mentioned: *consumption as communication* and *symbolic goods*. Suppose that we wish to explain why a person changes his clothes when he must take part in a ceremony. The most convincing explanation would be that he has put on that particular outfit (a wedding suit for example, or an official dress, or a uniform, or a priest's robes when celebrating mass) because this is what was demanded of him by that ceremony. The choice of outfit was a way of entering into communication with the other participants, of declaring the nature of the role he was playing. Given the context, the participants would be baffled as to how to interpret events if the person wore a different outfit. Could it be said that the person, by wearing that outfit, had maximised his utility? It could be said, but it would evidently be far from illuminating. The audience listening to an observer's description of the event would rather wish to know if the person behaving in such a way was acting

rationally, and if so, on what grounds such a judgement would be justified. At this point a response to the following questions would be unavoidable: a) what, in that particular culture, does putting on that type of outfit in those circumstances mean?; and b) how can the observer express, in the cultural understanding of his audience, the *significance* of an event belonging to that type of culture?

The phenomenon of *consumption as communication* allows us to account for the phenomenon of changing tastes. Our tastes, even if they are directed to satisfying more or less the same general needs (eating, clothing, health, shelter, and other similar basic needs), continue to change over the course of time. We can explain this only if we refer to the continual shifting of circles of recognition. This dictates change in tastes. Indeed, it is not needs that change, but tastes - and these change as a way to allow individuals to adapt to the conventions that regulate their relations with social groups.

We can refer to this as a case of conspicuous consumption. By proposing this notion, Veblen [1994] did not limit himself to offering a realistic interpretation of one particular style of consumption belonging to an ascending class in a given historical period. Nor did he limit himself to advancing a critique of the more general theory of equilibrium, which assumes that the consumer, when choosing between various goods in order to maximize his monetary gain, ignores the choices of other consumers. At least implicitly, he proposed a general theory of "consumption as communication." In other words, he proposed that consumer choices derive their value not from the intention to maximize utility independently of the choices of other consumers, but from the effects of this choice in terms of the consumer's placement in some social position.

If to consume means to communicate, then we must assume that there is an individual activity meant to evaluate the consequences of this communication, that is, to judge which choices should be carried out in order for the agent to present a distinct identity which may be recognised by other participants in the situation. We must then imagine the person as having some component which operates in a manner super-ordinate to the activity of carrying out choices. Simply referring to some general principles of well being (health, prestige, pleasure...), as proposed by Becker [1996] and others, is not sufficient. Firstly because this ignores the fact that the subject must have some criteria thanks to which he is able to evaluate, for example, if he should continue to strive for professional success or prestige at the expense of his health, or vice versa. Secondly, because it ignores that the results of his choice have a value which is derived from the judgements of others. The importance of others to the notion of self is best expressed in the form "circles of recognition." This concept allows both for a judgement on the private choices of a person, expressed by one's own individual self, and a judgement expressed by a social entity (like a circle of recognition) on those choices.

The notion of the "circle of recognition," it should be noted, could also assume an entirely virtual form, where the subject behaves simply by *imagining* the circle of recognition that would evaluate his choices. In this case the observer would see, and have to interpret, expressions of the individual's own ideology. To give

an example, one possible imagined circle of recognition is what Christian doctrine calls "the community of saints." This would then to a large extent justify the life choices of a hermit. Another imagined circle of recognition could be one constituted by the idea of a new society to be realised after the death of the agent. This would explain the behaviour of a revolutionary, or even of a *kamikaze*, who by definition chooses not to enjoy the outcome of his choice during his own lifetime, but considers that the recognition he imagines to receive by his circle is sufficient to justify a choice which leaves no future for him as a person.

When the choice to consume one product instead of another is not based on an evaluation of cost, but on an evaluation of the effects that a choice will have on the relative position of the individual in a circle of recognition, this circumstance will also affect the evaluation of the uncertainties surrounding that choice. If recognition by others has the function of validating the choices made by the subject, and determines his distinct identity, then the threat of uncertainty changes. In addition to the uncertainties that stem from the unpredictable nature of possible changes in the state of the world, the subject could also subsequently find himself confronted with the possibility that the objective of his choice, at the moment of producing a reward, has lost its value. This is not a simple case of regret. The person is not confronted with the possible irrationality of a past decision. The decision, at the moment it was made, was in all effects rational. But when the outcomes of the choice is appraised, the person could find that he no longer holds the same preferences, the same tastes, as when he made the choice. In making that choice he aimed to determine his identity, that is, the recognition of his social standing. Now it is as if the person finds himself to be another person. He is judged by a different circle. He made a choice because he assumed that the values that guided him would be recognised and would endure over time, but they have not. The result was achieved, the objectives acquired, but nobody is left that recognises their value. Think of an outfit bought in response to the latest fashion, or a decoration awarded for actions, that in a certain regime were highly appreciated.

Yet the fashion did not last till the next season, and the regime fell and was replaced by another that did not recognise the decoration awarded. In other words, the circles that recognise these choices, or better, to which these choices 'communicated' a particular significance on the basis of which some part of the person's identity was built, no longer exist. They have dissolved, or no longer represent the values that moved the person to make that choice. Therefore the person perceives one of his past acts as belonging to an identity he no longer considers his own, and no-one any longer recognises the values that were realised through that choice. The currency used to calculate the utility of the choice is now useless, and it can no longer be exchanged for any other. Rationality is not in the intention of the agent; it is in the eye of the beholder. Or better, of the participants in a social situation.

With regard to uncertainty over a future state of the world, think of the impact of a tsunami, where the person is by definition impotent. In the case of value

uncertainty, the uncertainty may often depend upon the choice of the agent. The agent may decrease the probability of the value uncertainty by keeping his relations with a circle of recognition stable, or increase it by changing circles, or, at the extreme, by conversion. Naturally, in real world situations, processes of dissolution or transformation of circles of recognition are rarely total. Changes in fashion may be predictable or easily calculable, and so they resemble risks that are to some extent measurable. A change in fashion is not so difficult to predict. But this is markedly less true for changes in political regimes that recognise certain decorations; or changes in the moral order, e.g., from one where homosexuality is morally disapproved and performed covertly, to one where the same preference is manifested with pride; or changes in a profession we chose when its prestige was very high, and that we find suddenly down-graded or non-existent. The same can also occur when somebody migrates from a country where certain titles (and their connected prestige), are highly regarded, to one where nobody recognises them, and the person appears to himself stripped of what he thought constituted his social identity. These are all uncertainties whose consequences an actor could not predict when choosing. A common strategy for actors in such cases tends to be that of keeping close to the group that continues to recognise the value of his choice, and of acting in a way that ensures his continued recognition. By this he would continue to be able to attribute value to a good which that he chose when he believed it would have a determined significance, thus preventing a situation where nobody remains to give that good significance.

12 CONCLUSIONS

It has emerged from this analysis that RCT has attained some partial successes in explaining human behaviour, but in its current form is deeply affected by a number of anomalies and predicaments, that have been summarised here. The endeavour to overcome these predicaments, as we have seen, points in two directions, one having to do with the structure of the agent, the other with the structure of the social situation within which the action takes place. The conflicts within the agent can be understood only if the agent is modelled in a more complex way than is commonly done in folk psychology, which considers the person as a unitary entity. The traditional analysis, which has identified within the human self the part of passion and the part of reason (the concern for immediate desires and the long term interest of the person), could rather be conceived as the distinct functions of choosing and of judging the validity of choices. The self which chooses acts according to a principle of maximisation, and its action opens and closes (with or without result) every time it deals with other people or things. The self which judges does so by accumulating certain results in time, and aiming then to minimise uncertainty. This is obtained by a commitment to bring coherence into the activity of choosing, and by securing the positive judgement of one (or several) circles of participants in a social situation from which the identity of the acting self depends. This view implies that it is not adequate to consider the superior aspect of self

(which some egonomists call the "principal") as a unitary entity. The judging self is also plural, and if he aims to control and discipline desires, he does so in view of gaining social recognition and position by the several social circles within which he operates. It is from these that the judging self receives the rules, which are then transmitted to the choosing selves. These are not private to the self, but are in a way public (or social), because they refer to some form of observable entity. The task of an observer aiming to explain the meaning of a social situation is firstly, to reconstruct the position of the several social circles from which an agent receives his identity, and secondly, to examine the consistency of his choices as a function of the identity he receives from this or that circle of recognition. It we consider these circles as a possible "fiction" of an agent, this conception can then analytically include those types of rationality which have been described by several authors as being distinct from instrumental ones, like the value rationality of Weber [1963] or the cognitive rationality of Bourdon [2003] or the reflexive rationality of Rovane [2004].

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ETHNOGRAPHY AND CULTURE

Mark Risjord

1 INTRODUCTION

> Imagine yourself suddenly set down surrounded by your gear, alone on
> a tropical beach close to a native village, while the launch or dinghy
> which has brought you sails away out of sight. Since you take up
> your abode in the compound of some neighboring white man, trader
> or missionary, you have nothing to do, but to start at once on your
> ethnographic work. Imagine further that you are a beginner, without
> previous experience, with nothing to guide you and no one to help you.
> For the white man is temporarily absent, or else unable or unwilling to
> waste any of his time on you. This exactly describes my first initiation
> into field work on the south coast of New Guinea. [Malinowski, 1922, 4]

With this romantic image, Bronislaw Malinowski introduced ethnography to the
public's imagination. Malinowski was aware that *Argonauts of the Western Pa-
cific* was not the first work of its kind: traders and missionaries had written about
the small, non-western communities among whom they had lived. Nonetheless,
Malinowski's narrative vividly illustrated a novel empirical methodology and crys-
tallized an emerging discipline. He contended that his method had several features
that distinguished it as a scientific work, in contrast to more casual travelogues.
Unlike travelers and colonial officials (but like some missionaries), he participated
in the daily routines of his subjects. He recorded their myths, stories, and responses
to his queries in their own language. This, he argued, enhanced the accuracy of the
record and preserved it for future research [1922, 23-24]. Moreover, Malinowski's
work was theoretically informed by theorists like Tylor, Frazer, Durkheim, and
Spencer. This background provided theoretical problems to focus his research and
a conceptual repertoire with which to interpret his observations [1922, 9]. Finally,
Malinowski's field work was holistic: "One of the first conditions of acceptable
fieldwork is that it should deal with the totality of all social, cultural, and psycho-
logical aspects of the community, for they are so interwoven that not one can be
understood without taking into consideration all the others" [1922, xvi].

   Participant observation, open-ended interviews in the native language, theo-
retical background, and holism: from the jaded perspective of the early twenty-
first century, this list seems quaint. Late twentieth century writers subjected

ethnography to withering criticism. The primary target of critical attack, how-
ever, was not ethnography, but *culture.* Malinowski, Boas, and the other pioneers
developed ethnography as a method for discovering the characteristics of partic-
ular cultures. As the concept of culture developed in the twentieth century, the
method of ethnography changed with it. When the culture concept was implicated
in colonial politics, ethnography and ethnographers were implicated as well. The
epistemological questions about ethnography thus cannot be answered without
addressing the conceptual or metaphysical questions about culture.

This essay will touch on several venerable philosophical debates about the social
sciences. The first is the "explanation — understanding" problem. In its tradi-
tional and paradigmatic form, the explanation — understanding issue concerns
whether the "social sciences" require ways of generating knowledge that are dis-
tinct from those used in the "natural sciences." In the early and middle twentieth
century, philosophers of science took the natural sciences to have a unified method.
Hence, the contrast between "social" and "natural" science was vivid. Philosoph-
ical inquiry into the natural sciences has undermined this confident contrast, and
the question is no longer pressing in its classic form. Moreover, it is obvious that
methods relying on measurement, experimental manipulation, statistical analysis,
and so on, can be applied to human subjects. Ethnography generally eschews these
tools, preferring to use interviews, participation, and forms of linguistic analysis.
A better way to raise the question, then, is to ask whether the methods distinctive
of ethnography have an epistemic status different from other ways of finding out
about the world. A negative answer would hold that there is a single epistemology
for both human and non-human phenomena. This might be called an epistemic
monism: there is one kind of theory structure and one form of confirmation suit-
able for all topics. The alternative — associated with the *verstehen* tradition,[1]
but not limited to it — would be an epistemic dualism.[2] Framing this issue in this
way permits us to escape the preconceptions about explanation that have been
undermined by recent philosophy of science [Risjord, 2000].

Methodological individualism is the second important philosophical debate to be
discussed here.[3] Twentieth century ethnography developed the idea that cultures
are independent of individuals; a person is born into a culture, absorbs it, and
transmits it to his or her progeny. This understanding of culture was not the first
to arise, and, as already noted, was severely criticized within anthropology. As we
will see below, twentieth century thinking about ethnography was shaped by the

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[1]For detailed discussion of the *verstehen* tradition in anthropology and sociology, see the
chapters by Turner and Outhwaite in this volume.

[2]Once we start multiplying epistemologies, there is no logical reason to rest with two. Hence
"epistemic dualism" might be a misleading term. It is appropriate here because every defense
of this view has insisted that there is something special about the difference between humans
and non-humans. It should be clear that epistemic dualism does not entail metaphysical dualism
(*e.g.* the Cartesian thesis that minds are substances). However, many have made the converse
inference.

[3]For discussion of methodological individualism, see the chapters by Zahle and Little in this
volume.

debate between those who wanted to treat culture as something *sui generis* and those who wanted to treat it as a collection of individuals.

The pivotal question of twentieth century ethnography is the product of the foregoing epistemic and metaphysical issues: to what extent is ethnography a generalizing form of inquiry? Anthropologists who treat culture as a collection of individuals take ethnography to be primarily descriptive. The goal of ethnography is to capture the native's point of view, and this is taken to require a description of the representations, experiences, or other traits of the individuals. There is little or no need for the generation or testing of hypotheses. On the other hand, anthropologists who treat culture as something distinct from individuals are faced with an epistemic problem. They must use what the individuals say and do as evidence for their understanding of the culture. Their statements about the norms, institutions, or other cultural forms are hypotheses that must be tested against more concrete forms of evidence. Ethnography is thus a form of inquiry that requires hypothesis generation and testing.

The role of moral or political values in social scientific inquiry is the final broad issue relevant to this discussion of ethnography.[4] Ethnography developed as part of the colonial encounter. At the very least, ethnographic research required the cooperation of colonial authorities; occasionally, it was more deeply implicated. Willingly or unwillingly, ethnographers occupied a position in a structure that dominated and exploited non-European peoples. The very concept of culture, some have argued, is a product of the need to govern indigenous groups. When the political critique of ethnography, and by extension, all of cultural anthropology, emerged in the late nineteen-sixties, it precipitated a crisis. Today, many anthropologists take it for granted that the concept of culture has been deconstructed. Yet the philosophical problems raised by such a deconstruction remain unsolved: What becomes of ethnography if there is no culture? What becomes of cultural anthropology if there is no ethnography?

2   CULTURE AS TRAITS: TYLOR AND BOAS

It might seem odd to begin a discussion of ethnography with Edward Bennett Tylor. Subsequent generations roundly criticized Tylor's work for being inadequately based on empirical research (e.g. [Radcliffe-Brown, 1923]). And while Tylor was a traveler, he never did fieldwork, even by nineteenth century standards. Tylor was nonetheless seminal to ethnography[5] for several reasons. As a theorist, he was quite concerned with the empirical basis of his theories. He

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[4]See Jarvie, this volume, for a description of some of the values underlying the anthropological tradition.

[5]In the anthropology of Tylor's time, the terms "ethnography" and "ethnology" did not have well established uses. By 1922, Malinowski could make a clear distinction and justify it as "a useful habit of the terminology of science" [Malinowski, 1922, 9]. This essay will adhere to Malinowski's distinction: "ethnology" is the comparative study of culture, "ethnography" is the empirical study of particular groups of people.

wrote about the methodological difficulties of obtaining information about other cultures, and he was sensitive to issues of bias and incompleteness. Under the auspices of the British Association for the Advancement of Science, he initiated some of the first systematic ethnographic research. Finally, Tylor provided the first English-language[6] development of the anthropological concept of culture and hence the first notion of what ethnography is about:

> Culture or Civilization, taken in its wide ethnographic sense, is that complex whole which includes knowledge, belief, art, morals, law custom, and any other capabilities and habits acquired by man as a member of society. [Tylor, 1871, 1]

Tylor thus provided the motivation for ethnography and shaped its conceptual framework.

It is easy to misunderstand Tylor's culture concept. In 1952, Kroeber and Kluckhon undertook a systematic survey of definitions and concepts of culture [Kroeber and Kluckhohn, 1963, 87]. They were puzzled by the fact that Tylor's definition was not substantially revised, or even revisited, by anthropologists for almost fifty years. Then, after 1920, there was an explosion of definitions [Kroeber and Kluckhohn, 1963, 295]. Such a lack of theoretical work on the concept of culture was made more puzzling by what Kroeber and Kluckhohn saw as its theoretical weakness. Tylor's definition was no more than a list of traits. Culture, they argued, is something abstracted from the superficial traits, hence defining culture in terms of the traits is bound to be misleading. Why, then, were anthropologists satisfied for so long with an inadequate definition? And why was there a sudden interest in the concept after 1920? These puzzles hint at the way in which Kroeber's and Kluckhohn's own culture-concept influenced their view of the history of their field. Tylor's conception of culture is importantly different from the conceptions that developed in the nineteen-twenties and thirties. For Tylor, culture was not something that explained belief, art, and so on. It was not something theoretical that stood behind the phenomenon; it *was* the phenomenon. It was less a theoretical concept than it was an outline of an area of research. The idea that culture might be a theoretical entity arose around 1920, and thus the question of how culture was to be defined became pressing.

Tylor's concept of culture was closely tied to the ethnological project of the nineteenth century. Anthropologists were concerned with a broadly historical problem of understanding the development of human civilization. Indeed, while he introduced the term "culture," "civilization" was Tylor's preferred term, and the two terms remained cognate until the early twentieth century. Culture was thus not understood as something shared by a small group of people which makes them different from their neighbors. Rather, culture or civilization is something that all humans share. It can be divided up into developmental stages, and peoples from

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[6]Tylor borrowed the concept from German-speaking authors. In this tradition, the concept of culture had been developing for about one hundred years. For historical background and discussion, see Kroeber and Kluckhohn [1963] and the essays in Stocking [1996].

around the globe can be placed into the same scheme. Traits do cluster into what we now call cultures — the Inuit are different from the Navajo — but each trait might be compared with others across the globe without loss or distortion.

Conceiving of culture as a collection of traits has immediate consequences for the study of culture. A comparative science of ethnology would require a large database of cultural similarities and differences. The data would have to be gathered systematically, so that ideas or artifacts from different parts of the world could be compared. A lack of information on a topic would be as problematic for comparative theory as incorrect information. Ethnography was important, but it was little more than the collection of material and ideational artifacts. From this perspective, one can see why a manual like *Notes and Queries on Anthropology* would be useful. Published in four editions between 1874 and 1912,[7] *Notes and Queries* was a list of topics to guide amateur fieldworkers. If culture is a collection of traits, then anyone with time and patience could be an ethnographer. Tylor was a major contributor to the first three editions of *Notes and Queries,* and its organization and substance directly reflected his conception of culture [Urry, 1972].

Two further features of Tylor's concept of culture had ramifications for ethnography. Tylor presupposed that cultural traits were shared by individuals. At a theoretical level, this presupposition was required by his project. He was looking for world-wide patterns of culture that can be explained as a succession of stages. Traits will thus be shared among people in different times or parts of the world; the members of a local a group at a given stage of cultural evolution will share beliefs, art forms, morals codes, laws, etc. In the following passage he makes this presupposition explicit and draws out its methodological consequence:

> The quality of mankind which tends most to make the systematic study of civilization possible, is that remarkable tacit consensus or agreement which so far induces whole populations to unite in the use of the same language, to follow the same religion and customary law, to settle down to the same general level of art and knowledge. It is this state of things which makes it so far possible to ignore exceptional facts and to describe nations by a sort of general average. It is this state of things which makes it so far possible to represent immense masses of details by a few typical facts, while, these once settled, new cases recorded by new observers simply fall into their places to prove the soundness of the classification. [Tylor, 1871, 10-11]

Since the items that distinguish culture as a phenomenon are shared by a number of individuals, and the interest of the anthropologist is precisely in the shared aspects, ethnography must ignore the individual differences and grasp what is similar among all. Because the issue became tendentious later, it is worth noting that Tylor did not let this presupposition blind him to the role of individuals in shaping and transmitting the elements of culture. He affirmed a reductionist

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[7]Two subsequent editions were published, in 1929 and 1951, but they were different in important ways from the earlier editions.

conception of culture, writing that "collective social action is the mere resultant of many individual actions" [Tylor, 1871, 13].

Tylor's concept of culture and view of ethnography spread to America through the influence of Franz Boas. A distinctive feature of Boas' ethnography was that he amassed huge quantities of data. His work on the Kwakiutl generated thousands of pages of text, but nothing like the narratives of Malinowski. Subsequent anthropologists have been puzzled by this rather undigested mass and attributed it to his strong anti-theoretical bias (cf. [Harris, 1968]). When we view his ethnography in the light of Tylor's concept of culture, it makes better sense. Later anthropologists expected ethnographies to provide a commentary that synthesized or explained the varied cultural traits. Boas makes little or no attempt to analyze or explain the facts he presents because, like Tylor, he is not thinking of the culture as something that stands behind them.[8] Ethnography was the collection of data for anthropological theorizing. And Boas, especially in his early work, agreed with Tylor that the science of ethnology aimed at discovering laws [Boas, 1896/1940]. It was Boas' methodological rigor and the theoretical questions he tried to answer with this data that set him apart from Tylor.

While his orientation to culture and ethnography was inherited from Tylor, Boas developed the concept of culture and with it the practice of ethnography. The first important development was the emphasis on the holistic character of culture. Tylor's definition already took culture traits to form a "complex whole." Yet, that whole is nothing more than an aggregate of the traits. By the late 1890s, Boas' writings began to reflect the idea that the individual traits could only be understood in the context of a particular culture. The significance of an individual trait is partly determined by the other elements of that culture.[9] Boas clearly understood the consequences of this sort of holism for ethnography. Ethnographers cannot be casual collectors. They must spend enough time among a people to understand how the different items relate to each other. Anthropologists throughout the nineteenth century had recognized the superiority of observers who lived for a long period among their subjects. Appreciating the holistic character of cultural traits gave the idea new significance. Observers who lived for long periods among their subjects were not merely in a position to observe more facts, or to observe them more accurately. Rather, only by understanding the whole body of cultural features could the ethnographer see the significance of the individual parts. The idea that lay behind the first editions of *Notes and Queries* — that a casual traveler might be able to collect tidbits of interest to the anthropologist — was discredited.

Boas developed the concept of culture in two further ways. Both arose out of his concern with the question of whether mental capacities vary by race. His initial engagement with this issue looked to physical anthropology and psychometrics for

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[8]This is true in the first half of Boas' career. His concept of culture developed in the direction of the modern conception, though he never embraced Kroeber's "superorganic."

[9]This idea also had profound consequences for the kind of comparative project favored by nineteenth century anthropologists. See [Boas, 1896/1940].

evidence [Boas, 1894/1974]. By 1901, he was arguing that differences in behavior are the result of cultural differences. All humans, he argued, share a set of psychological processes for manipulating ideas [Boas, 1901, 3-5]. Apparent differences among humans are the result of similar processes operating on different ideas. An individual's behavior is determined by her unique set of ideas and her psychological mechanisms for reasoning with and acting on these ideas. Individuals within a culture act in similar ways because they share a set of ideas. The culture of a group is then understood as "the total mass of traditional matter present to the mind of a given people at any given time..." [Boas, 1901, 2-3]. Two important changes in the concept of culture are signaled here: culture determines behavior, and cultural facts are marked off as being traditional, passed down among generations. While they did not influence ethnography immediately, these developments set the stage for more profound changes.[10]

3   THE "SUPERORGANIC" AND CLASSICAL ETHNOGRAPHY

By arguing that any cultural trait could only be understood in the context of the larger group of traits and that culture determined behavior, Boas began to give the concept of culture explanatory power. It could no longer be identified with the corpus of texts, artifacts, behaviors and artworks. The culture of a group partially explained those very traits. Culture thus began to take on the status of a theoretical entity. The character of this theoretical entity and its relationship to individuals has remained problematic until the present day, but it influenced ethnography through the rest of the century.

The earliest and most forceful recognition of the status of culture as a theoretical entity is A. L. Kroeber's famous essay, "The Superorganic" [1917]. The burden of this essay was to insist on the distinction between organic and social life, and to argue that culture is not reducible to psychology or biology. It is important to note that throughout this essay, Kroeber is thinking of culture (he uses "culture," "civilization," and "history" interchangeably) as whatever it is that separates humans from the other animals, not something that makes different groups distinct. In this respect he is still working within the Tylorian framework. The argument for a distinction between organic and social phenomena is that there is an obvious difference between organic evolution and invention. Humans adapt to new environments by inventing means of dealing with them. Unlike evolved traits, inventions can be immediately shared with other humans to whom the inventor is not related, and they can be shared with the living members of previous generations. The social or cultural characteristics of a person, then, are those that are contingent on learning and contact with other members of the culture; the biological characteristics are those that will be acquired regardless of environment.

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[10]Boas' developments in the concepts of culture and ethnography were paralleled in Britain prior to the landmark publication of Malinowski's *Argonauts*. For an excellent discussion of these developments, see Urry [1972].

The distinctive feature of culture is that patterns of behavior and ideas may be transmitted from person to person. Kroeber infers from this that culture is not reducible to psychology or biology:

> "...tradition, what is 'given through,' handed along, from one to another, is only a message. It must, of course, be carried; but the messenger after all is extrinsic to the news. So, a letter must be written; but as its significance is in the meaning of the words, as the value of a note is not in the fiber of the paper but in the characters inscribed on its surface, so tradition is something superadded to the organisms that bear it, imposed up on them, external to them. And as the same shred can bear any one of thousands of inscriptions, of the most diverse force and value, and can even be tolerably razed and reinscribed, so it is with the human organism and the countless contents that civilization can pour into it" [Kroeber, 1917, 178]

Kroeber thought of culture as a kind of entity distinct from individuals, and he sometimes wrote as if the behavior of individuals was irrelevant to the history of culture. While he held a stronger version of this ontological position than any of his colleagues, anthropologists in the nineteen twenties followed him in conceiving of culture as something that (1) could not be explained in biological terms, (2) was distinct from the ideas or behavior of individuals, and (3) was transmitted from one generation to the next. Notice Krober's use of the metaphor of a code: meaningful messages are transmitted from one generation to the next and shared among individuals.

On the British side of the Atlantic, Malinowski was drawing conclusions similar to Kroeber's. However, he emphasized the normative character of culture in a way that the Americans did not. Malinowski thought of a culture as having a skeleton of laws or rules:

> In popular thinking, we imagine that the natives live on the bosom of Nature, more or less as they can and like, the prey of irregular, phantasmagoric beliefs and apprehensions. Modern science, on the contrary, shows that their social institutions have a very definite organization, that they are governed by authority, law and order in the public and personal relations, which the latter are, besides, under the control of extremely complex ties of kinship and clanship. Indeed, we see them entangled in a mesh of duties, functions and privileges which correspond to an elaborate tribal, communal, and kinship organization. [Malinowski, 1922, 10]

This emphasis arises, perhaps, from Malinowski's theoretical commitment to functionalism. If cultural traits are rule-governed, traits might be explained by showing how the rules help satisfy the needs of the community. The job of the ethnographer, then, is to describe the set of rules that govern social life. Notice how Malinowski's conceptualization of culture, more than Boas' or Kroeber's, pushes

toward the idea that culture is something that a specific group of people share. In this sense, the Trobriand Islanders have a different culture than the Navajo. The American students of Boas, by contrast, continued to think of culture as something that separated humans from non-humans.

The reason that ethnography seems to spring forth in its mature form around 1920 is that, at about this time, anthropologists on both sides of the Atlantic began to conceive of culture as something distinct from a set of traits passed among generations. Culture was something that helped explain why some traits were preserved and others discarded, why groups had different sets of traits, and why individuals behaved in particular ways. This way of thinking about culture had a number of important epistemic consequences. In 1917, Lowie was already making these explicit:

> Psychology, racial differences, geographical environment, have all proved inadequate for the interpretation of cultural phenomena. The inference is obvious. Culture is a thing *sui generis* which can be explained only in terms of itself. [Lowie, 1929/1917, 66]

Both Kroeber and Lowie made the inference that since culture had a distinct metaphysical status, it required distinct methods. Both understood this as marking a difference between the natural sciences and the "historical" sciences. Kroeber asserted that explanation by mechanical, causal laws is inconsistent with historical understanding. History has to look for the antecedents of historical phenomena among other historical phenomena, and any causality in history is teleological [Kroeber 1915]; cf. [Kroeber, 1917, 206-208]. It is ironic, perhaps, that these abstract formulations — so attractive to philosophers of the social sciences — had little impact on the actual practice of ethnography. Malinowski, who was not afraid to call his inquiry "scientific," did much more than Kroeber to spell out the practical epistemic consequences of reifying culture.

Since culture was being conceived as a theoretical entity exhibited or instantiated in the actions of the individuals, questions of evidence arose for Malinowski in ways that they could not on a Tylorian conception of ethnography. Unlike the traits gathered by nineteenth century ethnographers, culture is not something directly present to an observer. Ethnographers have to form hypotheses about cultural entities like "rules," "charters," "functions," "ideals," or "world views." This put ethnography on a par with other empirical disciplines in the sense that it required hypotheses that go beyond the data, and ethnographers must test or justify their claims by appeal to evidence. In the Introduction to *Argonauts of the Western Pacific,* Malinowski turned his attention to the evidence available to an ethnographer and made specific recommendations. Several of these are worth noting here.

First, Malinowski recognized an epistemic problem in the discovery of native rules:

> The Ethnographer has in the field, according to what has just been said, the duty before him of drawing up all the rules and regularities

> of tribal life; all that is permanent and fixed; of giving an anatomy of
> their culture, of depicting the constitution of their society. But these
> things, though crystallized and set, are nowhere *formulated.* [Mali-
> nowski, 1922, 11]

The ethnographer needs to gather evidence about what *ought* to be done on the
basis of what *is* done. The rules outrun the behavior. Malinowski's solution is
to use native judgments about what ought to be done as further evidence. He
suggests presenting informants with concrete examples and asking them how they
would proceed. While Malinowski does not emphasize it (but, *cf.* [1922, 21-
22]), subsequent generations of fieldworkers recognized that participation plays an
epistemic role here too. By participating in the local activities, the ethnographer
will violate the norms. Her transgression will be marked by amusement, disgust, or
impatience on the part of her subjects, and these reactions will provide clues about
the scope of the rules. Malinowski himself primarily conceived of participation as
a way of putting flesh on the bones of the rules and laws. Participation helped the
ethnographer get a feel for the lived reality of the social system. It permitted her
to identify the typical ideas, sentiments, and reactions of members of the culture.
It was the best way to gather evidence about "the native's point of view."

In the late 'teens and early 'twenties, then, a concept of culture emerged that
formed the basis for ethnography in its classical form. The birth of ethnography
was not, however, without difficulty. While virtually all agreed that the study of
culture demanded methods different from the natural sciences, both the character
of culture and its consequences were debated. The role of the individual was one
prominent issue. Edward Sapir argued that Kroeber's conception of the "superor-
ganic" eliminated any possibility of understanding how individuals contribute to
or change their culture [Sapir, 1917]. While an interest in the aspects of social life
that are historically transmitted may be definitive of social inquiry, it does not
follow either that there are social forces or that the contribution of individuals can
be ignored. Sapir thus agreed that the social sciences could not adopt the methods
of the natural sciences, but denied Kroeber's premise that the two kinds of inquiry
dealt with different objects. The students of Boas held various positions on this
issue, with Kroeber and Sapir standing at opposite poles.

Paul Radin developed Sapir's line of criticism in his *Method and Theory of
Ethnology* [Radin 1987/1933], and he extended the critique to bring Sapir's own
work within its scope. Radin's argument is more directly methodological, and it is
noteworthy for the way in which it anticipates the "postmodern" arguments of the
nineteen-seventies and 'eighties. Radin's argument began with Sapir's observation
that a reified conception of culture excludes the individual. Radin pointed out
that ignoring the individual renders ethnography speculative and ungrounded.
If individuals play no role in cultural processes, then their description has no
role in the characterization of the culture. Radin argued that this is not a mere
theoretical consequence, but that the ethnographic work of Boas, Mead, Sapir,
Benedict, and others self-consciously eliminated descriptions of individuals [Radin,
1987/1933, 41-44, 56]. The consequence was that their ethnographic descriptions

had no epistemic basis; we are left with empty abstractions. Radin insisted that
what made a discipline historical was its concern for the individual — be that
an individual person, event, or culture [Radin, 1987/1933, 32]. By abstracting
generalized persons or general characterizations of the culture, ethnographers not
only lost the evidential grounding for their work, they lost its proper object as
well. Radin drew the methodological consequence that ethnographers need to
study individual persons and events in all of their rich detail.

In 1937, Robert Lowie launched a related criticism of Malinowski in his *History
of Ethnological Theory*:

> First and foremost, a science of Culture is not limited to the study of so
> many integrated wholes, the single cultures. This is doubtless impor-
> tant, but it constitutes neither the whole nor even the preponderant
> part of the ethnologist's task. A science of culture must, in princi-
> ple, register every item of social tradition, correlating it significantly
> with any other aspect of reality, *whether that lies within the same cul-
> ture or outside.* In defiance of the dogma that any one culture forms
> a closed system, we must insist that such a culture is invariably an
> artificial unit segregated for purposes of expediency. Social tradition
> varies demonstrably from village to village, even from family to family.
> [Lowie, 1937, 235], emphasis in original)

Lowie was here arguing partly for *ethnology* as a comparative and historical en-
terprise, but the remarks are revealing and, like Radin's, proleptic. The Boasians
were largely engaged in the historical reconstruction of Native American prehis-
tory, and toward that end they compared culture traits among different tribes.
This project led Lowie to recognize the permeability of cultural boundaries. He
did not disagree with the tenets of functionalism as he saw them. Some cultural
practices did have functions that correlated with the needs of the community. But
he insisted that many traits were borrowed and modified. Moreover, there is vari-
ability within any group of people. This variability entails that the ethnographer is
going to have to make some relatively arbitrary decisions about which individuals,
families, clans, etc., are the paradigmatic bearers of the culture. The ethnographer
needs to distinguish cultures for the sake of analysis, but that should not lead her
to reify the individual cultures.

The philosophical debate about ethnography in the nineteen-twenties and 'thir-
ties, then, was not whether ethnography should emulate the methods of the natural
sciences. All parties recognized that the methods of the natural sciences would not
be useful in ethnography or ethnology. They held this position for slightly different
reasons, of course. Kroeber and Lowie held it because the special object of study
was not susceptible to causal analysis. Radin held it because historical inquiry
was concerned with the individual case, not the general laws. The philosophical
disputes were (1) whether culture is a kind of thing distinct from the individuals
or whether it is the common ideas, actions, and artifacts of a group, (2) the de-
gree to which knowledge of the culture depended on knowledge of the individuals,

and (3) whether cultures could be treated as distinct wholes or whether they were artifacts of the ethnographer's analysis.

4   THE "NEW ETHNOGRAPHY"

The tension that remained in the concept of culture and practice of ethnography through the Second World War was whether and to what degree culture could be identified with the ideas or representations of individuals. Was culture "in the head"? Or was it a kind of abstract entity supervening on the individuals? In the nineteen-fifties and 'sixties, the psychological side of the debate received a boost from the emerging field of structural, and later transformational, linguistics. Since early in the twentieth century, ethnographers had found kinship relations to be the key to understanding social organization. Hence, gathering and analyzing kinship terminology was a standard part of ethnography. In the nineteen-fifties and 'sixties, Ward Goodenough, Charles Frake, Stephen Tyler, Harold Conklin, and others developed a method they called "componential analysis." First applied to kinship, componential analysis was an extension of phonemic analysis to semantic and cultural fields [Goodenough, 1956]. Phonetics identifies the total field of possible speech sounds. Speakers and hearers of a particular language pick out only some of these sounds as significant. These are the phonemes: minimal units of speech that make a difference to communication. Roman Jakobson postulated that the speakers' discrimination among phonemes could be identified by the presence or absence of specific phonetic features. For example, the phonemes /p/ and /b/ are distinguished in (Northeastern American) English by being voiced or unvoiced (that is, whether the vocal cords vibrate as the sound is made), /t/ and /d/ are distinguished on the same grounds. The pair /p/ and /b/ are distinguished from /t/ and /d/ by the position of the lips and tongue; the former are bilabial and the latter are alveolar. All four are formed by the sudden release of air pressure in the vocal tract (plosive). The phonemic distinctions that English speakers find significant in this range of sounds can be fully represented by a set of phonetic features: roughly, /p/ is a plosive vocalized bilabial, /b/ is a plosive unvocalized bilabial, /d/ is a plosive vocalized alveolar, /t/ is a plosive unvocalized alveolar. Not all languages recognize this particular set of distinctions, but the phonemes of any language can be systematically identified by minimal contrasts like voiced/unvoiced and bilabial/alveolar.

Similarly, different cultures recognize only part of the total field of possible biological relationships as significant. Kinship relations can be exhaustively specified (in relation to a given person) in terms of mother, father, brother, sister, etc. Anglo-American English speakers distinguish siblings of one's parents only by gender (uncle, aunt). Other cultures find the difference between a mother's brother and a father's brother to be significant and have different terms for each. Componential analysis thus demonstrated the meaning of kinship terms by identifying those features that native speakers used to distinguish one term in a semantic field from others. Componential analysis quickly proved itself useful for analyzing

conceptual fields outside of kinship relations. Frake famously applied it to disease classification [Frake, 1961], and others used componential analysis to understand categorizations of plants, animals, colors, foods, and so on.

Componential analysis was attractive to ethnographers for several reasons. First, it promised to introduce a more rigorous method for obtaining data, analyzing it, and for identifying interesting theoretical problems. Componential analysis was applied primarily to domains where the ethnographer could provide a clear description of the objects denoted and their properties. Then, relationships among local terms could be identified by systematic questions: "Is this an X?" "Is X a kind of Y?" "What kinds of X are there?" and so on. As a method, the use of such questions has the virtue of being repeatable and systematic. Unlike replicable methods drawn from psychology, the use of systematic questions elicited conceptual differences salient to the native speakers. The set of contrasting and complementary terms elicited by the questions was treated as the natives' conceptual map of their environment [Frake, 1964]. A second attractive feature of componential analysis, then, was that it seemed to provide a way to systematically discover and represent "the native's point of view."

The so-called "new ethnography" was more than a systematic method. In elaborating componential analysis, proponents also reconceived the object of ethnography. In a central theoretical articulation of this approach, Frake wrote:

> Ethnography... is a discipline which seeks to account for the behavior of a people by describing the socially acquired and shared knowledge, or culture, that enables members of the society to behave in ways deemed appropriate by their fellows. The discipline is akin to linguistics.... The ethnographer, like the linguist, seeks to describe an infinite set of variable messages as manifestations of a finite shared code, the code being a set of rules for the socially appropriate construction and interpretation of messages. [Frake, 1964, 132]

The new ethnographers thus synthesized a number of trends in twentieth century thought about culture and ethnography. They adopted the Boasian idea that culture was something both shared among individuals and passed down from previous generations. They also accepted Malinowski's idea that rules or norms for behavior were an important aspect of what was shared and learned. Finally, they heeded Sapir's critique of Kroeber, and gave individuals a central place in the analysis. The result was a conception of culture and ethnography that was closely analogous to the conception of language that Chomsky was developing in linguistics. What an individual learns when she acquires her culture is a set of rules for appropriate behavior [Frake, 1964, 133]. These rules are in the mind of each individual.[11] By eliciting relationships among concepts of the native's social and natural world, the ethnographer identified the criteria relevant to deciding what was (and was not) an appropriate thing to do or say. The rules would capture these criteria of relevance.

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[11]An important difference was that Chomsky argued for innate rules, while the new ethnographers assumed that all cultural rules were learned.

If these rules could be systematized in the way that grammatical rules can be, the result would be a "theory" of the culture that explained actions as the result of the application of general rules to particular circumstances.

The new ethnography was criticized on a number of grounds, many of which were raised by Martin Harris in *The Rise of Anthropological Theory* [1968, 568-604]. Three are of particular philosophical interest: the problem of psychological validity, inter-cultural variability, and the relationship of "emic" to "etic" research. The last issue was particularly prominent in Harris' critique. The emic/etic distinction was drawn by analogy with the distinction between phon*emic* and phon*etic* descriptions. Etic descriptions of cultural phenomena depend on the ethnographer's criteria of significance. They are not falsified if the natives have a different view of the matter. Emic descriptions try to capture the native point of view. By characterizing of the object of ethnography as finding the code that generated the natives' view of their world, the new ethnographers restricted themselves to emic descriptions. Harris argued that this was a harmful limitation on the scope of ethnography. There are cultural events and processes not represented by the locals that are nonetheless important. Within Bathonga society, to use Harris' example, it is a rule that brothers and sons rear their families in or near their father's compound. There are many norms governing how family members are to treat one another, but none that specify conditions under which lineages may break apart. Etic investigation shows that the lineages tend to divide when the population reaches one or two hundred. A new compound is established by a social rupture which is typically charged with hostility and witchcraft accusations. Clearly, understanding such transformations is an important part of understanding Bathonga life. If the object of ethnography is only to discover the rules recognized by the natives, crucial aspects of the culture will be incomprehensible [Harris, 1968, 601-602].

One of the main intellectual thrusts of *The Rise of Anthropological Theory* was to make room for etic research in ethnography. With these arguments, Harris engaged a target larger than the "new ethnography" alone. As we have seen, one main line by which the culture concept developed in anthropology emphasized the "native point of view." The methodology of ethnography focused on techniques for capturing that point of view to the exclusion of other psychological and sociological methods. Etic research — including analyses of economics, nutrition, agriculture, and so on — provides an important body of information about the culture. Any conception of ethnographic method that precluded the possibility of blending etic and emic research objects would be *prima facie* deficient. While he does not frame the issue in these terms, Harris' critique raises a deep problem for ethnography. How are etic and emic bodies of evidence to be integrated into a single theory or interpretation of a culture? The new ethnographers had particular difficulty with this problem, but as Harris showed, it was pervasive in twentieth century ethnography.

A different sort of problem with componential analysis is that feature analyses are underdetermined by the data. In "Cognition and Componential Analysis:

God's Truth or Hocus Pocus?" Robbins Burling pointed out that that any set of contrasting and terms can be arranged into sub-sets in a large number of ways [Burling, 1964]. The evidence presented by ethnographers failed to distinguish among these possibilities. Burling did not mention Quine in this essay, but his argument overlapped substantially with Quine's indeterminacy argument [Quine, 1960].[12] Like Quine, Burling despaired of discovering the criteria or rules actually represented by individuals. A more limited aim, however, was achievable. By successfully capturing verbal distinctions, a componential analysis provided rules for use of the terms that corresponded to native usage. Arguably, insofar as it captures rules for public use, a componential analysis has described the meaning of the native terms. This, Burling suggested, might be sufficient for ethnography even if such rules are not taken to be psychologically real [Burling, 1964, 27].

The new ethnographers confidently generalized their findings to the whole culture. Harris and Burling both noted that within our own culture, plants or kinship taxonomies are subject to substantial intra-cultural variation. Different individuals have different ways of using the terminology, and if these uses are taken to reflect real cognitive organization, then different individuals have different cognitive maps. The problem is deeper than a mere overconfidence in the ethnographer's analysis. Harris argued that the ambiguities created by different meanings, either between individuals or within a single individual's conceptual scheme, might be functional [Harris, 1968, 582-585]. Hence, to represent the culture as using a single, unambiguous cognitive map would be to misunderstand something important. It is interesting to note that the new ethnographers had anticipated this point and accepted its consequences. Anthony Wallace had argued that not only were different representations of the social and natural environment possible, shared representations were not necessary for cultural stability [Wallace, 1961, 36-39]. Like Harris, Wallace argued that systematically different cognitive maps among sub-groups might be an important cultural phenomenon. Goodenough himself saw a further implication of intra-cultural variability: "The very fact that it is possible to construct more than one *valid* model of a semantic system has profound implications for cultural theory, calling into question the anthropological premise that a society's culture is 'shared' by its members" [Goodenough, 1965, 259]. Wallace and Goodenough thus accepted the individualistic consequences for which Burling and Harris argued, and rejected the longstanding idea that culture was something shared.

5   THICK DESCRIPTION

Wallace and Goodenough chose to cleave to the ideal of providing a psychologically realistic description of individual representations at the cost of losing culture as something shared by the individuals. The presupposition of the new ethno-

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[12]For discussion of this argument and its significance for ethnography, see Henderson, this volume.

graphers' approach was that words or actions became meaningful by being psychologically represented. This is the root of the various problems raised against componential analysis. A philosophical alternative to this presupposition had been brewing for some time in the philosophy of language. Ludwig Wittgenstein argued that meaning is not best understood to be something private and personal. Rather, meaning was constituted by the public use of words [Wittgenstein, 1953]. Peter Winch extended the idea to the significance of action. Action becomes meaningful, Winch argued, when it was put into the context of the norms and expectations of a group [Winch, 1958]. Clifford Geertz recognized that these new trends in philosophy could be combined with the hermeneutic tradition in a way that supported mainstream ethnographic practice. In many ways, his "interpretive" view was the culmination of the classical model of ethnography.[13]

In the notorious "private language argument," Wittgenstein argued that following a rule required the possibility of making a mistake, and mistakes were possible only if there were public criteria for correct and incorrect ways of applying the rule. He concluded that "obeying a rule is a practice" [Wittgenstein, 1953, 81]. Insofar as the meaningful use of words requires adhering to the rules for their use, the meaningfulness of words depends on their public use. This line of argument not only undermined the presupposition of the new ethnography, it ran contrary to the individualistic strain in ethnographic thinking that arose from Radin's and Sapir's rejection of Kroeber's superorganic conception of culture. The new ethnography crystallized the idea that culture was encoded in the minds of individuals, and that they best way to study culture was to describe those individuals. The Wittgensteinian arguments, along with related work by Peter Winch and Gilbert Ryle, purported to show that individual thought, speech, and action is meaningful only against a background of shared practice. Hence, understanding individuals was impossible except as part of a larger group. Geertz extracted the anthropological consequence from these philosophical arguments: "Culture is public because meaning is" [Geertz, 1973c, 12]. Culture is neither a psychological phenomenon nor some kind of abstraction from individuals. It is the social interactions themselves, perfectly public and observable, yet distinct from any individual participant.

To characterize ethnography, Geertz borrowed a technical term from Gilbert Ryle: thick description [Ryle, 1971]. A thick description deploys concepts that have substantial implications. A thin description, by contrast, is behavioristic, saying as little as possible about the intentions of the agents or social consequences of an action. "Killing" is a relatively thin action verb, "murder" is thicker because it presupposes intentionality and has legal consequences. "Shutting one eyelid and quickly reopening it" is a thin description, but "blink" and "wink" are thicker, each with its own implications. The difference between thick and thin descriptions (perhaps "thicker" and "thinner" would be more precise) is substantiated by the use of the words in the community. An important aspect of the use of words is the practice of drawing inferences. Upon hearing that Smith was killed in an

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[13]Geertz was also a student of Parsons. For discussion of Geertz's relation to both Parsons and the hermeneutic tradition, see the chapter by Turner in this volume.

automobile crash, one does not infer that someone had a motive to kill him. The question of motive arises immediately, however, if it is said that Smith was murdered in an automobile crash. There are obvious practical consequences to these different uses of words as well. Thick descriptions, then, have deeper ramifications and more relationships to other descriptions and actions than do thin descriptions. An ethnographer's goal, according to Geertz, is to create thick descriptions of the local activities. The descriptions will get their meaning from the local practices, and because they are thick, they will show how these meanings are related into the integrated whole of the subjects' culture.

Geertz's interpretive ethnography had all of the traditional elements of ethnography and it made their epistemic rationale clear. Thick descriptions must be stated in the language of the ethnographer, since the ethnographer is writing for her colleagues in anthropology. The terms of the ethnographer's description, however, must be glosses on local terms. The ethnographer must be able to substantiate her thick descriptions with characterizations of the practices that give local speech and action sense. Participation using the native language is thus necessary to identify the practices and learn the appropriate terms of thick description. Holism is implicit in the systematic approach to cultural phenomena. Thick descriptions are thick precisely because they have a broad range of implications. Hence, providing a thick description will require tracing out the relationships among practices. Open-ended interviews are necessary because it is not known *a priori* how any given practice will relate to others. Finally, Geertz was able to embrace the goal of capturing the native's point of view without slipping into the subjectivism suggested by the phrase [Geertz, 1973b, 14]. The native's point of view is not an experience; it is the meaning of the things and events which surround him. This meaning is embodied in the practices and performances of the culture. By engaging these, recording them, and interpreting them, the ethnographer has understood no more and no less than the natives themselves.

To a large extent, Geertz's approach finessed the problems that worried earlier ethnographers. Kroeber and Malinowski treated culture as a kind of thing *sui generis* that was independent of the individuals. Sapir and Radin objected that this reification suppressed the role of individuals in creating, responding to, and changing their culture. By treating culture as something enacted by individuals, Geertz did not reify culture. It was the product of joint activity, not something independent. This permitted the interpretive approach to side-step Sapir's worry about the individual's contribution to culture. The practices that create meaning are the product of individual actions, and particular individuals can have a greater or lesser impact on their development. Moreover, the knowledge of practices can only be obtained by the observation of how individuals act and respond to each other. At the same time, culture is not reducible to individuals or their psychological states. The use-theoretic approach to meaning entails that individual thought and action is meaningful only as part of a culture. Interpretive ethnography thus applauded Radin's press for richly detailed depictions of particular persons and cultural practices. At the same time, it blocked the new ethnographers' slide from

particular descriptions to portraying the culture as a set of individual representations.

Radin's objections to the ethnography of his time, however, were not entirely circumvented. While Geertz's interpretive approach did not reify culture, it did suppose that cultures could be interpreted more or less univocally. Thick descriptions required generalizations about the culture as a whole. According to a use conception of meaning, the meaning of a word depends on its use in *the* language. Hence, the set of utterances to be counted as part of the language needs to be identified. To articulate *the* use requires identification of the right and wrong way of using the word; this in turn depends on generalizations about how members of the culture act. Thick descriptions thus presuppose that the actions of the subjects are neatly homogenous. From the point of view of classical ethnography, this presupposition is innocuous. Cultures just are what groups of individuals share. In the nineteen-eighties, a direct critique of this presupposition arose on grounds that Radin would have recognized.

6   THE DECONSTRUCTION OF 'CULTURE' AND ITS CONSEQUENCES FOR ETHNOGRAPHY

The publication of *Writing Culture* in 1984 created a shockwave among cultural anthropologists. While some dismissed the so-called "postmodern critique" as pointless navel-gazing, the debate changed ethnographic practice in subtle and profound ways. *Writing Culture,* edited by James Clifford and George Marcus, floated a complex of critiques, hypotheses, and analyses, the most prominent of which was a critique of ethnographic writing. Through rhetorical analysis, the authors illuminated ways in which ethnographic writing distorted and falsified its intended object. The critics also raised anew issues about the suppression of agency and change in ethnography. These arguments were given new force by a rising political anxiety about the position of cultural anthropologists in the system of colonial oppression.

In the nineteen-sixties and early 'seventies, political upheavals on university campuses and the rise of Marxism in academic circles problematized the relationship between anthropological research and colonialism. In his introduction to *Anthropology and the Colonial Encounter,* Talal Asad summarized the problem this way:

> We must begin from the fact that the basic reality which made pre-[Second World] War social anthropology a feasible and effective enterprise was the power relationship between dominating (European) and dominated (non-European) cultures. We then need to ask ourselves how this relationship has affected the practical pre-conditions of social anthropology; the uses to which its knowledge was put; the theoretical treatment of particular topics; the mode of perceiving and

> objectifying alien society; and the anthropologists claim of political
> neutrality. [Asad, 1973, 17]

The essays that followed[14] documented the ways in which colonial power supported
ethnographers. Colonial administrations provided local contacts and resources for
the ethnographers. The presence of colonial powers facilitated access to remote
areas and the peoples who inhabited them. The contributors argued that, in spite
of the liberal sentiments of most ethnographers, colonial administrators had indeed
put anthropological knowledge to work in maintaining the colonial order. These
arguments, however, did not cut to the heart of the ethnographic enterprise. At
worst, they showed ethnographers to be unreflective and naïve about relationships
of power. The essays of *Writing Culture* went deeper insofar as they purported
to show that the form of knowledge produced by ethnographers — the very way
in which it was documented and written — was undermined by their political
naïveté. The arguments are complex and nuanced; only a partial perspective can
be reproduced here.

The goal of ethnography, from Malinowski forward, was to capture "the native's
point of view." Doing so involves surmounting a problem, one which ethnogra-
phers had often recognized. The native's point of view is necessarily foreign. To
understand it, the ethnographer has to participate in the culture and learn the
language. She has to see that point of view for herself. The problem arises when
she tries to take this understanding home and express it to her colleagues. How
can this foreign way of thinking, acting, or experiencing the world be expressed
so that it is comprehensible, and yet still foreign? The contributors to *Writing
Culture* framed this as a rhetorical problem, and took the strategy for addressing
it to be a matter of ethnographic style. As Crapanzano put it, the ethnographer
"must render the foreign familiar and preserve its very foreignness at one and
the same time. The translator accomplishes this through style, the ethnographer
through the coupling of a presentation that asserts the foreign and an interpreta-
tion that makes it all familiar" [Crapanzano, 1986, 52]. Crapanzano here suggests
that there are two moments to the ethnographic monograph. First, a narrative
presents the strange and puzzling life of a foreign culture. Second, an interpreta-
tion renders it comprehensible, if not familiar. To make the narrative convincing,
the ethnographer has to establish her *authority*. She was, after all, the observer
who experienced the events depicted. Having convinced the reader of the accuracy
of her depiction, she can go on to interpret it.

Crapanzano, and Clifford before him [Clifford, 1983], argued that the process of
establishing the authority of the ethnographic narrative undermines the interpre-
tations built upon it. As ethnography had come to be written, there were several
rhetorical strategies for establishing authority. One was to make the presentation
realistic; details of the events are vividly reproduced. The author is put into the
scene through accounts of first contact or by narration of personal experiences.

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[14]The essays in *Reinventing Anthropology* [Hymes, 1969] were also an important source of the
political critique of cultural anthropology.

These strategies are complicated by the fact that the goal is to reproduce the *native's* point of view. Hence, not only do the physical comings and goings need to be described in rich detail, but the native reactions, feelings, motives, and meanings must be realistically portrayed as well. The result, Clifford and Crapanzano argued, is a construction that mixes the ethnographer's feelings and reactions with the events she observed. The realism is achieved by assembling a montage of concrete details drawn from a variety of events. To capture the subjective aspects of the culture, the ethnographer ends up speaking of feelings and beliefs that have no proper subjects. A favorite stalking-horse of this kind of criticism is Geertz' essay, "Deep Play: Notes on the Balinese Cockfight" [Geertz, 1973a]. Geertz freely wrote of "the Balinese" feelings and reactions: social embarrassment, moral satisfactions, and so on. These are never attributed to any specific person; they are generalized. This technique of abstracting the subjective away from its subjects was not invented by Geertz. It goes back to Malinowski at least, and was typical of mid-century ethnographies [Clifford, 1983].

The rhetorical techniques that establish the ethnographer's authority have several unintended effects. The narrative is a construct that represents no particular cultural events or properties. It is no more than the ethnographer's synthesis. Moreover, the interests and theoretical concerns that motivated that particular synthesis are hidden from the reader. The ethnographer herself appears in the text only as a neutral, objective bystander. Finally, the description is univocal and monolithic. The use of free voice creates a point of view that is no one's in particular. The rhetorical figures used to establish authority thus force the creation of something artificial. The problem with this construct arises in the second moment of ethnographic writing, the interpretation. In this part of the presentation, the ethnographer purports to exhibit the hidden meanings and social structures. Once we see that the text to be interpreted is nothing more than the ethnographer's conceit, the interpretation loses its force. Crapanzano writes about Geertz:

> Despite his phenomenological-hermeneutical pretensions, there is in fact in "Deep Play" no understanding of the native from the native point of view. There is only the constructed understanding of the constructed native's constructed point of view. Geertz offers no specifiable evidence for his attributions of intention, his assertions of subjectivity, his declarations of experience. His constructions of constructions of constructions seem to be little more than projections, or at least blurrings, of his point of view, his subjectivity, with that of the native, or, more accurately, of the constructed native. [Crapanzano, 1986, 74]

It is not difficult to hear the echo of Radin's critique of Kroeber and Malinowski in this passage. The epistemic problem of generalizing from particular individuals to "the culture" took on new significance against the background of political uneasiness about anthropology's position in colonialism. The ethnographer's conceit did not merely falsely represent the natives. It represented them by homogenizing

and stereotyping. These representations thus made it easier for colonial officials and others with a political agenda to exploit and dominate the local population.

A further aspect of Sapir's and Radin's criticism of ethnography was also rediscovered in the postmodern critique.[15] We have every reason to believe that the individuals within a culture differ from each other in significant ways. Monographic ethnographies suppress the contrarian, marginalized, or peripheral voices in favor of those that are dominant. By presenting the dominant self-conception of a culture as *the* culture, it misrepresents the life of the people it is trying to capture. In many, if not all cultural contexts, participants have systematically different points of view on the culture. For example, men and women may have opposed, but related, notions of what the appropriate behavior should be in a particular context. Homosexual and heterosexual members of a culture may have different views about how one should be related to extended family members. Moreover, the dominant norms of a culture may be in dispute. Different groups with different political interests might be arguing about just what the norms are or how they are to be implemented (cf. [Bourdieu, 1977, 30-72; Risjord, 2000, 168-173]). These issues are particularly pressing when ethnographers turn their attention to cases of culture change or cultural blending. In contemporary urban life, we find ourselves at the intersection of a variety of cultural practices. There is no reason to think that 'traditional' life was ever much different, especially in places where different cultures and language groups regularly interacted [Rosaldo, 1989, 26-30]. The postmodern critics recognized something that Sapir and Radin did not: such differences and disputes within a group are shot through with power relations and political ramifications. By describing one perspective on social norms as the correct one, an ethnographer is taking sides on a political issue within the culture. In the context of colonialism or post-colonial nationalism, choosing one description as 'the culture' cannot be a politically neutral act.

While the main elements of the postmodern critique seem to have become embedded in the culture of cultural anthropology, several lines of criticism have emerged in response. First, the contributors to *Writing Culture* and similar works accepted that the goal of ethnography is to capture "the native point of view," where this was understood to be something subjective and experiential. Their complaint was that ethnographic writing made the experiential basis of the anthropologist's reconstruction insufficiently transparent. The paradox of monological ethnography is that it tries to capture the subjectivity of individuals and raise it to the level of a common experience. But there is no such trans-subject subjectivity. In response, Steven Sangren argued that the puzzle can be better resolved by rejecting the presupposition that ethnography should capture the native point of view at all. Sangren did not argue that ethnography should eschew analysis of systems of belief or value; rather it should not be concerned with experience *per se* [Sangren, 1988, 417]. Geertz himself understood 'the native point of view' to be a matter of meaning and symbolism. As we have already seen, such meanings

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[15] The authors in *Writing Culture* and similar works seemed to be unaware of the degree to which their arguments had been anticipated by Radin and Sapir.

were understood to be public in the first place [Geertz, 1973c; 1983]. Indeed, by emphasizing the experiential character of the ethnographer's object, the postmodern critique may have created the very problem it sought in ethnographic writing. Mid-century ethnographers were not typically concerned with the personal experiences of their subjects. The critics seem to have read a contradiction into ethnography, and then criticized ethnographers for embracing a contradiction.

Sangren also argued that the postmodern critique entailed a problematically individualistic conception of culture [Sangren, 1988, 417-418]. The individualism has two sources. First, the critics emphasized the ethnographic importance of individual experience over the social structures or processes. Second, the postmodern critics argued that ethnographic generalizations about structures and processes failed to represent — or worse, obscured — real fissures and disputes within the communities. Together, these critical points result in a conception of culture as a dialogue among individuals in relations of power (cf. [Clifford, 1986, 15]. Sangren begins his response by remarking that

> "meaning" and "culture" are not merely the negotiations "between" subjects in acts of "communication"; such acts of communication are inevitably embedded in encompassing systems of power and meaning. These encompassing systems are related dialectically in the process of social and cultural reproduction to the "experiences" of the subjects that they encompass and that are necessary in their reproduction. [Sangren, 1988, 417]

To understand a dialogue among individuals in relations of power requires an analysis of those power relations. Any adequate analysis will have to address the conditions under which individual experiences are produced and reproduced, and this will require analyses that transcend individual experiences. Sangren goes on to apply this point to the postmodern critique itself. By privileging experience, postmodernism has simply reproduced a bourgeois ideology. In spite of their explicit attention to reflexivity, the postmodern critics are insufficiently reflexive [Sangren, 1988, 418ff]; cf. [Roth, 1989, 559].

Reflexivity was introduced by the postmodern ethnographers as a way of addressing the political aspects of ethnographic writing. As already noted, the postmodern critique of ethnography derived some of its force from political anxieties about the position of ethnographers. The postmodern critics argued that characterizing a culture in writing was a political act, and that the standard modes of ethnographic writing obscured the political position of the author. Reflexivity was the solution to the problem; the position of the author was to be made explicit in the text. Rather than effacing the author, her engagement, attitudes, reflections, and motivations were to be made explicit. Sangren and Roth both pointed out that rhetorical changes, such as emphasizing the active voice or writing in the first-person, are insufficient to address the real problem. Roth remarks that "Clifford suggests that tortured self-consciousness regarding the social construction of knowledge is somehow emancipatory, but we await any demonstration that

such accounts reveal much beyond the ambivalences of their authors" [Roth, 1989, 559]. Moreover, if the political position of ethnographers *vis-à-vis* colonialism is the problem, then reflexivity is unable to either expose or fix it. Reflexivity does not aid recognition; ethnographers were rarely self-conscious about the relationship between their research and the colonial authorities (as the essays in *Anthropology and the Colonial Encounter* showed). Nor does reflexivity resolve the political issues; for that one needs a critical stance.

In the light of subsequent work on the relationship of values to scientific research, the political worries of the postmodern critics seem superficial. Indeed, the criticism seems buoyed by the idea that science can only be objective if it is entirely free of values. Research in the philosophy of science during the last two decades of the twentieth century, and feminist philosophy of science in particular, has made substantial progress on this issue [Harding, 1991; Longino, 1990]. It is now a commonplace that all scientific research presupposes values and that good science may have an explicit moral or political orientation. Thus, while the postmodern critics were right to criticize the political naïveté of earlier ethnographers, they were wrong to conclude that this vitiated either their results or the form of their presentation. Moreover, while reflexivity may be an important part of objectivity, more is required. In particular, the social organization of the scientists themselves, including the way in which work is reviewed and power is shared among inquirers, is a crucial aspect of objectivity.

While Sangren and Roth effectively questioned the presuppositions of the postmodern critique, there is an important claim left untouched by their arguments. Indeed, this element of the postmodern critique had the most far-reaching effects on ethnography. Even if we agree that any discussion of experience and communication must be embedded in an understanding of the social structures that make experience and communication possible, the postmodern critique problematized the character of those social structures. Any set of norms, institutions, or other social structures is (or might be) subject to dispute within the group itself. We cannot return to the sanguine generalizations about 'the' culture of mid-century ethnography. We must, it seems, give up the myth of univocal culture. But what can ethnography be if there is no culture?

7 TWO CONCEPTIONS OF CULTURE; TWO CONCEPTIONS OF ETHNOGRAPHY

We have seen that anthropological thought has vacillated between two ways of conceptualizing culture and its study. One apogee is individualistic. Early Boas, Sapir, Radin, the new ethnologists, and the postmoderns have all understood cultures to be more-or-less arbitrary collections of individuals. The fundamental cultural phenomena are the representations, experience, or other "traits" of a person insofar as these are similar to other persons or passed down among generations. On this kind of view, the goal of ethnography is to capture and record the experiences or representations of individuals. It is as concerned with individual

differences as it is with commonalities. The other extreme is holistic, and here we find Malinowski, Kroeber, and Geertz. Cultural phenomena, on this view, are independent of individual experience or representation. Indeed, individual experiences or representations get their sense from the cultural context, and culture is something in which the individuals participate. Ethnography thus needs to make hypotheses about rules, institutions, symbols, or other cultural forms, and its explanations emphasize the ways in which norms, practices, and institutions are interrelated. From the perspective of the early twenty-first century, we can see in the twentieth century a gradual, dialectical development of these two positions. Both are represented in contemporary cultural studies. This final section will outline the conceptual and epistemological challenges facing each.

Let us call the conception of culture and ethnography that emphasizes individual experience or representation "interactionist." Proponents of this view do not espouse a simple-minded methodological individualism. They agree that individual representations and experiences are the product, at least in part, of interactions among individuals. At the same time, they are suspicious of any attempt to reify cultural phenomena. Individuals engage with one another in acts of communication or negotiation, and these fundamentally shape the individual's representations. The similarities among individuals that support cultural generalizations are produced by patterns of communication. Interactionist conceptions of culture are shared by anthropologists with a wide variety of theoretical and methodological commitments. We saw above how the postmodern critique of ethnography resulted in an interactionist conception of culture. Since the "new ethnographers" ended up embracing an interactionist conception of culture, it is perhaps no surprise that their intellectual progeny have as well. By blending the cognitive sciences with anthropology, writers like Scott Atran [2002] and Dan Sperber [1996] have developed a powerful research program. While they have little sympathy for the postmodern turn in anthropology, they agree that culture is to be understood in terms of the distribution of representations among individuals.

The alternative to an interactionist conception of culture is contemporary practice theory.[16] Practice theorists recognize the postmodern critique of the reification of culture. As they see it, the problem of traditional ethnography was the monolithic presentation of a culture as a single, integrated body of practices. In response, they think of practices as something trans-individual, but not monolithic. A group of people engages in a variety of practices, and these give rise to norms, rules, and institutions. But these practices need not be, and in general are not, univocal or well-integrated. There may be important structural cleavages embedded in different practices, and these may instantiate asymmetric power relations. Hence, one can accept the postmodern reservations about homogenizing intra-cultural differences without rejecting the idea that something trans-individual gives action and thought its significance. In anthropology, Bourdieu [1977] and Ortner [1984]

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[16]For a more detailed discussion of practice theory, see Rouse, this volume.

are leading practice theorists; philosophically important developments have come from Brandom [1994] and Rouse [2002].[17]

The interactionist and practice conceptions of culture have very different methodological ramifications. To conclude this essay, we will explore four issues: the question of how understanding social phenomena is different from understanding natural phenomena, the scope of ethnography, the place of traditional ethnographic methods, and the relationship between emic and etic explanations.

At the outset of this essay, I distinguished between 'dualist' and 'monist' epistemologies. A monist epistemology holds that there is a single form for all empirical inquiry. This kind of view has been traditionally associated with positivism. However, such a view need not hold that the natural sciences are the model for all inquiry. I have argued elsewhere that knowledge in the natural and social sciences can be represented in a unified way without doing violence to those forms of understanding that are distinctive of social inquiry [Risjord, 2000]. A dualistic epistemology holds that there are deep differences between the social and natural sciences. Proponents of *verstehen* and hermeneutic methods have held such a view, as have philosophers as different as Winch [1958] and Davidson [1984]. Proponents of the interactionist view typically argue for a deep difference between the epistemology of the social and the natural worlds. Postmodern ethnographers embraced a traditional *verstehen* epistemology and emphasized the importance of hermeneutics. It is perhaps surprising to note that contemporary cognitive anthropology cleaves to this doctrine as well.[18] In both cases, the commitment arises from the way in which experience or representation is conceptualized. Experiences and representations are identified by their content, and the goal of ethnography is to describe this content. Proponents of the interactionist view reject the Wittgensteinian critique of content, and as a result they hold that persons have a kind of direct access to the meaning of their representations or the content of their experience. There is nothing analogous to this in the natural sciences. Hence, ethnography must use methods that are fundamentally different from those deployed in the natural sciences.

Proponents of the practice view, on the other hand, tend to be epistemic monists. This is partly because practice-theoretic analyses of the natural sciences tend to downplay differences between the social and the natural sciences (cf. [Rouse, 1987; 1996]). The deeper reason is that practices and the phenomena they give rise to, such as norms and institutions, outrun the actions or speech of individuals. As Malinowski recognized, any identification of a norm must be hypothetical, and such a hypothesis must be tested against the responses of the individuals. Ethnography, then, must have the same epistemic form as any inquiry where hypotheses are formed and tested. In *Woodcutters and Witchcraft*, I argued that the ethnographic description of norms and practices could be understood as a case of inference to the best explanation. This assimilated the

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[17]Also see the essays in [Schazki, *et al.*, 2001].

[18]For analysis and criticism of this presupposition in Sperber's and Atran's work, see Risjord [2004].

confirmation of ethnographic interpretations to the confirmation of other empirical theories [Risjord, 2000]. There is however, an important challenge to this line of thought. Stephen Turner has argued persuasively that appeal to practices is pseudo-explanatory [Turner, 1994]. While there is no room here to engage the debate, if ethnographic appeal to practices is to be explanatory, Turner's arguments need to be met.[19]

Part of the disagreement between interactionists and practice theorists about the epistemic status of ethnography has to do with their different visions of the goal of ethnography. For interactionists, the object of ethnography is to describe experience or representation. But, because interactionists are suspicious about cultural generalizations, it is difficult to see how ethnography can rise above the description of individual experiences. Torturing the well-known frontispiece of Radin's *Method and Theory of Ethnology* [Radin, 1987/1933], one might say that by and by ethnography will have the choice between being biography or nothing. This anxiety about generalizations was played out in the postmodern experiments with poly-vocal ethnographies. It has also been influential on "qualitative" research in the health sciences and education. In these disciplines, which never deployed a robust concept of culture in the first place, it has become common for researchers to describe the experiences of very small groups of individuals. One wonders, however, what the value of such descriptions might be. It is not clear that this kind of "qualitative research" could provide data for any interesting ethnology, social theory, or theory of health behavior. For practice theorists, by contrast, ethnography needs to be a generalizing form of theory. Information about what individuals (or small samples of individuals) think is useful only insofar as it exemplifies or provides evidence for larger trends.

The third issue about ethnography on which it is instructive to compare interactionists and practice theorists is the continued relevance of traditional ethnographic methods. As we have seen, classical ethnography was characterized by participant observation, holism, and open-ended interviews (in the native language). What becomes of these in twenty-first century ethnography? Participant observation is one way of gathering reams of fine detail, but it is not the only way. Participant observation is necessary only if one thinks of a culture as composed of norms. By participating, the ethnographer can get a feel for how people respond to her actions, and hence a feel for how the norms influence action. Interactionists do not understand culture to be normative in this sense. Hence, from its perspective, there is no longer anything special about participation. The same results may be obtained by interviewing a sample of individuals or by analyzing tapes of their conversations. Practice theorists, on the other hand, hold onto the view that practices are normative. Hence, participation is a crucial part of coming to understand a group of people.

Holism was important to mid-century ethnography because cultural anthropologists were committed to the idea that, normally, cultures were integrated collections of symbols, institutions, and practices. This view was profoundly criticized,

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[19]See Rouse, this volume, for further discussion of this issue.

as we have seen, and it is difficult for any ethnographer to treat holism in the way it was treated earlier in the century. The interactionists who take the postmodern criticism most seriously restrict holistic interplay to the ideas of an individual, or perhaps individuals in conversation. Practice theorists are also careful about holistic interpretation, since they do not want to say that practices form a coherent set. They are, however, free to give holism a wider range in ethnography. Practices both productively and destructively interfere with each other. It may be important, for example, that a practice is maintained so as to suppress or marginalize counter-practices. Recognizing this kind of conflict might be an important part of understanding the practices of a group. On a practice theoretic conception of culture, the epistemic appeal to holistic relationships among practices can become much more complicated and nuanced than it was in mid-century ethnography.

What of unstructured interviews? For Malinowski and those who followed him, unstructured interviews were partly necessitated by the commitment to holism. Because the ethnographer did not know how different practices interacted, it was important not to force the responses into a predetermined formula. For any ethnographer, unstructured interviews remain useful because they let the subjects judge the significance of different ideas, themes, or topics. For ethnographers with an interactionist conception of culture, unstructured interviews will be the best way to capture the subject's experience. For practice theorists, unstructured interviews have additional value. Practices interact in ways that are unexpected. In concert with participant observation, unstructured interviews can uncover hidden presuppositions and relationships.

The final point on which to compare interactionist and practice theoretic ethnography is in their attitudes toward emic and etic explanations. The question is whether, and to what extent, interpretations couched in the subjects' own conceptual framework (emic) and explanations drawn from evidence or conceptualizations that are outside the current knowledge of the subjects (etic) can support one another. Harris' example, described above, of household fissure among the Bathonga is an example of emic and etic analyses mutually supporting one another, which is sometimes called "triangulation." On an interactionist view of culture, there is little or no role for triangulation in ethnography. For ethnographers of the postmodern stripe, describing the subjects' experience is the primary object of ethnography. Nutritional or ecological analyses have little or no role to play.[20] Triangulation does play a role in ethnography motivated by practice theory. The shape of a practice is influenced by power relations, economic relationships, or underlying psychological mechanisms. Hence understanding the practices that give rise to norms and meaning will require evidence from other kinds of analysis. There is thus potentially a relationship of mutual support between traditional ethnographic techniques (participant observation, open ended interviews) and economic or nutritional analyses, game-theoretic concerns, psychological or sociological experiments in the field, and so on.

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[20]In [Risjord, 2004] I argue that cognitive anthropologists also cannot give any robust role to triangulation in ethnography.

8 EPILOGUE

As this essay was being written, *American Anthropologist* published a small collection of essays.[21] Written by relatively young anthropologists, the essays articulated and defended a "neo-Boasian" approach to culture and ethnography. They argued that Boas and his students had a more subtle and flexible approach to culture than the postmodern critics had recognized. Moreover, they argued for possible alliances between the Boasians of the twentieth century and the practice theorists of the twenty-first. At the moment of this writing, it is not clear whether neo-Boasianism will capture the attention of ethnographers and cultural anthropologists. The publication of this collection of essays in such an important journal, however, demonstrates clearly that the issues surveyed above are very much alive.

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CATEGORIES AND CLASSIFICATION
IN THE SOCIAL SCIENCES

Warren Schmaus

1   INTRODUCTION

Social scientists have been proposing hypotheses regarding the relationship between categories and society ever since Émile Durkheim and Marcel Mauss. These hypotheses range from the cognitive relativist claim that there are culturally constructed sets of categories that give rise to incommensurable perceptual realities to the structuralist claim that there is a single, underlying, unconscious structure of categorical concepts that is the same for all cultures. Before we can consider the arguments and evidence offered in support of these claims, we need to get clear about what is meant by "categories". Then we need to clarify exactly what is the relationship that these hypotheses claim to hold between society and the categories. Once we have introduced all of the relevant distinctions, we will find that the arguments and evidence support the following claims: that some of the most fundamental categories of thought have important social functions, and that in order for these categories to perform these functions, there must be some cultural system for representing or naming them.   The relative contributions of innate cognitive mechanisms and cultural processes to this conceptual repertoire need to be worked out through a cooperative investigation by the social and cognitive sciences, including the cognitive neurosciences.

There are at least seven different senses of the term category. First, there are lexical categories, which are simple kinds of things, properties, and actions such as plants, colors, and crimes. These are distinguished from the biologists' taxonomical categories, which include kingdom, phylum, class, order, family, genus, and species. The classificatory concepts under each of these categories, such as the plant and animal kingdoms, are called "taxa" rather than "categories".

Aristotle's concept of a highest predicable is a third sense of "category". His categories include the concepts of substance, quantity, quality, relation, place, time, being-in-a-position (position or posture), having (state or condition), doing (action), and being-affected (affection, passivity).[1] They differ from both lexical and taxonomical categories. For Aristotle, plant and animal are not categories but

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[1] *Categories* 1b25-27. I am relying on J. L. Ackrill's translation, Aristotle [1963], and following the standard scholarly practice of referring to passages in Aristotle according to the page, column, and line number of the nineteenth-century Berlin Academy edition of his works.

kinds of substances or what he calls "secondary substances", as opposed to "primary" or individual substances, while the taxonomic category "kingdom" would be a kind of kinds of substance.

Kant's pure concepts of the understanding that structure experience are a fourth sense of "category". These are not quite Aristotle's highest predicables, since Kant groups his twelve categories under four super-categories. Under quantity, Kant lists the categories unity, plurality, and totality; quality includes the categories reality, negation, and limitation; relation includes substance (inherence and subsistence), cause (and effect), and community (reciprocity between agent and patient); and modality includes possibility (vs. impossibility), existence (vs. non-existence), and necessity (vs. contingency).[2] To say that Kant's categories structure experience is not to suggest that they are some sort of filter, framework, or conceptual scheme through which we perceive the world. The categories are the concepts that are found in experience, not those that generate it. As Kant explained in the *Prolegomena*, the categories are not part of an empirical psychology but rather belong to a critique of cognition that should be undertaken as a preparation for doing psychology [1783, 4: 304]. However, over the last two centuries philosophers have introduced variations of Kant's concept of a category, with many of them understanding it as having a psychological meaning. The notion of psychologically necessary conditions of experience thus constitutes a fifth sense of the concept of a category, to be distinguished from Kant's logically necessary conditions of experience.

Kant arrived at his table of the categories by analyzing the different forms that judgment could take, as he thought that the same concepts or "logical functions" underlie both the forms of judgment and the categories. That is, the same logical functions that give unity and structure to our experience of the world also give structure and unity to our judgments about it. One might then expect to find some relationship between Kant's categories and grammatical categories. Grammatical categories, such as subject, predicate, number, case, or tense, overlap with Kant's categories but are not identical with them. For instance, there does not appear to be a grammatical category that corresponds with Kant's category of limitation. In addition, gender is a grammatical category, at least in some languages, but not one of Kant's categories. Grammatical categories thus appear to be a sixth type of category.

Finally, there is a seventh kind of category that I will call "Durkheimian categories". These are the fundamental concepts that facilitate the normal functioning of human society. For Durkheim, they included space, time, causality, substance, genus, quantity, and personhood, among others. They overlap but are not identical with Aristotelian, Kantian, and grammatical categories.

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[2][Kant, 1781/1787, A80/B106; 1783 4: 303]. In referring to passages in the *Critique of Pure Reason*, I am following the standard reference system of providing page numbers in the original first edition of 1781, prefixed with the letter *A*, followed by page numbers in the second edition of 1787, prefixed with the letter *B*. For references to the *Prolegomena*, I am following the standard reference system of providing the volume and page number of the German *Akademie* edition of Kant's works [Kant, 1902].

In addition to these distinctions among different senses of "category", the concept of a category must also be distinguished from the way in which it is represented, either in an individual's mind or in a system of cultural or collective representations. Many social scientists would refuse to distinguish a concept from its cultural representation. But to refuse to grant this distinction would be to turn the provocative claim that there are different categories in different cultures into an uninteresting tautology. We may also want to distinguish cultural from collective representations. The Durkheimians originally used the term "collective representation" to refer to a shared mental entity. But it has also been used to refer to such public representations as works of art, songs, dances, spoken words, emblems, symbols, and so forth. Perhaps it would be better to call these things cultural rather than collective representations in order to avoid the confusion with Durkheim's mental entities.

The distinction between an individual and a cultural representation of a category can help to clarify the issue of the relationship of cultural anthropology and the sociology of knowledge to psychology and the cognitive sciences. For instance, Alfred Reginald Radcliffe-Brown and his followers understood Durkheim as having advocated that, since the categories are cultural products and thus fall completely within the domain of the social sciences, anthropology and sociology may completely ignore psychology [Gluckman, 1963, 2–3; Jahoda, 1982, 33, 40]. We may decide instead that the social sciences concern themselves with cultural representations of the categories while the cognitive sciences are interested in individual mental representations of them. The relationship between cultural and individual mental representations then becomes a research topic for a collaborative effort among the social and cognitive sciences.

We also need to distinguish two general sorts of claims regarding the relationship of these various conceptual entities to societies: (1) claims about the social or cultural origins or causes of the categories or their representations, and (2) claims about their social or cultural roles or functions. Public representations of the categories may have both social functions and cultural origins. These public representations of the categories may be what allow the categories to exercise their social functions. However, the fact that the categories have these social functions does not constitute evidence that the categories are of social origin. In addition, even if it could be established that the categories are cultural products and that public representations of the categories are culturally variable, it would not follow that the categories themselves are culturally variable. If Durkheim were right that there are certain fundamental concepts that are necessary to the normal functioning of society, each culture would have found some way or other of representing this same set of concepts.

2 DURKHEIM AND MAUSS

The earliest claims regarding the social character of the categories may be found in Durkheim and Mauss's [1903] paper, "On Some Primitive Forms of Classifi-

cation: Contribution to the Study of Collective Representations". Drawing on ethnographic evidence from Australia, North America, and elsewhere, they advanced the thesis that "the classification of things reproduces the classification of men" [1903; 1969, 402; t. 1963, 11].[3] They argued not only that the societies they considered rank human beings and natural things under the same totems, but also that the very classificatory concepts of genus and species, the concepts that allow us to subsume one group under a larger group, were originally constructed on the model of human social groupings. According to Durkheim, "Neither the spectacle of nature nor the mechanism of mental associations could furnish us with the idea of it. Hierarchy is exclusively a social thing" [1912, 211; t. 1995, 149].

We can agree with Durkheim that we do not obtain this idea of hierarchy simply by observing nature or by the association of ideas. However, psychology in the last ninety years has moved well beyond the associationism Durkheim rejected and whether the idea of hierarchy is social or psychological in origin remains an empirical question. In more recent times, David Bloor [1982] has attempted to revive Durkheim and Mauss's primitive classification thesis by arguing that natural classifications that reflect a society's structure are maintained because they serve certain social interests, offering evidence from the history of science to support this interpretation. But even if Bloor were right, his thesis would touch on only the social uses of systems of classification, and not the more fundamental question of the source of hierarchical thinking, as Steven Collins [1985, 70] reminds us. A test implication of Durkheim's thesis that the idea of a hierarchy is social in origin would be that non-hierarchical societies such as the San would not have hierarchical systems of natural classification. However, there is no evidence to support this. On the contrary, ethnographic evidence suggests that hierarchical thinking is culturally universal. Beginning with the work of Berlin, Breedlove, and Raven [1973], ethnobiologists have found that folk taxonomies universally have a minimum of three categories or three hierarchical levels of taxa: (1) the "unique beginner", the highest, like plant or non-human animal, corresponding roughly to kingdom, (2) life-form, such as fish, bird, tree, grass, vine, bug, worm, and (3) generic-specieme. In any given locale, there is typically one species representing a genus, so species and genus are extensionally equivalent. Eleanor Rosch [1978, 28] argues that there may be a psychological explanation for hierarchical thinking. The task of a system of classification is to provide the maximum amount of information with the minimum amount of effort. It may simply be more efficient to include the characteristics of wings and feathers under the category "bird" than to have to repeat this information for each sort of bird.

Be that as it may, Durkheim and Mauss went on to generalize their claim about the social and cultural origin of classificatory concepts to include also what they took to be the categories of space, time, cause, and substance. In *The Elementary Forms of Religious Life*, Durkheim said:

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[3]I provide my own translations from Durkheim's French texts. Page numbers for the most recent English translations of each work are given for the convenience of the reader.

> There are, at the root of our judgments, a certain number of essen-
> tial notions that dominate our entire intellectual life; they are those
> that philosophers, since Aristotle, have called the categories of the un-
> derstanding: notions of time, space, genus, number, cause, substance,
> personhood, etc.   They correspond to the most universal properties
> of things. They are like the solid framework that encloses thought; it
> appears that it cannot free itself from them without destroying itself,
> because it seems we cannot think of objects that are not in time or
> space, which are not numerable, etc. Other notions are contingent and
> changeable; we conceive that they may be lacking to a person, a so-
> ciety, an epoch; the former appear to be nearly inseparable from the
> normal functioning of the mind [1912, 12–13; t. 1995, 8–9].

Although Aristotle is mentioned and Durkheim's reference to categories of the
understanding at the root of our judgments sounds very Kantian, Durkheim's
concept of a category differs from both Aristotle's and Kant's. Even his list of cat-
egories is different than both Kant's and Aristotle's, neither of whom, for instance,
included the concept of personhood as a category.[4] In addition, space and time
are forms of intuition for Kant rather than categories. Furthermore, Aristotle's
categories were not part of a theory of the understanding or forms of judgment
as they were for Kant. Aristotle's categories concern the classification of words
and the things to which they refer, considered in isolation from the judgments in
which these words may be used.[5]

Durkheim explained in several places that he was not using the term "category"
in the same sense that Kant had used it, that is, to refer to the necessary con-
ditions of experience. He said, for "the recent disciples of Kant... the categories
preform the real, whereas for us, they recapitulate it. According to them, they
are the natural law of thought; for us, they are a product of human art."[6] In this
passage, it appears that Durkheim, like many nineteenth-century philosophers,
understood the Kantian categories as psychologically necessary rather than logi-
cally necessary conditions for experience.[7] For Durkheim, however, the categories

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[4]The French *personnalité* is typically translated as "personality". In English, however, this has
additional meanings not intended by Durkheim. His inclusion of personhood among the categories
probably reflects the influence of Charles Renouvier (1815-1903), who listed nine categories in
order from simplest and most abstract to most complex and concrete: relation, number, position,
succession, quality, becoming, causality, finality, and personhood (*personnalité*) [1912, vol. I,
120ff]. Mauss [1938] departs from this usage in referring to the category of the person (*personne*)
instead.

[5]As Aristotle put it, "things said without any combination" refer to things that fall under
one or more of the categories (1b25). By "things said without any combination" he meant
expressions considered apart from their combining with one another to form "affirmations" or
judgments (2a4-10).

[6][Durkheim, 1909, 757 and n. 1], [t. 1982, 239–40 and n. 1]. This is a published article
that was subsequently edited to serve as the introduction to *The Elementary Forms*. Although
this particular quoted passage did not make it into *The Elementary Forms*, the sense of it is
consistent with the passage from this latter work quoted below.

[7]This is explained more fully for the case of France in Schmaus [2004].

were not the necessary conditions of experience in either sense, but rather the collective representations of these concepts. In *The Elementary Forms*, he distinguished the categories of space, time, causality, and genus in his sense of the term "category" from the individual's sense of space, duration, regular succession, and resemblance. According to Durkheim, "the relations that the categories express exist, in an implicit manner, in individual consciousnesses" [1912, 628; t. 1995, 441]. An individual human being, he stated, has no more need of the categories to find his or her way in the world and guide his or her actions than an animal does [1912, 632; t. 1995, 444]. He thought that even the most primitive systems of classification presuppose the ability to recognize resemblances among the particular things the mind perceives [1912, 206; t. 1995, 146].

In *The Elementary Forms*, Durkheim defended his theory of the social causes and origins of the categories by arguing that it provides the best explanation of the universality, necessity, generality, and cultural variability of the categories. Empiricism, he said, cannot explain the universality, generality, and necessity of the categories, and the *a priori* philosophy cannot explain their cultural variability [1912, 18–21; t. 1995, 12–14]. On the other hand, he argued, the sociological theory of the categories, in which they are identified with their collective or cultural representations, can explain all of these things. The social character of the categories explains the necessity with which they impose themselves on our thought. According to Durkheim, these categories are necessary for social life. In order for society to maintain itself, it must impose these norms of thought on individuals: "If thus, at every moment of time, men did not agree on these essential ideas, if they did not have a homogeneous conception of time, space, cause, number, etc., all agreement among minds and consequently all common life would become impossible" [1912, 23–24; t. 1995, 16]. Durkheim argued that the necessity with which the categories are thus imposed on our thought is not a physical or metaphysical necessity, since the categories are variable with respect to place and time, but a kind of moral necessity, analogous to moral obligation [1912, 24–25; t. 1995, 16–17]. The collective character of the categories explains their universality, that is, the fact that they are communicable from one individual to another – and even, at least in principle, to all individuals. That the categories are produced collectively and over many generations explains the fact that their extension is more general than the experience of any individual [1912, 619–21; t. 1995, 435–36].

Many of Durkheim's critics have argued that his sociological theory of the categories is circular: that is, they have accused him of attempting to derive the necessary conditions of experience from social and cultural experience.[8] These critics have apparently overlooked the passages quoted above where Durkheim distinguished his concept of a category from the Kantian concept of a necessary condition of experience. A more serious objection is raised by Terry Godlove. He

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[8]This objection was raised by Durkheim's earliest critics outside of France, including Charles Elmer Gehlke [1915, 52], Edward L. Schaub [1920, 337], and William Ray Dennes [1924, 32–39]. More recently, it has been raised by Terry Godlove [1986, 392–93; 1989, 32] and Steven Lukes [1973, 447].

points out that Durkheim's argument places conflicting demands on a theory of categories, criticizing the empiricists for not explaining their universality and necessity and at the same criticizing the a priori philosophy for failing to account for their cultural variability [Godlove, 1989, 43–44]. This raises the question how Durkheim's own theory can meet these conflicting requirements, without making inconsistent or ambiguous assumptions. The problem stems from Durkheim's identification of the categories with their culturally variable collective representations. The best way to resolve the difficulty is to insist on distinguishing these things: the categories may be universal, necessary, and general while their representations are culturally variable. For instance, culturally variable systems of measurement may be regarded as the cultural representations of space and time that presuppose these categories, which are culturally universal. Similarly, systems of natural classification may be culturally variable, while the category of genus that they presuppose is culturally universal.

In fact, Durkheim and Mauss provided evidence of the cultural variability only of representations of the categories, not of the categories themselves. For example, they pointed to the way that the Zuñi divide space in seven directions, each named for the clan that occupies the corresponding section of the circular campsite when the entire tribe gathers [1912, 16; t. 1995, 11; Durkheim and Mauss, 1903; 1969, 425ff; t. 1963, 42ff]. The Zuñi thus provide ethnographic evidence for the cultural variability only of ways of representing space, not of the category of space itself. Similarly, in *The Elementary Forms* Durkheim wrote about different cultural conceptions of causality. He brought forth ethnographic examples of collective representations of causal powers in nature, such as the Sioux notion of wakan, the Iroquois notion of orenda, and the Melanesian notion of mana [1912, 290–92; t. 1995, 205–6]. Ways of representing causality may vary even among social groups within a larger society. As Durkheim pointed out, the idea of causality is not only different for the ordinary person than it is for the scientist, but is different even in different branches of science, such as physics and biology [1912, 527 n. 1; t. 1995, 373 n. 30]. Not only do different people in the same society have different conceptions of causality. One could add that one and the same individual may even use different conceptions of causality on different occasions.[9]

For there to be cultural variability in the categories and not just in their cultural representations, there would have to be cultures that totally lack categories that other cultures have, or that have categories not found in other cultures. Although in *The Elementary Forms* Durkheim maintained that there is a culturally universal set of categories, he subsequently equivocated on this question. In his lectures on pragmatism, he said: "We can no longer accept a single, invariable system of categories or intellectual frameworks. The frameworks that had a reason to exist in past civilizations do not have it today" [1955, 149; t. 1983; 71].

Mauss appears to have agreed with his uncle's latter position, claiming that

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[9]Indeed, people are capable of making so many different kinds of causal judgments that Pascal Boyer [1992; 1994 ch. 5] has questioned whether it makes sense to talk about cultural conceptions of causality at all. This is an issue I shall take up later.

wholly different categories may be found in different cultures. In a widely quoted passage, he said, "Above all it is essential to draw up the largest possible catalogue of categories; it is essential to start with all those which it is possible to know man has used. It will be clear that there have been and still are dead or pale or obscure moons in the firmament of reason" (Mauss, [1924] 1979, 32). Among the concepts that were formerly but are no longer categories Mauss included big and small, animate and inanimate, and right and left. He also suggested that the category of substance derived from the concept of food (Mauss, [1924] 1979, 32). In his last major essay, he argued that what he regarded as the category of the person is historically derived from the notion of the role played by an individual in sacred dramatic rituals [Mauss, 1938].

However, in making the claim that the categories are culturally variable, Mauss equivocated over what he meant by a category. His animate and inanimate are lexical categories, not fundamental categories in either Aristotle's or Kant's senses. Similarly, right and left are not fundamental categories either, as they may be subsumed under either the category of quality, as in right or left hand, or under space, when considered as directions. Big and small may be subsumed under either quantity or relation. With regard to what he said about substance, it is not clear whether it is the category itself or its cultural representation that derives from the concept of food. Mauss's category of the person appears to combine at least three notions: Aristotle's primary substance, or the notion of an individual; a secondary substance, specifically that of a human being; and particular, culturally variable, moral qualities that are assigned to human beings. It was the last of these that interested Mauss. In his essay on the category of the person, he explained that his topic is neither the grammatical category of the first person singular nor the psychological sense of self. With respect to the latter, he said, "there has never existed a human being who has not been aware, not only of his body, but also at the same time of his individuality, both spiritual and physical" [1938; t. 1985, 3]. Just as Durkheim had argued that one does not require a cultural representation of space in order to orient him or herself, or a cultural representation of genus to recognize that two things are similar in appearance, Mauss thought that one's sense of self requires no concepts from his or her culture. Mauss was concerned not with this sense of self but rather with the various forms that the concept of the self has taken on in cultural systems of law, religion, customs, social structure, and thought generally (ibid.). In other words, Mauss's project was to demonstrate the variability only of cultural representations of the category of the person, not of the category itself, if we can agree that this is indeed a category. In sum, what Mauss has shown to be culturally variable are either lexical categories or cultural representations of more fundamental categories, not fundamental categories such as space, time, causality, and substance.

3   LINGUISTIC DETERMINISM AND THE CULTURAL CONSTRUCTION OF REALITY

Edward Sapir may have been the first to express the hypothesis of linguistic determinism, according to which cultural differences in linguistic categories will affect the ways in which people of different cultures perceive the world. In a paper first published in 1929, Sapir made the following claim:

> The fact of the matter is that the "real world" is to a large extent unconsciously built up on the language habits of the group. No two languages are ever sufficiently similar to be considered as representing the same social reality. The worlds in which different societies live are distinct worlds, not merely the same world with different labels attached [Sapir, 1949, 162].

Sapir meant this claim to include not merely the perception of social reality, which is perception only in a metaphorical sense, but also actual visual perception:

> Even comparatively simple acts of perception are very much more at the mercy of the social patterns called words than we might suppose. If one draws some dozen lines, for instance, of different shapes, one perceives them as divisible into such categories as "straight", "crooked", "curved", "zigzag" because of the classificatory suggestiveness of the linguistic terms themselves. We see and hear and otherwise experience very largely as we do because the language habits of our community predispose certain choices of interpretation [Sapir, 1949, 162].

In this passage, however, Sapir's examples are all of lexical categories such as "straight" or "crooked". Sapir appears to have thought that these variable classificatory concepts structure perception, much as Kant had thought that more fundamental categories such as substance and causality structure perception, except that Sapir appears to have understood structuring perception in a psychological sense. Furthermore, Sapir appears to have held that these lexical categories are culturally variable.

Many among the subsequent generation of cultural anthropologists appear to have accepted something like Sapir's linguistic determinism. Max Gluckman, for instance, included lexical categories such as shapes and colors among the categories that construct reality for us:

> From infancy, every individual is moulded by the culture of the society into which it is born. All human beings see, but we know, for example, that how they see shapes and colours is to some extent determined by this process of moulding. More than this, their ability to describe their perceptions depends on the categories contained in their respective languages. [Gluckman, 1949-1950, 73–74].

Edmund Leach extended this hypothesis to include bushes and trees:

> I postulate that the physical and social environment of a young child is
> perceived as a continuum. It does not contain any intrinsically separate
> "things". The child, in due course, is taught to impose upon this
> environment a kind of discriminating grid which serves to distinguish
> the world as being composed of a large number of separate things, each
> labeled with a name. This world is a representation of our language
> categories, not vice versa. Because my mother tongue is English, it
> seems self-evident that bushes and trees are different kinds of things.
> I would not think this unless I had been taught that it was the case. . .
> Each individual has to learn to construct his own environment in this
> way. . . [Leach, 1964, 34–35]

Mary Douglas [1970, 20], perceiving an analogy between Durkheim's sociolog-
ical theory of the categories and Sapir's linguistic determinism, generalized this
thesis to include the effects not only of language but also of all forms of cultural
representation on our perception of reality. David Schneider provides perhaps the
clearest statement of this cultural constructionist thesis that I have found:

> The world at large, nature, the facts of life, whatever they may be,
> are always parts of man's perception of them as that perception is
> formulated through his culture. The world at large is not, indeed,
> it cannot be, independent of the way in which his culture formulates
> his vision of what he is seeing. There are only cultural constructions
> of reality, and these cultural constructions of realities are decisive in
> what is perceived, what is experienced, what is understood. . . Meaning
> is thus not simply attributed to reality. Reality is itself constructed by
> the beliefs, understandings, and comprehensions entailed in cultural
> meanings [Schneider, 1976, 204].

On this cultural constructionist view, we would be faced with an incommen-
surability of cultures much like the incommensurability of paradigms by which
Thomas Kuhn characterized the history of the sciences. In *The Structure of Sci-
entific Revolutions*, Kuhn proposed that the categories that shape perception or
"world view" vary even among scientific communities. As these perceptual cate-
gories take their meanings from paradigms that are "incommensurable" with one
another, "The proponents of competing paradigms practice their trades in dif-
ferent worlds. . . . Practicing in different worlds, the two groups of scientists see
different things when they look from the same point in the same direction" [Kuhn,
1970, 150]. In more recent writings, he described his position as "a sort of post-
Darwinian Kantianism" [Kuhn, 1991, 12; 2000, 104]. He saw his position as Kan-
tian insofar as he regarded taxonomies of kind concepts, like Kantian categories, as
preconditions of possible experience. For Kuhn [1991; 1993; 2000, passim], these
taxonomies include natural kinds, artifactual kinds, social kinds, kinds of person-
ality, and so on. His position is "post-Darwinian" insofar as it allows for variability

in these categories: "But lexical categories, unlike their Kantian forebears, can and do change, both with time and with the passage from one community to another" [Kuhn, 1991, 12; 2000, 104].

However, the degree to which perception is affected by the sorts of lexical categories Kuhn has in mind is debatable, to say the least. Although we may find that certain substance concepts such as caloric and phlogiston are historically variable, these are highly abstract, theoretical concepts rather than perceptual categories. With regard to past concepts of the most basic substances of which things are thought to consist – such as earth, air, fire, and water; or salt, sulfur, and mercury – although these may seem to be perceptual categories, in their role of basic building blocks of matter, they, too, are abstract, theoretical concepts. We do not literally "see" the "earth" that was thought to be a constituent of human and animal bodies. Other new lexical categories that have been introduced by scientific revolutions, such as the pendulum, may affect the way in which we perceive the world but only in a metaphorical sense and have a more direct effect on the way in which we describe the world. That is, scientists after Galileo did not literally see something different when they looked at a swinging chandelier, as Kuhn would have it, but understood what they were observing in an entirely new way.

Not only has Kuhn extended the claim that thought and perception are structured by categories to include lexical as well as the Kantian categories, but he seems to have interpreted Kant's philosophy of the categories as a psychological story about the generation or processing of experience, rather than a philosophical account of the logically necessary conditions of experience. The cultural constructionists and linguistic determinists seem to understand the categories in the same psychological way that Kuhn does. Also, in maintaining the cultural variability of the categories, none of the anthropologists or ethnologists we have mentioned so far, including Mauss, appear to distinguish lexical from more fundamental categories.

Dan Slobin [1971, 120–22] points out two ambiguities in the linguistic determinist thesis about categories embedded in language shaping thoughts. First, there is an ambiguity between a strong, deterministic and a weak, predisposing sort of claim. Second, there is an ambiguity regarding what is meant by a category. Slobin detects an equivocation between lexical categories and grammatical categories. Indeed, the ambiguity is even worse than that, as it extends to categories in a third sense, that is, to fundamental concepts such as space, time, and causality as well. Let us examine the arguments and evidence for linguistic determinism for each of these three senses of category separately.

3.1   Lexical Categories and Linguistic Determinism

Slobin finds that evidence regarding differences in lexical categories among languages supports only the weak, predisposing sort of linguistic determinism. He lists three sorts of lexical differences among languages. First, there may be the absence of a term. For instance, English lacks an equivalent term for the Ger-

man *Gemütlichkeit*. Second, a superordinate term may be lacking in a particular language. To take an example from English again, it lacks an inclusive term for fruits and nuts, while Chinese has one. Finally, different languages may divide up some domain differently. French, for instance, uses the same term *conscience* for both conscience and consciousness. Also, different languages may divide up the color space differently [Slobin, 1971, 123–26]. These differences do not necessarily imply any differences in what concepts may be expressed in some language. As Slobin argues, "any concept can somehow be encoded in any language", although it might be more difficult in some languages than in others [Slobin, 1971, 126].

Beginning with the work of Brent Berlin and Paul Kay [1969], there has been a regular industry investigating the relationship between color terms and cognition. Debates have focused largely on the issue of whether there are culturally universal color concepts. Kay [Kay *et al.*, 1997; Kay and Regier, 2003] continues to find evidence in support of color universals, while John Lucy [1997] and Debi Roberson [Roberson *et al.* 2000], question whether there are color universals, providing evidence that linguistic color categories have an effect on things such as memory tasks and judgments of similarity. However, this evidence supports only the weak interpretation of the linguistic determinist thesis. Furthermore, even Roberson and her colleagues recognize that there are limits on cultural variability imposed by our visual system. No language, for instance, would group yellow with blue, skipping over green [Roberson *et al.* 2000, 395].

There also appear to be limits to cultural variability even for biological taxa. As I mentioned earlier, folk taxonomies have at least three categories or levels of classification, the unique beginner, the life-form, and the generic-specieme. The greatest cultural variability is at the level of life-forms. Although among the vertebrates, they correspond roughly to the classes of scientific taxonomy, among plants and invertebrates, they correspond to nothing in scientific taxonomies and may vary from culture to culture [Atran, 1987; 1990; 1994; 1995; Berlin, 1992]. Even among the vertebrates, some cultures will classify bats with birds, others with quadrupeds, and still others as their own life-form [Atran, 1990, 57]. Also, as Ralph Bulmer [1973] has famously argued, the Karam do not consider the cassowary to be a bird. But as Scott Atran argues, the cassowary as a unique life form is the exception that proves the rule. There is no reason to think that the Karam would not classify them with emus or ostriches if they knew of them [Atran, 1990, 39–40].

The empirical question whether certain taxa are universally recognized should be kept distinct from the philosophical question whether these kinds are nominal or real. There are some universally recognized kinds that are not real kinds, such as the life-forms tree, bug, and worm. Also, there may be real kinds that are not recognized as such in any culture. For instance, Robin Andreasen [1998; 2000] has argued that some monophyletic groups of human beings may be regarded as real kinds, as they reflect actual evolutionary branching processes. However, these cladistic groupings of humans do not correspond to ordinary folk or common-sense racial or ethnic categories.

Unfortunately, the question whether certain kinds are universally recognized and the question whether these kinds are real are not always clearly separated. The cultural and historical variability of systems of classification is often used as a premise in arguments for various nominalist positions in philosophy and the social sciences. For instance, George Lakoff [1987, 186] has argued that that there are no such things as natural kinds by pointing out that even within our own culture, pheneticists and cladists may disagree whether the lungfish is closer to the amphibians or to other sorts of fish, or whether the mountain zebra is closer to the horse than to other sorts of zebras. However, even for this dispute to arise, both sides would have to agree that there is some kind that is the coelocanth or some kind that is the mountain zebra. It is one thing to say that we may change our minds about whether zebra constitutes a natural kind that includes both the mountain zebra and Grevey's zebra. It is quite another thing to say that there are no such things as natural kinds.

Lakoff [1987, 187–88] also argues that species are not natural kinds because they are not defined by a set of essential properties. That is, biological species are not classical categories, according to which their instances satisfy a set of necessary and sufficient conditions [Lakoff, 1987, 195]. As he points out, biologists and philosophers no longer see species as homogeneous groups of individuals but regard them either as polytypic groups of actually or potentially interbreeding populations, following Ernst Mayr, or as historical entities that change with time, following David Hull [Lakoff, 1987, 187–88].[10] However, to show that species do not fit the classical notion of a kind or category does not imply that species are not real. All it may show is that we need to re-define the notion of a natural kind. Hilary Putnam [1975] and Saul Kripke [1972a, b] also doubt that we can give necessary and sufficient conditions for membership in natural kinds, since we run into all sorts of anomalous cases such as albino tigers and black swans. They propose that we regard natural kind terms instead as rigid designators. We may recognize something as a tiger not because it fits a list of necessary and sufficient conditions for tigerhood, but because it resembles previously known tigers to some degree. The relationship between actual tigers and the properties by which we recognize things as tigers may be only probabilistic and not deterministic [Keil, 1989, 46].

Frank Keil [1989, 55] suggests that instead of thinking in terms of a strict opposition between natural and nominal kinds, we should think of kind terms as falling along a continuum according to the degree to which they are used attributively or referentially, with natural kind terms towards the referential end and nominal kind terms toward the attributive end, with artifactual kind terms in the middle. Keil's continuum provides an alternative perspective on the question of the ontological status of human social kinds. Polemics often present us with a stark choice between the view that races, genders, and other social categories are socially and culturally constructed, and the notion that they are defined by timeless essences. But this contrast assumes that for some kind to be real, it must fit some classical category.

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[10]Lakoff refers to Mayr [1963; 1984] and Hull [1970]. See also Hull [1978].

We may instead think of kinds of people as ranged along a continuum from purely natural to purely nominal kinds. Some kinds will be almost purely nominal, such as Ian Hacking's [1999, 25–28] "child viewer of television".[11] I say almost, because even for this apparently socially constructed kind, there are constraints imposed by nature. An adult cannot be a child viewer of television. Kinds defined within kinship systems, such as mother, may be somewhat closer to the natural end of the spectrum. This kind will include prototype cases in which a woman gives birth to a child with whom she shares genetic material and then nurtures it. But it will also include adoptive mothers, step mothers, foster mothers, egg donors, surrogate mothers, and others, for whom their status as mothers depends on cultural and social as well as biological facts. Kinds such as hunter-gatherers and pastoralists, which concern a people's economic relationship to nature, may be even closer to the natural end of the continuum. That hunter-gatherers may be facing extinction is no more an argument for the nominal character of this kind than is the fact that biological species have gone extinct a basis for an argument that species are not real. There may be cultures that mix hunting and gathering with agricultural activities, but such cultures simply fit the hunter-gatherer prototype to a lesser degree. Still other social kinds, such as races or ethnic groups, will be distributed along the continuum, with purely folk categories such as "Hispanic" or "Jewish" at the nominal end and monophyletic groups of human beings at the natural end.

To sum up, there is little evidence to support the linguistic determinist assertion that lexical categories embodied in language and culture shape thought and perception. There is less cultural variability in lexical kinds than linguistic determinists have claimed, and the cultural variability of lexical kinds cannot be used as a premise for philosophical nominalism. With regard to social kinds in particular, if it is correct that they can be arranged on a continuum from nominal to natural, this would appear to undermine the objection to the scientific status of the social sciences that is premised on the assumption that social kinds are nominal kinds and do not support inductions.

3.2   Grammatical Categories and Linguistic Determinism

To return to Slobin's criticism of linguistic determinism, he argues that with regard to grammatical categories, the differences among languages concern not so much what they are able to express as what they regularly do and are required to express. For instance, in French and German, but not in English, one must assign a gender to every noun and decide whether to use the informal or polite second person pronoun. In English, but not in Russian or Latin, one must decide

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[11]Hacking's arguments for nominalism with regard to social kinds work largely by selecting for his examples arbitrary and isolated groups, such as victims of child abuse [Hacking, 1988; 1991a; 1992; 1999, ch. 4], or even ephemeral groups such as people with so-called multiple personality disorder [Hacking, 1986; 1991b; 1995] or "mad travelers" [Hacking, 1998]. As I have argued elsewhere [Schmaus, 1992], Hacking does not appear to consider social groups that are part of more or less permanent social institutions, such as social kinds defined within kinship or caste systems.

whether to use a definite or indefinite article. Kwakiutl requires that one indicate whether the object of which one speaks is visible to the speaker at the time one is speaking. Another argument for linguistic determinism that one might raise concerns differences with respect to the part of speech to which a word belongs. For instance, "heat" in Indo-European languages can be a noun. One could argue that this is the reason Western scientists sought to explain heat in terms of a substance like "caloric". If they had spoken a language like Hopi in which "heat" is only a verb, they might not have done this. However, as Slobin points out, scientists were able to get over this and reject the caloric theory. He concludes that differences in grammatical categories among languages result in differences not so much with respect to what *can* be said in them, but with regard to what is relatively easy to say in them [Slobin, 1971, 127–30].

3.3   *Fundamental Categories and Linguistic Determinism*

Conclusions similar to those Slobin draws for the grammatical categories could also be reached with regard to the fundamental categories. That is, differences in the ways in which the fundamental categories such as space, time, or causality are represented in different languages affect only what is relatively easy to say in them.

Among those who have made the radical claim that a culture may lack one of the fundamental categories and not some mere lexical or grammatical category is Sapir's student Benjamin Lee Whorf, who notoriously argued that the Hopi totally lack the category of time. According to Whorf, if the Hopi language contained no words, grammatical forms, or other constructions or expressions that refer to time, it would be "gratuitous" to assume that their thinking contains the notion of time. The Hopi worldview is supposed to be completely different than our own [Whorf, 1956, 57–58]. Similar claims have been advanced by Gary Witherspoon [1971; 1977], who argued that the Navajo lack the category of a permanent object, and Dorothy Lee [1949], who argued that the Trobriand Islanders lack the category of causality.

Whorf's critics have argued that his evidence for fundamental conceptual differences between Hopi and English speakers turns on literal, unsympathetic translations from the Hopi [Brown, 1958, 230–33; Devitt and Sterelny, 1987, 177; Lenneberg, 1953, 464–65; Pinker, 1994, 60ff.]. There are also problems concerning what evidence drawn from the analysis of the Hopi language, or any other language, would show. Would it be evidence that the Hopi truly lacked the category of time? Or would it be evidence that they simply lack cultural representations for communicating about this category? Perhaps it shows only that their cultural representations of time are so different from ours that they are hard to recognize.

Dan Little [1991] raises three problems for the ways in which Whorf and Witherspoon have attempted to use empirical evidence to support their conclusions. The first problem concerns the interpretation of another culture's language and other forms of public representation. Here he draws on Donald Davidson's [1984] argument that it does not even make sense to say that people in different cul-

tures have categories that are radically incommensurable with ours. According to Davidson, the assertion that there are fundamentally different conceptual systems amounts to the statement that there are languages that are not inter-translatable, which he found to be inconsistent with the notion that languages can be used to make true claims about the world. Little argues that if Hopi concepts are truly incommensurable with ours, then how would an ethnographer be able to interpret their culture so as to be able to tell this? For interpretation to be possible, he says, there must be a "core set" of shared beliefs and concepts [Little, 1991, 208]. I agree. But what is not clear to me is just how much overlap there must be between cultures for interpretation to be possible. For instance, even if it were true that the Hopi lacked a concept of time, we might still be able to find common ground with them on certain propositions that do not depend on the concept of time, such as the claim that an eagle is larger than a mouse. The question is whether the common ground would be sufficient for interpreting their culture well enough to determine that they lack a concept of time.

Little's second problem with Whorf and Witherspoon's appeal to empirical evidence turns on Quine's thesis of the indeterminacy of translation. Little argues that if Quine were right about the indeterminacy of translation, then there would be no basis for preferring an interpretation of Hopi culture according to which it lacked the category of time over another interpretation that denies this [Little, 1991, 209]. However, Quine's indeterminacy of translation thesis is problematic. As Laudan [1990, 128] argues, the Quinean argument for the indeterminacy of translation can be seen as a special case of the argument for the underdetermination of theory by evidence. Two translations are like two hypotheses supposedly explaining the same data, the data in this case being the utterances made by native speakers of the language. One can always reply to such arguments that it may still be rational to choose one hypothesis, theory, or interpretation over another even when a single answer is not "determined". After all, it is scarcely credible that we will ever be faced with a choice between two hypotheses that are exactly empirically equivalent. Arguments for the possibility of empirical equivalence are typically supported by philosophical thought experiments, such as Nelson Goodman's grue paradox, rather than by actual cases from the history of science. Also, non-empirical factors such as parsimony play a role in theory choice. But even if one were to insist on the indeterminacy of translation thesis, we need to distinguish translation from understanding here. Although we may not be able decide on the best translation of the language of another culture, it does not follow that we do not sufficiently understand their culture in order to determine whether or not they have some way of talking about time.

Little saves his most thought-provoking argument against Whorf and Witherspoon for last. Here he says that we must be careful to distinguish concepts for ordinary, everyday things from higher-level, metaphysical interpretations of them. For instance, from the fact that the Navajo language appears to lack a term for permanent object, we cannot infer that they lack the ordinary notion of objects such as trees, houses, people, and animals [Little, 1991, 209]. The Navajo simply

have a different way of classifying such ordinary objects and different beliefs about them. Although they may not have a term for objects in general, they make a distinction between objects at rest and active objects. Objects at rest are further sub-divided into fifteen different classes [Witherspoon, 1971]. This suggests simply that the Navajo have neither use nor need for a public representation of the metaphysical notion of a highest genus of objects that are permanent in time. They are able to say everything they want to say with concepts of active objects and objects at rest.

Consider also Dorothy Lee's claim that the language of the Trobriand Islanders lacks terms for expressing causal relationships such as "cause, reason, effect, purpose, to this end, so that, why". She adds, "This does not mean that the Trobrianders are incapable of explaining a sequence in terms of cause and effect, but rather that this relationship is of no significance" [Lee, 1949, 407]. If Lee is correct about the Trobriand language, this shows only that the islanders have little use for general terms for causality. On the other hand, their language does not seem to lack for transitive verbs such as "beat", "awaken", or "throw", which express causal relations. We should not infer that the Trobrianders lack the category of causality from their lack of a general term for it; similarly, we should not infer that the Navajo lack the category of an object from their lack of a general term. In the same way, we cannot infer from the fact that English, unlike Chinese, lacks a general term for fruits and nuts, that English speakers cannot form this concept. What we can conclude is that it is more difficult to express these general concepts in some languages than in others.

In sum, there is no unequivocal evidence that there is cultural variability in the most fundamental categories of thought. It is possible that the perceptual experience of individuals from different cultures is structured by the same set of fundamental categories. But for one reason or another, these categories may be represented in different ways in different languages and cultures.

4 THE SOCIAL FUNCTIONS OF THE CATEGORIES

To say that different cultures may have different representations of the same category is only to raise the question as to what it is that makes them representations of the same category. For instance, what is it about the Zuñi system of seven directions that makes it a way of dividing *space*? What makes it an alternative to the four cardinal points of the compass? Similarly, to identify the category of causality with its cultural representations is to leave unexplained what makes all of these representations of causality. What do the Sioux notion of *wakan*, the Iroquois notion of *orenda*, and the Melanesian notion of *mana* have in common that would have led Durkheim to interpret them all as cultural representations of causal power? If we simply identify the categories with their cultural representations, it is difficult to answer these questions. I suggest that what makes representations from different cultures representations of the same category is that they play similar social roles or perform similar social functions. The meaning of a cultural

representation derives from the social function it serves. Cultural representations from different cultures that serve similar functions in their respective cultures are similar in meaning and represent the same categories.

This way of thinking about the categories is implicit in Durkheim's argument in *The Elementary Forms* that there are categories that are necessary for social life, including space, time, causality, and genus. He said that society is possible only if the individuals and things it includes are classified into groups that are then classified in relation to each other. To avoid conflict among these groups, space must be divided according to a system of directions recognized by everyone. In addition, it would be impossible to call people together for such cooperative endeavors as feasts, hunts, and military expeditions unless a society had some system for fixing dates and times so that everyone understands time in the same way. For people to cooperate with the same end in view, they must also be able to reach a consensus on a causal relationship between their collective end and the means to achieve it [1912a, 632–33; t. 1995, 444–45]. A culture also needs some way of representing the idea of necessary connection implicit in the concept of causality in order to make the notion of obligation and thus moral rules possible [1912a, 524–27; t. 1995, 370–73].

As I mentioned above, we may regard this social functional sense as a seventh, Durkheimian sense of the concept of a category, in addition to the lexical, taxonomic, grammatical, Aristotelian, Kantian, and psychological senses of this concept. To refer to categories in this seventh sense as Durkheimian is not to suggest that Durkheim had the definitive list of which categories play important social roles. For instance, although he includes totality among these categories, it is hard to see that this concept of a highest genus has a social function distinct from the idea of genus or classification in general [1912, 629–30; t. 1995, 442–43]. Nor is it clear that these concepts are *necessary* for social life, as Durkheim claimed, since after all there are social animals that do not appear to have all of them. To argue that these concepts may not be necessary for social life generally but only for human social life such as we know it is to risk tautology. Nevertheless, ways of communicating about time, place, and causality certainly facilitate the social life of human beings. In addition, the difference between human beings and the higher animals can only be one of degree. Animals are not wholly without some ways of conceptualizing, for example, quantitative, causal, and spatial relationships. Even relatively simple social animals such as honey bees can communicate about the distance and direction of nectar-bearing flowers.

Durkheim also thought that social life depended on the individual members of a society sharing "homogeneous conceptions" or the same collective mental representations of the categories [1912, 23–24; t. 1995, 16]. However, individual mental representations, which are private, may differ as long as these individuals can communicate about collective activities using public cultural or linguistic representations. All that is needed to coordinate collective actions is for everyone to understand and agree upon the meanings of these public representations. What is inside each person's head is irrelevant, since others have no way of knowing

what that person means except through the public representations he or she uses. To say that a person understands the meanings of these public representations through their social functions is not to imply that individuals must have a full awareness of these functions. For instance, following Durkheim, we could say that the social function of the concept of a necessary connection is to make moral rules and obligations possible and thus help to maintain social unity. But this does not mean that all these thoughts must be running through a person's mind at the very moment he or she reminds some other individuals of their obligations, although on subsequent reflection he or she may concede that society would fall apart if people did not fulfill their duties.

Claude Lévi-Strauss similarly thought that public cultural representations take their meanings from their social functions. He describes primitive thought in terms of Saussure's notion of a sign, which he characterizes as intermediary between images and concepts, having a greater power of reference than a mere image. That is, the meaning of a representation or image used as a sign does not relate merely to the thing from which it is drawn, but has to do with the way it is used. The primitive takes images from nature to use as signs to express concepts that are necessary for social life, much as the bricoleur utilizes whatever he or she finds ready-to-hand in order to get a certain job done (Lévi-Strauss 1966, 18-20). For example, the primitive employs systems of natural classification in order to express categorical relationships. As Lévi-Strauss explained, it is not that myths are invented to explain natural facts, but that such facts are the medium through which the primitive attempts to explain facts not of a natural but a logical order [1966, 95].

If, as Durkheim argued, the categories of genus, space, time, and causality were truly necessary for social life, then we would expect all cultures to have some system or other for representing them that permits their use in communication among the individual members of a social group. Of course, not all cultures need to represent these categories in the same ways in order for the categories to carry out their respective functions in each society. In addition, different cultural representations of the same categories need not have the same sorts of causes or origins in order to perform the same sorts of functions. The meanings of these cultural representations depend on the social functions that they serve, not on their causes. To link the meanings of cultural representations to their social functions rather than their causes can help us get clear about how communication is possible. If the meanings of the cultural representations depended on their social causes or origins, then people who had not been exposed to these same social causes would not be able to understand them. Indeed, this would be a problem not only for the visiting ethnographer but also for the individual members of one and the same society, whose experiences may vary and who may not have been present when the use of some cultural representation began, if we can even make sense of the notion of such a beginning. This obstacle to explaining how people can communicate would be removed if the meanings of the cultural representations of the categories had to do instead with their social functions.

Defining cultural representations in terms of their social functions rather than their causes also helps to explain how intercultural communication is possible, just to the extent that different representations may serve similar functions in different cultures. A person may recognize that a representation is being used in another culture in a way that is similar to that in which some other representation is used in his or her own culture and thus assume that these two are representations of the same category. For example, Lakoff [1987, 313–14] cites the way in which Mixtec languages talk about spatial location by projecting the image of the body on things, instead of through the use of prepositions and case. The top of a mountain is referred to as the "head" of the mountain, and something under the table is said to be in the "belly" of the table. Lakoff argues that what allows us to interpret the Mixtec's metaphorical use of the body as a way of indicating spatial location is that "we too have the capacity for metaphorical projection of this sort, even though our conceptual system is not conventionally organized in this way" [1987, 314]. But surely the interpretation of these Mixtec metaphors also relies on our ability to recognize their purpose in using them. To say that some object is either under the table or in the table's belly each serve the similar function of helping another person locate this object.

For the purposes of social science, then, a category such as space, time, or causality may be defined in terms of the social function it serves, and would include all the various cultural representations that serve this same function. This is not to say that all these cultural representations have exactly the same meaning, or that they all satisfy a set of necessary and sufficient conditions for the membership in this category. For instance, we may understand that the traditional Chinese periods of yin and yang, like the four seasons of the Europeans and Americans, are cultural representations of the category of time by the fact that they perform similar social functions, such as organizing agricultural work. This is not to suggest that the function of organizing agricultural work exhausts the meaning of yin and yang. Chinese representations of time clearly have different connotations from our four seasons. We may also understand the seven directions of the Zuñi as well as the points of the compass of Western societies as ways of indicating directions in space, while nevertheless recognizing that these directional concepts have additional meanings in their respective societies as well.

The fundamental categories appear to give rise to prototype effects, such as differences in reaction times and typicality ratings for instances that fall under the concept, that are not unlike those that Rosch has discovered for lexical categories such as colors or birds [Lakoff, 1987, 39–46]. According to Rosch, for example, a songbird is regarded as more typical than a duck, penguin, or eagle. Similarly, Lakoff finds prototype effects for the category of causality. He characterizes prototypical causation, which he takes to be direct manipulation, in terms of the following ten properties:

1. There is an agent that does something.

2. There is a patient that undergoes a change to a new state.

3. Properties 1 and 2 constitute a single event; they overlap in time and space; the agent comes in contact with the patient.

4. Part of what the agent does (either the motion or the exercise of will) precedes the change in the patient.

5. The agent is the energy source; the patient is the energy goal; there is a transfer of energy from agent to patient.

6. There is a single definite agent and a single definite patient.

7. The agent is human.

8. (a) The agent wills his action

   (b) The agent is in control of his action.

   (c) The agent bears primary responsibility for both his action and the change.

9. The agent uses his hands, body, or some instrument.

10. The agent is looking at the patient, the change in the patient is perceptible, and the agent perceives the change. [Lakoff, 1987, 54–55]

Lakoff finds that statements such as "Brutus killed Caesar" match this prototype exactly [Lakoff, 1987, 55]. According to Lakoff, other cases of causality differ from this prototype in one or more of these characteristics. For instance, mechanical billiard-ball causality includes only characteristics 1 through 6; indirect causation lacks characteristic 3 (*ibid.*). Although a culture may not have cultural representations of the general notion of "causality", we may find a prototype structure among the verbs and morphemes used to express causal relationships. Lakoff, for instance, mentions that Mixtec has three causative morphemes, a word and two prefixes, that are used to distinguish among degrees of direct or indirect causation [1987, 55].

Of course, as Lakoff [1987, 151–52] and Keil [1989, 29–32] remind us, from the fact that a concept gives rise to prototype effects we cannot infer that the concept itself has a prototype structure, since, as Armstrong, Gleitman, and Gleitman [1983] have demonstrated, even the concept of an odd number, for which there are certainly necessary and sufficient conditions, gives rise to prototype effects. Nevertheless, thinking of the category of causality in terms of prototype effects instead of in terms of a set of necessary and sufficient conditions, in addition to thinking of this category in terms of its social function, can help us get a handle on the interpretation of various cultural representations of causality. Durkheim's examples of cultural representations of causal powers in nature, that is, wakan, mana, and orenda, fit those characteristics of Lakoff's causal prototype that do not depend on the humanity of the agent, which helps to explain how it is that we recognize them as cultural representations of causal powers. To consider another

example, Lucien Lévy-Bruhl writes about a kind of magical causation in which the agent attempts to bring about some harm to the patient through some action on his image, hair, nails, bodily fluids, footprints, utensils, or other things that are said to "participate" in the patient. This case also shares many of the characteristics of Lakoff's causal prototype, although it need not include 3 and 10, and 5 and 8c must be reformulated in terms of the beliefs of the agent and other members of his or her culture. The energy transferred is a form of magical energy in which only members of this culture believe, and only members of this culture would hold the agent responsible for harm to the patient through witchcraft.

   Pascal Boyer has argued that such "strange" causal conceptions as those discussed by Lévy-Bruhl are not really part of the culture but attributed by anthropologists to people in a culture in order to make sense of their causal claims [1992, 189; 1994, 126]. Boyer says, "People do not plow their fields... in terms of 'participation' and 'resemblance'" [1994, 129].[12] According to Boyer, what differ from culture to culture are not concepts of causality so much as ontologies of things with causal properties. However, it seems that conceptions of causality must be implicit in such ontologies. Boyer thus brings us back to the issue of cultural interpretation raised by Witherspoon's claims about the Navajo lacking the concept of object and Lee's claims about the Trobrianders lacking that of causality. Just as the absence of a cultural representation of the general category of causation or permanent object does not imply the absence of such concepts, the absence of a cultural representation for some more specific causal concept, such as magical causation, does not imply the absence of this concept, either. The culture may have no need for a cultural representation of magical causation in general, as it is able to express all that it needs about it through more particular cultural representations of magical causal powers of the items in its ontology.

   I propose that there may be different, overlapping prototypes for intentional and mechanical causality, as these differ in their social functions. Intentional causality is different in meaning than mechanical causality because it involves the social function of assigning responsibility for an action to an agent. Concepts such as "killing" that fit the causal prototype for intentional causality provide a means of holding someone accountable for someone else's death, which facilitates the maintenance of social order. However, intentional causality also includes bringing about some change through words as well as deeds, and even for bringing about some state of affairs through a failure to do or say something, such as a failure to warn someone of some impending danger that the agent perceives but the patient does not. In this latter case, Lakoff's characteristics 5 and 9 do not apply, raising the question whether they are in fact a part of the prototype for intentional causality.

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[12]This remark is somewhat unfair to Lévy-Bruhl, who said that in practical matters such as procuring food the so-called "primitive" employs a different causal concept, the notion of an invariable antecedent. It is only in giving explanations of natural phenomena that the primitive invokes some higher, metaphysical notion of causality [Lévy-Bruhl, 1922, 511–17; t. 1978, 438–43]. Indeed, as a Comtean, Lévy-Bruhl's whole point appears to have been that magical notions of causality gradually give way to a more Humean notion of causality that has its source in the practical or economic sphere of life [Schmaus, 1996].

What is important for this prototype, however, is the attribution of beliefs, desires, and possibly other intentional states to the agent. This characteristic does not belong to the prototype for mechanical causality. Mechanical causality, on the other hand, is involved in practical activities such as tool-making, as well as in social activities such as teaching the younger generation how to make tools. This concept includes the characteristic of repeatability of the effect under the same circumstances; intentional causality does not. Ordinary causal judgments may be recognized as such just to the extent that they fit one or another prototype for causality, with verbs such as pushing, hitting, and making regarded as representing causal relationships. In English, at least, many of these verbs can be used to express both intentional and mechanical causality (Bob hit Bill; the rain hit the roof). However, it should be clear from the context which sense of causality is meant.

Social scientists should also be able to discover prototype effects for other categories in addition to causality. The characteristics of each categorical prototype should refer to the social functions that each category fulfills. For instance, drawing on the examples above, the prototype for the category of time would include the functions of organizing agricultural and other work and calling people together for festivals and other collective activities. The prototype for space would include the functions of indicating locations and directions and dividing territory among competing interests. The prototype for the category of a genus may include recognizing different social groups that have competing interests, but could also include communicating about the natural kinds important to hunting and agriculture. If cultural representations have at least some of the characteristics of these prototypes, we will be able to recognize or interpret them as representations of categorical concepts. To say that we can interpret the cultural representations of another culture through the functions they serve, however, is to suggest that there is something that we all share in common that makes it possible for us to recognize these functions in other cultures. This suggestion raises the question of the interplay of cultural and individual cognitive factors in the formation and interpretation of categorical concepts.

5   THE SOCIAL AND THE COGNITIVE SCIENCES

To say that there are functionally-defined categories that are culturally universal is not necessarily to imply that these categories are somehow grounded in innate, unconscious psychological mechanisms. Cultural universals could also be due to convergent cultural development. The psychologists Michael Cole and Sylvia Scribner [1974] have questioned whether anything about mental categories or concepts can be inferred from language categories. More recently, John Lucy questions whether we can make this inference in either direction [1997, 339–40]. We cannot assume that there is some innate neural mechanism behind every lexical item in our language. Nor should we assume that every neurophysiologically-based psychological category must be represented in language. Languages will include only what societies find useful to communicate about.

To sort out the relative contributions of culture and biology to our conceptual repertoire will require the cooperation of sociology and anthropology with cognitive psychology and the neurosciences. Anthropologists and sociologists may compare different systems of cultural representations to see what they may have in common, in order to gain some insight into what concepts facilitate social life, and then investigate the extent to which these common features may be explained by convergent cultural development. The cognitive sciences may investigate the contributions that our innate cognitive mechanisms make to concepts such as space, time, causality, permanent object, and classification. This is not to suggest a strict division of intellectual labor among these disciplines or that the cognitive scientists must wait for the social scientists to complete their task. Rather, there should be constant cooperation and sharing of results.

Social scientists may also investigate how cultural representations of the categories allow us to improve upon the cognitive resources that natural selection has given us. There are at least three reasons for thinking that our inborn cognitive resources may be less than optimal. Natural selection can adapt the mind only to relatively stable features in the environment, whereas the development of cultural representations may allow us to think and communicate about rapidly changing circumstances. In addition, natural selection may have adapted cognitive mechanisms that originally evolved for other tasks to new functions to which they may not be as well suited. And third, ever since Darwin, we have known that species may evolve new behaviors while the evolution of biological structures that could facilitate these behaviors may lag behind. Consider, for instance, how the human spine is not fully adapted to our upright posture. Human behavior could also have gotten ahead of our inborn cognitive mechanisms. Thus it is at least possible that human beings through the development of culture are finding ways of representing space, time, or causality that are more coherent or less ambiguous than the ways in which our naturally selected cognitive mechanisms represent them.

Consider again, for example, the category of causality. Cognitive scientists postulate two different cognitive mechanisms for perceiving causal relations: one for physical or mechanical and one for intentional causation (e.g., [Sperber, Premack, and Premack (eds.), 1995]). This hypothesis can explain how mechanical causality is subject to perceptual illusions such that even when we know it is an illusion, we cannot help but to see it. These illusions can be produced, for example, by projecting images that appear to be interacting causally. One spot of light may appear to strike another spot of light and cause it to move, although upon reflection we know that the causal relation is only an illusion. Also, studies in developmental psychology indicate that human infants are able to perceive this sort of mechanical causality much earlier than they are able to understand intentional causality. In children with Asperger's Syndrome or childhood autism, the ability to attribute intentional states to others, and thus to fully understand intentional causality, is much delayed, while the ability to perceive mechanical causality is not affected.[13]

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[13]See, for example, Leslie [1991].

Let us imagine for the sake of argument that neuroscience research finds evidence for the cognitive scientists' hypothesis of separate inborn cognitive mechanisms for perceiving mechanical and intentional causation. It will nevertheless remain true that cultural representations of causality may either combine these two notions of causality or introduce finer distinctions in them. As Steven Pinker [1997, 315] points out, cultural representations of causality in animistic explanations of natural phenomena and in anthropomorphic tales blend intentional and mechanical notions of causality. On the other hand, it is only through the development of a system of cultural and linguistic representations that philosophers such as Hume have been able to analyze the concept of mechanical causation and distinguish the notion of an invariable sequence from that of force or power. Cultural representations allow us to refine our concepts of intentional causality, as well. For example, much of the development of the English and American common law tradition can be regarded as clarifying the conditions under which people may be held responsible for certain harms to others.

As Atran suggests, cultural representations can amplify human conceptual abilities [1995, 218]. Cultural representations also make possible an intellectual division of labor, in which not everyone needs to carry the full load of a culture's concepts. To continue with our example, only lawyers may need to know all the ways in which the law of torts holds people responsible for the consequences of their actions, words, and inactions. Similarly, there may be ways of measuring space and time that concern only physicists and astronomers, or concepts of substance of which only theologians or philosophers are aware. Ordinary people may go their entire lives without using many of these concepts.

6 CONCLUSION

I have argued that there is a seventh kind of category in addition to lexical, taxonomic, grammatical, Aristotelian, Kantian, and psychological categories, which I have called Durkheimian categories. These are the fundamental concepts that facilitate the normal functioning of human society. They resemble Aristotelian categories in that they are highest predicables. They may overlap somewhat with grammatical and Kantian categories. For instance, substantives name kinds of objects and many transitive verbs express causal relationships. However, not all grammatical categories have social functions. Societies may be able to get along just fine without the grammatical category of gender. What categories help to maintain society is a question for empirical investigation by anthropology and the sociology of knowledge.

In order for the categories to fulfill their social roles, everyone in a culture will need appropriate linguistic or other cultural representations to express at least some of the concepts that fall under these categories. A culture need not have some way of representing each category in the most general terms. For instance, a culture may not have a representation of causality in general, but may have representations of various causal concepts that come under the category of

causality. Other concepts falling under these categories may be the province only of experts or specialists. But even for the specialists who use these concepts, there must be systems of representations that allow them to communicate with one another about them.

It is important for social scientists to keep the different senses of category distinct from one another and from their cultural representations, and to be careful not to generalize conclusions from one to another. If it is true that Kantian categories structure experience and judgment, it does not follow that lexical categories do. Similarly, from the fact that some of Aristotle's or Kant's categories serve important social functions, it does not follow that they all do. In addition, one cannot infer the cultural variability of the categories from the cultural variability of their cultural representations. Finally, from the lack of a cultural representation for the general idea of a category, one cannot infer that the culture lacks concepts that belong to that category. As I have tried to show, maintaining these distinctions allows us to explain how cultural interpretation is possible, to avoid the unwanted implications of linguistic determinism, and to show how the social and cognitive sciences may cooperate in investigating the conceptual requirements of social life and how they are met.

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HERMENEUTIC AND PHENOMENOLOGICAL APPROACHES

William Outhwaite

1 INTRODUCTION

The philosophy of anthropology and sociology do not of course exist as formally constituted separate sub-fields of the philosophy of social science, which itself is a somewhat diffuse branch of the philosophy of science. But this is to look at things from the wrong end. More to the point however, and particularly relevant to the concerns of this chapter, is the fact that in the social sciences, and especially in sociology and anthropology at the relatively "soft" end of social science (the other end being occupied by most variants of economics and empirical social psychology), the interplay of philosophical and substantive sociological/anthropological concerns is particularly close. This chapter is concerned, then, with the impact of hermeneutics and phenomenological philosophy on the social sciences and, more broadly, with the "very idea" of a social science such as anthropology or sociology, and with the philosophical implications of the practice of these two intellectually, if not institutionally, inseparable disciplines.

"Hermeneutics" in the sense of the science, art or technique of interpretation of written texts long precedes the crystallization of anthropology and sociology in their modern form, while phenomenology as a philosophical approach coincides with their consolidation in the early twentieth century. Both terms have been used in an extended sense to denote a variety of approaches in the social sciences. The word hermeneutics refers, of course, to the Greek messenger-god Hermes, and reflection on the problems of interpretation and criteria for the truth, validity or adequacy of interpretations goes back to ancient Greek thought in the European tradition and a similar historical distance in the other world civilizations. This pre-history of hermeneutics was revived in the philological criticism of classical texts in the Renaissance, the interpretation of Roman Law, the interpretation of the Bible in Christianity (especially Protestantism) and the philosophical analysis of texts.

The systematisation of hermeneutics occurred largely in German-speaking Europe in the course of the nineteenth century, though this was substantially anticipated by Giambattista Vico (1688-1744), who formulated the basic principle that our knowledge of what we ourselves have made (individually or collectively)

is different from what we have not made. The world of human society and culture is in some sense 'our' product, whereas the natural world is God's product.

This is perhaps where one can find the core idea of hermeneutic approaches to the social world. Hermeneutic, phenomenological, or more broadly interpretive social science theory is motivated by an interest in knowledge which is rather different from the more general scientific interest in understanding and explaining processes in the social world. One way of putting this is to say that it is interested in insider knowledge rather than, or as well as, outsider knowledge (Merton, 1972) — in knowledge of *what it is like* to be a social actor of a particular kind, and in how such people understand their social situation. Another way of expressing the same idea is to say that interpretivists are more interested in understanding (from the inside) than in explaining (from the outside).

This distinction, crucial to later hermeneutics and 'hermeneutic' social science, between what we know from the inside and what we know because we have learned *about* it is taken up in a rather more speculative form in Hegel's differentiation between reason (*Vernunft*) and the understanding (*Verstand*), though the maker here is the world-spirit coming to recognise its own productions (including, ultimately, the world itself) and its learning-processes as rational, in contrast to the essentially contingent states and relations found in nature and described by the mathematical and natural sciences. Something more like Vico's idea returns again in the second half of the nineteenth century, when Hegel's concept of objective mind is extracted from its surrounding developmental 'grand narrative' and treated more as a descriptive category.

It was Friedrich Schleiermacher (1768-1834) who consolidated hermeneutics in a systematic form, establishing the term understanding (*Verstehen*), which has survived as standard usage in English-language social science discourse, and making it central to interpretation - understood as a more systematic activity. Interpretation here involves both the linguistic understanding of meaning and a psychological understanding of the author's intention. Schleiermacher introduced the distinction between technical and more developed or speculative forms of interpretation and formulated the much-discussed principle that it should be possible to understand an author better than 'he' understood himself. Schleiermacher's contribution was made the central and culminating point of the account of 'the rise of hermeneutics' given by Wilhelm Dilthey (1833-1911), with whom hermeneutics becomes central to the self-definition of what he called the human sciences or *Geisteswissenschaften*. In these sciences, as Dilthey put it, the mental activity of humans and of some other animals, and its products, can be *understood*.

Dilthey and his contemporary, the philosopher of history J. G. Droysen (1808-1884) developed what we would now call a research programme for history and the other human sciences based on the distinctiveness of human psychic expressions and the understanding of those expressions. In a move which was to become a definitional feature of later interpretive social science, Dilthey, like Schleiermacher, emphasized the continuity between everyday understanding and more formal processes of interpretation. His distinction between the natural and human sci-

ences was developed in large part in opposition to Comtean positivism, which had become influential, even in the German-speaking countries, by the middle of the nineteenth century. In a parallel but more methodological formulation, two other neo-Kantian thinkers, Wilhelm Windelband and Heinrich Rickert, argued that the study of culture is essentially concerned with individual processes and relating them to shared human values, whereas the natural sciences are concerned with general laws about objects which are essentially remote from questions of value. We are interested, for example, in the French Revolution not just as a member of a class of revolutions exhibiting certain common features (this would be, for Rickert, a natural-scientific mode of approaching it), but as a unique event embodying, and perhaps also violating, certain crucial human values.

This opposition between positivism and methodological dualism, and more particularly between causal explanation, analysed in terms of universal regularities, and 'understanding' comes to structure the emergent human or social sciences, as the term 'culture' increasingly gives way to "society" or "sociation" (*Vergesellschaftung*). There is a fairly strong line of influence from Rickert to Max Weber, both directly and also through his friend Georg Simmel, who discusses our knowledge of the social world in terms which foreshadow social constructionist theory.

2 THE TWENTIETH CENTURY

Max Weber's personal development from economic and legal historian to sociologist continues the encounter between hermeneutics and the social sciences which had been a dominant feature of the late nineteenth century, and he also develops a middle position on the issue of methodological dualism, as he had earlier on the related opposition in economic theory (the *Methodenstreit*) between the pursuit of a systematic laws and the more interdisciplinary approach of the "historical school." For Weber, as he put it in an early essay, "the course of human action and human expressions of every sort are open to an interpretation in terms of meaning which in the case of other objects would have an analogy only at the level of metaphysics" [Weber, 1975: 217-8]. He therefore later defines sociology, in the first sentence of his major work *Economy and Society,* as a science which aims at an interpretative understanding of action in order thereby to understand its course and its effects. Whether by this Weber means that explanatory understanding is itself a form of causal explanation, or merely complementary to it, the crucial point for him is that explanations of social phenomena must be both "causally adequate" and "meaningfully adequate."

For Weber, then, our access to knowledge of the social world is importantly different from our knowledge of nature. It is not, however, in his view any less objective. He heroically attempts to hold together Rickert's principle that our perspectives on cultural phenomena, and our knowledge of them, are shaped by values (based for Weber on ultimately ungroundable existential choices), with the idea that the social sciences can attain a bedrock of solid and "value-free" knowl-

edge which would have to be accepted, as he sometimes curiously puts it, "even by a Chinese." Weber insists that it is the intentions and purposes (subjective meanings) of human actors which define their actions and which therefore have to be understood by the historian or sociologist, but he moves rapidly to the construction of a system of ideal types of action-orientation less limited than that found in economic theory but still substantially dependent on it.

Weber's synthesis was pulled apart from both sides in the decades following his early death in 1920. At one pole, there was now a more stridently naturalistic and indeed reductionist variant of positivism: the logical empiricism of the Vienna Circle, in whose "unified science" the statements of all sciences should be ultimately reducible to material-object language or to statements in physics; *verstehen* was of no more importance, in Otto Neurath's vigorous formulation, than a good cup of coffee which sustains the social scientist.

From the other direction, the Austrian Alfred Schutz (1899–1959) brought phenomenology into the philosophy of social science and the practice of sociology. Phenomenology must not be confused with phenomenalism, the philosophical doctrine that only phenomena are real, with nothing underlying them. Phenomenology, as another Austrian, Edmund Husserl (1859–1938), developed it in the early years of the twentieth century, means an approach to knowledge which focuses on our experience of things, bracketing out the issue of whether or not they really exist or are optical or other illusions, and what they are made of. Thus a phenomenological approach to time, for example, will not be concerned with its intrinsic nature so much as with our experience or awareness of it.

It is this latter dimension of our social experience that Schutz felt had been overlooked in conventional sociology, even when, as in Weber's case, it purported to be concerned with understanding the intended meaning of human actions. In a book published in Vienna in 1932 with the title *Der sinnhafte Aufbau der sozialen Welt* (*The Meaningful Constitution of the Social World*), Schutz argued that the problem with Weber's ideal types was not that they were insufficiently scientific, but precisely the opposite: Weber was too quick to impose them on the phenomena he described, paying insufficient attention to their grounding in acts of typification performed by ordinary members of society. For Schutz, the social scientist is merely constructing second-order typifications based on those already carried out in the lifeworld.

> The observational field of social scientist — social reality - has a specific meaning and relevance structure for the human beings living, acting, and thinking within it. By a series of common-sense constructs they have pre-selected and pre-interpreted this world which they experience as the reality of their daily lives... The thought objects constructed by the social scientists, in order to grasp this social reality, have to be founded upon the thought objects constructed by the common-sense thinking of men (sic) living their daily life within their social world. Thus, the constructs used by the social scientist are, so to speak, constructs of the second degree, namely constructs of the constructs

made by the actors on the social scene, whose behaviour the scientist
observes and tries to explain in accordance with the procedural rules
of his science. [Schutz, 1962: 59]

The lifeworld in this sense meant the world of common-sense perception, before
it was subjected to phenomenological analysis. In Schutz' more informal use of
phenomenological terminology, it refers to the social world which we interpret and
make meaningful through our "typifications." A person comes to the door in a
police uniform; we assume he or she is a police officer and behave accordingly.
(We may of course be wrong in our assumption; the person may be a robber
impersonating a police officer, or someone going to a fancy dress party.)

The point, for Schutz, is that we make sense of the world through what he calls
a stock of knowledge at hand which we do not normally problematize. One of
Schutz's most famous essays, "The Stranger," is about a person finding their way
around in unfamiliar surroundings and negotiating social situations in which they
are not "at home." We inhabit multiple social realities, based on the nature of
our knowledge of people, places and so on; we can construct concentric circles of
people we know intimately, people we recognise or whose names we know, people
we have seen only on TV, and so on.

Schutz was not a full-time academic, and he wrote mostly essays. He was how-
ever very influential, and can now be seen as an important figure linking Simmel
and Weber to more recent and radical developments in interpretive social theory.
The word radical is meant here in an intellectual rather than political sense, re-
ferring to the notion of social construction which may or may not be linked to
a demystifying or debunking orientation; to say that the emperor's new clothes
are socially constructed may also be to say that he is naked. Some of the appeal
of interpretive social theory in the mid-1960s and subsequently derived from the
context of the radical and innovative forms of political protest in the student and
'alternative' movements. A focus on small-scale or micro interactions in everyday
situations may have wider implications for social structural analysis. Schutz him-
self was by no means however radical in either a political or an intellectual sense.
In a typically phenomenological gesture, he suggested that his work was *comple-
mentary* to more systematic types of theory such as functionalism or neoclassical
economic theory and that it opened up an area linking everyday or commonsense
social understanding with more systematic analysis. Systematic theorists like Par-
sons could merely be criticised for neglecting or denying the need for social theory
to be grounded in attention to the 'subjective point of view' (see [Schutz and
Parsons, 1978]).

This theme was taken up by the phenomenologist Aron Gurwitsch in the United
States, where Schutz had also settled, and by Peter Berger and Thomas Luckmann
[1961], who finally put the term "social construction" on the map, offering in the
guise of a sociology of knowledge a paradigm more directly adapted to use in social
research. By the time Schutz's first book was republished, in Germany in 1970 and
in the United States in 1967, the way had been prepared by his own later work and
by Berger and Luckmann. For Berger and Luckmann, what they called "society as

an objective reality" is the product of processes of definition and conceptualization. They were explicitly relativistic in their approach, arguing that the sociology of knowledge should be concerned with 'whatever passes for "knowledge" in a society' [Berger and Luckmann, 1966: 15]. Society, though socially constructed, is both an objective and a subjective reality.

One can of course ask how far the approach taken by Schutz, Berger and Luckmann and others in this tradition is really a phenomenological approach, and how far it is just using phenomenology as a metaphor. The English title of Schutz's major work begs this question, but Schutz himself was careful to stress that the only chapter of the book which was phenomenological in the strict sense was that on time. Luckmann, too, in the introduction to his influential edited collection [1978: 7], was careful to separate his strong claims for the importance of phenomenology "as a philosophical foundation of modern social science" from what he calls "recent examples of sociological analysis which builds on that foundation."

> The phenomenological method is more radically descriptive than any method of an empirical science could conceivably be — or could want to become...
>
> The goal of phenomenology is to *describe* the *universal* structures of subjective orientation in the world, not to explain the *general* features of the *objective* world. [Luckmann, 1978: 8–9]

The middle decades of the twentieth century thus come to replay many of the debates which had dominated the second half of the nineteenth. Droysen's complaint, in a letter of 1852, about the rise of what he called "crass positivism," and his crusade against it in a course which he taught from 1857 onwards, is echoed by antinaturalist social science just over a hundred years later. In terms of research practice, the anthropologists Bronislaw Malinowski (1884–1942) and A. R. Radcliffe-Brown (1881–1953) had inaugurated a tradition of field-work and participant observation which replaced the "armchair anthropology" of the nineteenth and early twentieth centuries. In the U.S., Clifford Geertz, following the British philosopher Gilbert Ryle, developed his notion of "thick description" into a reconstruction of ethnographic practice. Ryle was concerned with the sort of sensitive description which can differentiate between a wink and an involuntary blink or twitch, and all the mixed forms (mocking imitation of a blink and so on); for Geertz, a "thick description" is one which brings in the cultural context and thus makes sense of what is observed. Thus explanation in the social sciences is often not a matter of simplification or condensation, as in $f = ma$ or $e = mc^2$, but rather of "substituting complex pictures for simple ones while striving somehow to retain the persuasive clarity that went with the simple ones" [Geertz, 1973: 33].

> In finished anthropological writings...[the] fact — that what we call our data are really our own constructions of other people's constructions of what they and their compatriots are up to — is obscured because most of what we need to comprehend a particular event, rit-

> ual, custom, idea, or whatever is insinuated as background information before the thing itself is directly examined. [Geertz, 1973:9]

Geertz' *Interpretation of Cultures* thus brings out the way in which ethnography must be read as narrative and text — thus establishing a connection with more traditional hermeneutic theory (for a useful overview, see [Luhrmann, 2001]). In a related approach, the British anthropologist Mary Douglas studied the way in which societies categorise the world in simple oppositions between, for example, clean and dirty, where dirt means "matter out of place." Following the Durkheimian school, she saw these as the key to our understanding of the most fundamental opposition in social life between the "sacred" and the "profane." In this sense creatures or substances that fall outside of familiar categories or fall in between categories can be at once dangerous, or poisonous, *and* sacred. Transsexuals or social scientists, for example, may be suspect because they are "neither one thing nor the other" — in the latter case, neither cultured humanists nor "proper scientists." Douglas and those working with her have become increasingly interested in the political implications of this model, both in terms of looking at the internal power struggles within cultures and in conducting studies of public policy controversies. Her model is Durkheimian in its stress on the need for cultures to preserve themselves by rituals of solidarity and the punishment of deviance; it is *neo*-Durkheimian however in that cultures are also seen as divided and "adversarial" [Douglas, 2001].

Issues of social and cultural inequality are given a sharper twist in the work of Pierre Bourdieu (1931-2002), an anthropologist and sociologist who not only contributed in major ways to the sociology of knowledge, education and culture, but also put it at the centre of what he came to call reflexive sociology. Bourdieu shows how a strong conception of the separation of social science from everyday thought, something more usually associated with more objectivist approaches, can be combined with a sensitivity to issues of reflexivity and understanding. Bourdieu's rejection of structuralism had been partly driven by an awareness of the gap between formal rules, whether located in the heads of actors or those of social scientist, and the reality of practice(s). He was, therefore, suspicious both of sociological commonsense and of actors' concepts, and also of the formal models designed to replace them.

For Bourdieu, the sociology of knowledge is not a mere specialist field of the subject but rather an essential preliminary and accompaniment to sociological investigation. Like budding psychoanalysts who have to undertake a "training analysis" with an established practitioner, all sociologists, he argued, should undertake a more or less formal "autosocioanalysis." It is of course no accident that Bourdieu cut his teeth in ethnographic fieldwork, where the interplay and tension between the horizons of expectations of the anthropologist and the culture under investigation is explicitly thematized as a resource.

Whether or not they followed theorist practitioners like Geertz, Douglas, or Bourdieu, anthropologists tended to follow a practice of ethnographic research closer to what is being described in this chapter than did sociologists, for whom interpretive approaches competed with empiricist and functionalist ones. It is no

accident that Peter Winch, whose work is discussed below, mostly drew his negative examples from sociology (including that of Max Weber) and his positive ones from ethnographic field-work. There was however in sociology a well-established minority North American tradition of symbolic interactionism, which also experienced a certain resurgence in the 1960s with, for example, the republication of the work of G. H. Mead (1863-1931) and that of Herbert Blumer (1900-1987), and also the publication of various studies by Erving Goffman. Members of the Chicago School of sociology, some of whom shared Max Weber's main philosophical influences, notably Rickert and Windelband, had conducted ethnographic studies of local social problems, permanently shaping the image of sociology as paradigmatically concerned with the observation of "low-life."

When Schutz emigrated to the U. S., his closest contacts were with other followers of Husserl, but he was also led into a more intense engagement with the very strong North American philosophical tradition of pragmatism. This movement had developed in the late nineteenth century from the work of C. S. Peirce (1839-1914). Peirce stressed that questions of knowledge which had been central to Western philosophy since the time of Descartes should no longer be abstracted out of the practical context in which they occurred: that of people's active engagement with the world and their attempts to make sense of it. Thus rather than *a* problem of knowledge, or meaning, or truth, pragmatists are concerned with how we develop and test our knowledge and the concepts we form of things in the world. The popularity of this approach in North America is often explained rather simplistically by a cultural context in which settlers from Europe, escaping religious and other ideological conflicts back there, were more concerned with the practicalities of making a living in their new environment on the basis of hard facts and hard cash. It is however also worth noting the parallels between pragmatism and the related appeals to "practice" in the work of Marx and Engels or to "life" in that of Friedrich Nietzsche, and to the phenomenological analysis of consciousness in the philosophy of Henri Bergson (1859–1941).

By the end of the nineteenth century, William James (1842–1910) had systematised pragmatism as a philosophical approach and developed it in relation to, notably, the psychology of religious experience. James was exceptionally influential in Europe as well as North America. Social theorists such as Max Weber and Emile Durkheim were impressed by his work and, in Durkheim's case, interested in pragmatism as a philosophical approach which he saw as having affinities to his own variety of neo-Kantianism, though ultimately to be condemned for the "irrationalism" of its conception of truth [Durkheim, 1955: 28]. In the twentieth century, pragmatism was developed further by John Dewey (1859–1952) and George Herbert Mead (1863–1931). Dewey, like James, was concerned to work out the implications of pragmatism for other areas of knowledge and in particular for social and political philosophy and democratic theory. These social and political concerns were also an important, though often neglected, aspect of Mead's work and of that of later thinkers influenced by pragmatism such as Alvin Gouldner and Richard Rorty. Mead, though he published little systematic work himself,

reached a wider public though the work of his student Herbert Blumer and became one of the founders of social psychology and what Blumer called "symbolic interactionism."

Unlike philosophers and social theorists who began from the individual and his or her action, Mead focussed on situations of social interaction. In humans, this is symbolically mediated; we respond to others' gestures, rather than just to their behaviour, and we put ourselves imaginatively in their place, in what Mead called "taking the role of the other." These expectations may be momentary, as when I interpret your gesture as meaning that you are waving me on in a traffic queue, or more systematic; Mead distinguished between the "I," the individual ego, and the socially structured "me," made up of others' expectations of us. An individual may have multiple and overlapping "me"s, arising from different situations and roles — professional, personal and so forth.

Mead's approach implied a novel conception of knowledge, language-use (there are parallels with the Russian psycholinguist Lev Vygotsky), socialisation and so on, with some echoes of German Romantic philosophy and cultural theory. Most fundamentally, perhaps, it involved what Hans Joas [1992] has called a conception of the 'creativity' of action. Mead called his approach "social behaviourism," but it is very different from that of Watson (1878–1958) or B. F. Skinner (1904–1990). Whereas behaviourists focus only on observable behaviour and avoid any speculation about the mental processes which accompany it, Mead's interactive conception necessarily involves conjectures about the ways in which humans interpret each other's behaviour in complex structures of intentional action and interaction — what Harré and Secord [1972] later called "act-action structures." More generally it involves what Harré and Secord ironically called the "anthropomorphic model of man": "treating people as if they were human beings."

The term Symbolic Interactionism was introduced in 1937 by the Chicago sociologist Herbert Blumer; see also [Blumer, 1969]. The Chicago sociology department, founded by Albion Small in 1892, was not the first in the country, but it was the first to develop a collective conception of social research; this was oriented to the ethnic and other urban crises of early twentieth century Chicago and to reformist impulses from Jane Addams and others. An admirer of Simmel, Blumer saw his main achievement as bringing together George Herbert Mead's pragmatic philosophy, which Mead had himself developed into a social psychology, with the sociology of W. I. Thomas and others. Thomas is now remembered principally for his slogan: "If men (sic) define things as real, they are real in their consequences." This idea, as Blumer could show, fitted well with C. H. Cooley's idea of the "looking-glass self," based on our idea of how we appear to others (see Goffman, below) and Mead's distinction between the "I" and the "Me," where the latter refers to a self which is produced and constantly reshaped in social interaction and in the reciprocal exchange of perspectives. We might think of this in terms of a contrast between actors who are simply speaking from a script (though of course even then they are usually interacting with others) and the improvisation of "method" actors. In other words, we relate to people and things according to our interpretations of

them; we respond to a threatening gesture before it becomes a real threat, or to a friendly approach indicated by a momentary smile.

This 'creative' model of action can be contrasted with that of Talcott Parsons, also systematically presented in 1937 in his main work, *The Structure of Social Action*. Although Parsons described this conception of action as "voluntaristic," in contrast to economic or what later came to be called rational choice models, in practice Parsons emphasized an orientation to shared norms, in an approach increasingly criticized as conservative. And as in Schutz' model of typifications, in which those of the social scientist build on and reconstruct those produced by ordinary members of society, the interactionist sociologist's account is continuous with the social actor's own more or less conscious awareness of what they are doing, rather than offering a description or explanation at a radically different level.

Blumer's systematization of interactionism coincided with the eclipse of the Chicago School by other centres of US sociology, and it was particularly important because Mead himself did not systematically present his own work; his *Mind, Self and Society* (1934) was put together after his death. Interactionism continued as an oppositional current to functionalism as sociology expanded in the U. S. and U. K. after World War II, with the work of Blumer's students Anselm Strauss, Tamotsu Shibatani, Howard Becker and others. It experienced a certain revival with the growing opposition to functionalism in the 1960s and 1970s. By then, the idea that a theory should be precisely formulated, testable and of general application — an idea to which even many interactionists had subscribed, especially in social psychology — was giving way to a more pluralistic and informal conception of theories as sensitizing frameworks, closer to the original pragmatist ideas which had inspired Chicago sociology. An influential text by Glaser and Strauss [1967] formalized a version of this approach and the idea of "contexts of awareness," and Strauss in particular also did substantial work in the sociology of medicine.

Mead, as we saw earlier, had sometimes described his approach as "social behaviourism," and one variant of symbolic interactionism, the so-called Iowa School following Manford Kuhn (1911-1963), developed a more classically behaviourist focus on measurable role behaviour. The more influential and theoretically creative strand of interactionism, however, followed the original pragmatist model in stressing the informal and negotiated aspect of social roles and social interaction in general — notably in the ethnographic work of Erving Goffman, who is discussed in the next section. Interactionism is valued by many social scientists as a "sensitizing" perspective, even if they believe that it needs to be complemented by more structural analyses. The reproduction of, for example, class or gender relations in everyday interaction is obviously a crucial aspect of those relations, but material resources may also need to be taken into account.

Erving Goffman (1922-1982) obtained his doctorate at Chicago with a thesis based on fieldwork in the Scottish Shetland Islands. Although he published a dozen other major works, he remains best known for his first book, *The Presentation of Self in Everyday Life* (1956/1959). For Goffman, the notion of "performing" social

roles means just that : we are "on stage" in our everyday lives, moving between "front stage" and "back stage," dressing to create an impression, even if only an understated one, and constantly monitoring the impression we create. Sometimes, as in our homes, the stage metaphor is almost literal, as we admit visitors to some rooms or parts of rooms and not others. On the other hand, it serves in Goffman's presentation as a guide to a more fundamental issue. As he stresses at the end of the book,

> The claim that all the world's a stage is sufficiently commonplace for readers to be familiar with its limitations and tolerant of its presentation, knowing. . . that it is not to be taken too seriously. . . This report is not concerned with aspects of theatre that creep into everyday life. It is concerned with the structure of social encounters – the structure of those entities in social life that come into being whenever persons enter one another's immediate physical presence. The key factor in this structure is the maintenance of a single definition of the situation, this definition having to be expressed, and this expression sustained in the face of a multitude of potential disruptions. [Goffman, 1959 (1971): 246]

In a later book, *Frame Analysis* (1974), Goffman shows how situations can be shaped by a variety of alternative perspectives; we must use "frame clues" and "frame conventions" to know (or rather guess) whether someone is joking or serious, blinking or winking, polite or sarcastic, unaware of a social convention or deliberately flouting it. As with the pragmatist philosopher William James, the interactionist Anselm Strauss, with his focus on "awareness contexts," or in Schutz' analysis of the definition of social situations with the use of a "stock of knowledge at hand," we are confronted with multiple realities in the form of a choice between alternative perspectives. And these interpretations may be self-fulfilling; to misidentify a look as rude or hostile and to act accordingly may land you in hospital.

Goffman is often criticized for portraying a rather sad social world without sincerity or spontaneity, where people are constantly monitoring their performances and calculating their effects. Are all cultures as obsessed with impression management as he suggests, or is he falsely universalising particular features of advanced capitalist societies? The evidence of cross-cultural studies suggests however that Goffman may have been largely right in his assumptions [Smith, 1999]. Certainly the model of dramaturgical action should be put alongside those of norm-directed and economically rational or strategic action as part of the repertoire of social theory. (For Habermas these three types of social action need to be related to a broader notion of communicative action oriented by and to mutual understanding, to which he contrasts strategic action; see [Habermas, 1981; 1998].

But where the interactionists tended not to spend time on formal critiques of empiricism, Harold Garfinkel's "ethnomethodology" [Garfinkel, 1967] was more aggressive. One of Garfinkel's first studies, published in 1956, is titled "Conditions

of Successful Degradation Ceremonies," and explores both the dramaturgy of such situations and the ways in which the victim is defined as an outsider and the perpetrators are defined as acting in the public interest and according to universal values. Garfinkel later developed his analysis in three main directions. First, he looked more closely the reasoning processes through which people come to define situations in certain ways. Second, and relatedly, as a student of Talcott Parsons he reformulated in terms of everyday interaction Parsons' concern with what he called the "Hobbesian" question of the maintenance of social order. But where Hobbes and Parsons were concerned with the conditions of war and peace at the level of entire societies, Garfinkel was interested in the maintenance of order in interpersonal interactions. Third, Garfinkel realized that the implicit rules which structure social interaction could be identified through studying situations where they were breached, and that he and his students could deliberately cause them to break down.

Garfinkel coined the term ethnomethodology to describe the study of the reasoning processes routinely followed in everyday life, which he documented in studies of a trial jury and other sites of "mundane" reasoning. Conversational exchanges, he noted, were marked by what linguists call indexicality: the use of expressions like I, you, here, now which are given meaning by context. Elliptical expressions like "the next lecture will be in the other room" can be unpacked by listeners with the necessary background knowledge to mean: "the next lecture in this series will be in the second of the two lecture theatres which we are using this term." Forcing people to spell out what they mean by shorthand references of this kind is perceived as irritating and rude; one of Garfinkel's experiments involved asking people what they meant when they asked "How are you ?", and offering an unexpectedly detailed response. In another experiment, which demonstrates how we try to produce order and meaning in puzzling situations, a researcher posing as a counsellor gave a random succession of "yes" or "no" responses to the victim's requests for advice, leading him or her into more and more contorted attempts to reconstruct the logic of the counsellor's replies. Garfinkel draws the theoretical conclusion:

> In accounting for the stable features of everyday activities sociologists commonly select familiar settings such as familial households or work places and ask for the variables that contribute to their stable features. Just as commonly, one set of considerations are unexamined: the socially standardized and standardizing, "seen but unnoticed," expected, background features of everyday scenes. The member of society uses background expectancies as a scheme of interpretation. With their use actual appearances are for him recognizable and intelligible as the appearances-of-familiar-events. Demonstrably he is responsive to this background, while at the same time he is at a loss to tell us specifically of what he expectancies consist. When we ask him about them he has little or nothing to say. [Garfinkel, 1967: 36-7]; cf. Geertz, cited above).

Garfinkel was led, then, by his early study of the deliberations of a trial jury to emphasise, like Schutz, the importance of practical reasoning in everyday situations. The production of meaning is at the same time the production of social order — Parsons' major concern. Unlike his former teacher, however, Garfinkel insisted that social actors are not simply bearers of their social roles ("cultural dopes"), but active subjects obliged to practice social analysis in order to function in everyday society.

Like interactionism, ethnomethodology tended to become polarised between detailed sociolinguistic studies, in which its original antipositivistic thrust disappeared, and more speculative and essayistic philosophical reflections. The growth in the 1970s of cognitive science in conjunction with developments in artificial intelligence suggested for a time the development of a cognitive sociology [Cicourel, 1973]; this however did not take off in sociology as successfully as in anthropology where it has become a recognised sub-field [D'Andrade, 1995]. However, the characteristically interpretivist emphasis on the continuity between formal sociological reasoning and that carried on more informally by other members of society, the idea that we are all to some extent sociologically knowledgeable and skilled, has had a much wider influence in other types of social theory and has led to important explorations of the idea of reflexivity in modern social life.

Substantive work such as Garfinkel's was influenced by two further philosophical currents. The first was what had been a somewhat unexpected move within analytic philosophy, which in the earlier twentieth century had been dominated by logic and the philosophy of mathematics and science. Beginning on the margins of the logical empiricist Vienna Circle, Ludwig Wittgenstein had come to abandon the simple conception of a picturing relation between propositions and the world, and was drawn into a more sensitive and holistic analysis of the practicalities of "language-games" based on implicit rules and embedded in what he enigmatically called "forms of life." In a religious language-game, for example, words like prayer, sacred, holy, salvation etc. have a specific meaning which is given to them only by and in this context. Another important effect of Wittgenstein's analysis of language in social science was the introduction of the concept of "speech-acts" and "performatives." This was developed by the analytical philosophers John Austin [1962] and John Searle [1970/1]. When a priest says "I pronounce you man and wife," or the rector of a university says "I confer on you the title of Bachelor of Arts" they actually create the social fact of marriage or graduation. The analysis of linguistic competence and performance, and their implications, have been taken up a variety of ways by writers such as the French philosopher Jacques Derrida, in Habermas' model of "communicative action" and in the U. S. feminist theorist Judith Butler's work on "performativity."

An important book by the Wittgensteinian philosopher Peter Winch [1958] drew the consequences of Wittgenstein's concepts of language-game and "form of life" for social theory, using Max Weber, as Schutz had done, as one of the foils for his argument. For Winch, knowing a society meant learning the way it is conceptualised by its members. He thus revived the central principle of nineteenth

century German historicism, according to which every age must be understood in its own terms. He quotes in an epigraph Lessing's *Anti-Goeze*: "...it is unjust to give any action a different name from that which it used to bear in its own times and among its own people." Winch directly identified himself with the German idealist tradition by further insisting that social relations are 'like' logical relations between propositions [1958: 126] as well as, more concretely, with an ethnographic field-work approach [Winch, 1964]. Karl-Otto Apel [1967] brought out the similarities between analytic philosophy of language and the German tradition of the human sciences or *Geisteswissenschaften*; see also [Habermas, 1999].

Secondly, hermeneutic theory itself also took a new turn with the 'philosophical hermeneutics' of Hans-Georg Gadamer (1900–2002), whose *Truth and Method*, published in 1960 and translated into English in 1975, insists, in opposition to historicist hermeneutics, on the *practical* dimension of interpretation, conceived in Heidegger's sense of an "encounter" between the "horizon" of the interpreter and that of the text itself. Gadamer's philosophical hermeneutics is thus conceived in opposition to the methodological emphasis of traditional hermeneutic theories and their concern with the accuracy of interpretation. Gadamer's aim was to describe the underlying process, an existential encounter between two perspectives or horizons of expectation, which makes interpretationa possible in the first place. Understanding is not just a matter of immersing oneself imaginatively in the world of the historical actor or text, but a more reflective and practical process which operates with an awareness of the temporal and conceptual distance between text and interpreter and of the ways in which the text has been and continues to be reinterpreted and to exercise an influence over us. This effective history (*Wirkungsgeschichte*), which traditional historicist hermeneutics tends to see as an obstacle, is for Gadamer an essential element which links us to the text. Our pre-judgements or prejudices are what make understanding possible.

Although Gadamer often stressed that his *philosophical* hermeneutics, with its origin in Heidegger's hermeneutic ontology, was distinct from hermeneutics as a *technique* of interpretation, his approach clearly poses a challenge to more traditional conceptions of hermeneutics. These differences are brought out in particular in Gadamer's exchanges in the 1960s with Emilio Betti. The alternative conception of the human sciences or *Geisteswissenschaften* put forward in Gadamer's work also made it central to Jürgen Habermas' reformulation of the *Logic of the Social Sciences*. Habermas welcomed Gadamer's critique of hermeneutic objectivism, which he saw as the equivalent of positivism in the philosophy of the natural sciences, and also his stress on the totalizing character of understanding. For Habermas, however, Gadamer's stress on the fundamental nature of language, expressed in his claim that "Being that can be understood is language," amounted to a form of linguistic idealism. Together with Gadamer's stress on the importance of tradition and his rehabilitation of the category of prejudice, this suggested an ultimately conservative approach which was unable to deal with the "systematic distortion" of communicative processes by relations of power and domination.

Habermas and Gadamer debated these issues in the late 1960s and early 1970s

[Apel *et al.*, 1971]; see also [Bleicher, 1980; Scheibler, 2000; Harrington, 2001]; there are also echoes in Habermas's position of Adorno's earlier defence of an objective and contextualising hermeneutics, and this in turn has been carried forward by Ulrich Oevermann [Reichertz, 1986; Muller-Doohm, 2005]. Recent theorists have tended to stress the compatibility of hermeneutics and critical theory in a conception of critical hermeneutics [Thompson, 1981; Outhwaite, 1987]. More recently, Gadamer also engaged briefly with the French deconstructionist philosopher Jacques Derrida, whose conception of interpretation was (to put it very briefly) more sceptical.

Habermas' version of "critical theory" can be seen, along with critical realism and Anthony Giddens' structuration theory, as one of three particularly influential attempts in the final third of the twentieth century to reconcile, as Max Weber had done at the beginning of the century, the rival claims of explanation and understanding in the social sciences. Habermas, along with his close collaborator Karl-Otto Apel, argued for a complementarity between an empirical-analytic approach oriented to the explanation, prediction and control of objectified processes and a hermeneutic approach concerned with the extension of understanding, in an emancipatory model of critical social science, instantiated by psychoanalysis and the Marxist critique of ideology, which aims at the removal of causal blocks on understanding. Much of this remains in his more recent theories of reconstructive science and communicative action (see [Outhwaite, 2000]).

Critical theory tends towards a dualism of natural and social science, but in an increasingly muted form. The assumption that opposition to positivism also entailed dualism or antinaturalism was however also put in question in the late 1960s and early 1970s by the realist metatheory of science developed by Rom Harré and Roy Bhaskar. Both Harré and Bhaskar, like Habermas, were substantially motivated by the desire to undermine positivistic theories and approaches in the social sciences. Harré and Secord [1972] developed a philosophy for social psychology based on the work of the later Wittgenstein and the analytic philosophy of language practised at Oxford by J. L. Austin. Ordinary language, they argued, is better suited to the description of the mental processes of social actors than an apparently more scientific artificial terminology, and they drew attention to models of research practice of this kind in the work of Goffman, Garfinkel and others.

Harré and Bhaskar were in any case interested in giving a more adequate account of science as a whole, in world composed of relatively enduring structures and mechanisms. Some of these could be isolated in scientific experimentation, given the contingent existence of *homo sapiens* and *homo scientificus*. An important aspect of the realist programme developed by Harré, Bhaskar and others was a conception of explanation as involving not an essentially semantic reduction of causal *statements* to general laws but a reference to the causal powers of entities, structures and mechanisms. Causal tendencies might or might not be outweighed by countervailing tendencies, and two causal tendencies may neutralise one another, as do the centrifugal force of the earth's rotation and its gravitational

attraction, with the convenient consequence that human beings and other animals are safely anchored to the earth's surface.

This and other features of realism meant that the whole issue of naturalism could be rethought. Human beings could be seen as having causal powers and liabilities, just like other entities; it no longer mattered so much that their relations rarely sustained any universal generalisations of an interesting kind, but only sets of tendencies regular enough to be worth exploring. The fact that many of the entities accorded causal force in social scientific explanations were necessarily unobservable was not, as it was for empiricism, a problem of principle. And the understanding of meaning could, as Bhaskar put it, be seen as in some ways equivalent to measurement in the natural sciences. Finally, it seemed natural to include among the causes of human action the agents' reasons for acting - reasons which must be understood as far as possible.

The realist critique of traditional epistemology found an echo in social theory, notably in the work of Anthony Giddens, who had become similarly impatient with the residues of positivist social science as well as the more radical contentions of social constructionism. Giddens' conception of the "duality of structure" was designed to replace the traditional dichotomies between theories of social structure and social change, the micro-macro divide and between interpretive and more structural approaches. Approaches like these which aim to mediate between pure hermeneutics and more naturalistic conceptions of social science coexist, in the early twenty-first century, with more explicitly hermeneutic or phenomenological conceptions (See e.g. [Soeffner, 1989; Müller-Doohm and Jung, 1993]). Hermeneutics in a broader sense continues to exist as a major research tradition in the humanities, as well as a minority one in the social sciences [Shapiro and Sica, 1984]. More importantly perhaps, social scientists who would not sign up to an explicitly hermeneutic programme have at least accepted the importance of hermeneutic issues.

In the antirealist camp, Berger and Luckmann's relativistic sociology of knowledge, Garfinkel's ethnomethodology and social constructionism as a whole have been somewhat overshadowed by the rise of postmodernism in the 1980s. Lyotard's influential book on *The Postmodern Condition: A Report on Knowledge* was published in 1979 and in English in 1984, but it was his more sceptical work on language-games, along with Derrida's programme of 'deconstruction' and what came to be known in the English-speaking world as post-structuralism, which gave a new impetus to social constructionism. Many of the themes of what is now presented as postmodern sociology were however already present in ethnomethodology and the sociology of knowledge, as well as in other developments out of hermeneutics and the philosophy of language (see for example [Rorty, 1989; 1992]).

3   OPEN QUESTIONS

The diverse approaches discussed here can be related to one another in various ways, of which perhaps the best is Wittgenstein's notion of a family resemblance. It

is not so much that they share some single common feature, even such a general one as a concern with meaning, as that they are recognizably similar in their underlying approach to social life. We may not be able to identify a single proposition or set of propositions to which they would all subscribe, but they share a general orientation or style of social theorizing. One way to bring them together would indeed be with the notion of meaning, though some are happier with this term than others; those influenced by deconstruction and postmodernism are particularly dubious about the notion of fixed meanings and tend to talk about the "free play" of signs. The notion of meaning also points us to shared or at least related conceptions of definition, as noted earlier, which mark these approaches out in opposition to positivism. These would in turn have implications for more "central" philosophical concerns (at least in analytic philosophy) with the semantics of natural languages. More fundamentally these approaches point to the fact that, as Habermas [1968: 2] put it, "in the social sciences, heterogeneous aims and approaches conflict and intermingle with one another." This section explores some of these issues in more detail.

3.1 Explanation and/or Understanding

The relation between interpretive and other approaches has traditionally been framed in terms of the contrast between "understanding" (*Verstehen*) and explanation. There is however something unsatisfactory about setting things up in this way since, as Wittgenstein, Winch and Geertz showed in different ways, some descriptions are explanatory, at least in a preliminary way. Often what we call explanation in the social sciences takes the form of showing a *possible* reason why some observed effect has occurred – rather like transcendental arguments in philosophy which ask how some activity such as perception or science is possible. Such explanations are inherently open-ended, since it is always open to others to suggest an alternative explanation or to argue that the effect has been misdescribed.

We need first to decide what we mean by explanation. Curiously, for a research programme centrally concerned with the logic of science, logical positivism had a hopelessly inadequate model of explanation based on so-called "covering-laws" or constant conjunctions of events. Thus, in the much overused standard example, the law that pure water freezes when the temperature drops much below O degrees Celsius, together with the initial conditions, that I failed to put anti-freeze in my car radiator, "explains" the disappointing and costly scene which confronts me on a cold morning. Except of course that it doesn't really explain: it simply redescribes the situation in a way which directs attention to a genuinely explanatory account of the molecular properties of water and the mechanisms which lead it to solidify in cold conditions. Realist critics of this model, such as Harré and Madden, suggested that one should instead conceive explanation in terms of these mechanisms themselves. As Harré recently summarised the position:

> There are two post-Humean conceptions of causality that seem to be
> at work in both everyday commonsense understandings of the world

and at the sharp end of the physical sciences. The first kind is "event causality" where we take the causal relation to be between events as pairs of instances of certain event types. The question for an investigator would be what is the causal mechanism which is activated by the prior event and engenders the subsequent event. . . .

This will not do for the whole story of causality. There is the question of the site of the efficacy, of causal power to bring about effects. This is agent causality, the idea of a continuously existing being, continuously active which can bring about events without being stimulated in any way. [Harré, 2002: 112]

It should be clear, I think, that in either of these variants realist models of causality are open to application to the human and social worlds in a way in which covering-law models are not. To force explanations of, say, the French Revolution into a covering-law pattern is simply perverse, as are Lofland's ironic reformulations of Goffman:

If persons are placed in total institutions, *then* their selves will be mortified.

If persons are placed in total institutions, *then* they will develop secondary adjustments to protect themselves. . . [Lofland, 1980], (cited in [Atkinson,1996: 42]; cf. [Outhwaite, 1987]).

With this epistemic obstacle cleared out of the way, we are still of course left with the more substantive question of whether we actually want causal explanations or descriptions, *Verstehen*, or perhaps something like historical narratives which deliberately blur or transcend the distinction between description and explanation. Running briefly back over the approaches mentioned earlier, we can see that the nineteenth century antipositivists mostly opted for understanding as something seen as distinct from explanation, while Max Weber wanted to combine both, as did Schutz in a rather more complicated way. Wittgenstein introduced a further option, with a set of critiques of explanation and some elements of an alternative model which one might call perspicuous (*übersichtlich*) (re)description. As Theodore Schatzki pointed out in a useful overview, Wittgenstein's general philosophical method is one of description of cases of something which elucidates the concept [Schatzski, 1983; 125]. Applied to the human or social sciences (Wittgenstein was of course particularly interested in psychology and anthropology), this means that we

arrange the factual material so that we can easily pass from one part to another and have a clear view of it — showing it in a "perspicuous" way.

This perspicuous presentation makes possible that understanding that consists just in the fact that we "see the connections" (*Zusammenhänge*). [Wittgenstein, 1966: 241]

Thus, just as philosophy practised in this way removes some self-generated or at any rate unnecessary puzzlement or "bewitchment," social analysis renders intelligible something that was previously obscure — in this case, magic and religion. In so doing, it removes the need for explanation [Wittgenstein, 1966: 43-4]; alternatively, one might say that the description is itself explanatory, rather as for Hollis [1977] "rational action is its own explanation." The question why I am sitting at the computer is answered by the description of my activity: writing an article. This model, I think, captures rather well a lot of what is done in ethnography or the sociology of everyday life. In the case of a more obscure practice, such as the Balinese cockfight studied by Geertz, a more complex or thick description might be required, but not something qualitatively different.

Wittgenstein seems however to go beyond this modest formulation into a more explicit hostility to explanatory theory as not just prone to over-simplification and unwarranted generalisation but inevitably condemned to it. This is not just the point made explicit by Schutz in his postulate of adequacy quoted earlier: that "... The thought objects constructed by the social scientists ... have to be founded upon the thought objects constructed by the common-sense thinking of men (sic) living their daily life within their social world" [Schutz, 1962: 59]. It is more that he seems to fear that any theorist is going to be a 'terrible simplificateur'.

Some practitioners of interpretive social science would accept this and see their task as essentially descriptive. Even theorists feel this impulse from time to time; a good example is Bourdieu's *Misère du monde* (1993), though he also provides in the postface a theoretical justification for an approach which is more descriptive than that in his other works. Critics of Wittgenstein might reasonably point out that Fraser's theory of religion and Freud's theory of dreams are relatively easy targets and that he is himself generalising beyond what is justified by the evidence; Schatzki's attempt in a closing footnote to rally support for this approach from Foucault, Gadamer, Geertz and Habermas (!) is not particularly convincing. Personally, I would prefer to stick with a weaker (though still somewhat unorthodox) claim that the attempts to differentiate between description and explanation are mostly over-simplified.

3.2   One Model or Several?

I have of course tried here to differentiate the various positions discussed. It is also possible to relate the different interpretive conceptions discussed in this chapter in such a way that they complement each other. In part, this is just a matter of history. Symbolic interactionism was virtually unrepresented in Germany in the mid-twentieth century, yet it can reasonably be portrayed as practising what there was theorised as 'verstehende Soziologie' [Helle, 1992]. There are also of course important differences, notably between phenomenologically inclined sociologists and partisans of "objective hermeneutics." These are well documented in the published proceedings of a conference in 1990 at which I unsuccessfully attempted to pour oil on the troubled waters (Jung and Müller-Doohm 1993). For all that, there are

substantial commonalities if one compares these two camps to, say, empiricists or rational choice theorists.

More particularly, as Habermas [1968] showed, symbolic interactionism and phenomenological sociology can be enriched by a closer attention to language; ethnomethodology in the work of Garfinkel, Cicourel and others, is a major example of this enrichment. Similarly, Gadamer's notion of the fusion of horizons, the intermediation between competing perspectives, forms a useful correction to Winch's radical relativism. Finally, as suggested in the discussion above of Habermas, Bourdieu and Giddens, it may be useful to complement interpretive approaches as a whole with more structural perspectives drawn from critical theory, structuration theory, reflexive sociology or realism. Some interpretive theorists would deny the need for this, just as some empiricists, functionalists or rational choice theorists would deny the need to pay any attention to hermeneutic issues.

Here, then, we are back with familiar controversies over the place of sociology and the other social sciences "between literature and science' [Lepenies, 1985]. In some, such as history and social anthropology, an interpretive approach may be accepted more or less automatically; in others, such as economics, it will seem very exotic and eccentric. But what I think the debates throughout the past century have shown is that interpretation is not just an option in social theory; it is the way in which we get access to the social world. Precision of meaning, as [Bhaskar, 1979: 59] neatly put it, has something like the same importance in social theory as has precision of measurement in many areas of natural science.

4   PHILOSOPHY AND THE HUMAN SCIENCES

The other notable feature of the perspectives discussed above is the intellectually cosmopolitan interplay between more specifically philosophical perspectives and concerns and those of substantive social theory and research practice. The kind of dislocation sadly evident in mid-twentieth century philosophy of social science between a normative model of science taken as a unified whole (the "standard view') and the actual practice of the individual social science disciplines is largely absent from the tradition discussed here. In the early twentieth century this is not so exceptional. Simmel contributed even-handedly to what we would now tend to think of as distinct "disciplines" of philosophy and sociology, and Max Weber, though he sometimes apologised for reluctantly getting into philosophical debates, regularly did so. And so, from a somewhat different perspective, did Durkheim. In the second half of the twentieth century, however, with disciplinary consolidation often meaning that the Philosophy Department is some distance away from those of Sociology and Anthropology (themselves more often separated than conjoined), the close interplay in the thinkers and works discussed above looks more remarkable.

One way of illustrating this is biographical. Many of these thinkers moved on from a philosophical training into the theory and practice of anthropology and/or sociology (Schutz, Bourdieu). Others remained officially within philosophy but

are most prominent in the social sciences (Winch, Lyotard, perhaps also Hollis and Elster). A third category migrate back and forth between philosophy and sociology posts or hold joint appointments (Habermas, Honneth, Harré and one of the editors of this volume). "Social theory" or sometimes just "theory" have become convenient labels to express this fluidity.

How might one explain this particularly close relation between philosophy and the social sciences? Winch, of course, has the most dramatic answer in terms of linguistic idealism: it is that "social relations are expressions of ideas about reality" [23]; "social relations between men exist only in and through their ideas" [123]. Or as he put it a little later,

> Reality is not what gives language sense. What is real and what is unreal shows itself *in* the sense that language has. [Winch, 1964: 309]

In other words, *contra* an empiricist conception in which language-use is basically just reference and a materialist one in which ideas are either inaccessible or relatively insignificant, both are here made central to the ontological constitution of the social world. Although not many of us might want to go this far, the 'linguistic turn' in social theory has been quite far-reaching. Again, anthropology was there first, giving a particular importance to the nuances of language-use and the need for researchers to learn "native" languages rather than rely on translation and interpretation. But is has become well accepted in sociology as well; recent work on emotions, for example, has addressed the issue of whether one needs a particular vocabulary in order to *have* (as opposed to just express) complex emotions.

Something of this can be seen in questions of definition and concept formation. Positivists tend to mean by definition simple stipulations that "x (as I use it here) *means* abc". Interpretive theorists and researchers, by contrast, will tend to be more sensitive to the broader associations, including historical associations, of the words we use, and they also tend to tend to use the word definition in an extended sense, as in "definition of the situation" — a broad conception which frames individual experiences. Goffman's "frame analysis" is a good example of this, as is the movement from part to whole in classical hermeneutics. More generally, one can argue that the more we think of truth as the completeness of a Gestalt rather than the accumulation of true propositions, the more we shall pay attention to qualitative concerns. One of these might be with the quality of individual experience, a classical philosophical theme which in turn raises further issues of the nature of human and personal being [Beyleveld and Toddington, 2005].

Another way of making these connections would be through an analysis of rationality. I have not given much attention here to rational action theory, since this is discussed elsewhere, but in its softer and less economistic variant represented by Jon Elster and in particular by Martin Hollis there is a close connection with hermeneutics [Hollis, 1977, esp. chapter 8]. Hollis may be a little brisk, as he concedes, with his provocative slogan that "rational action is its own explanation" [21], and in practice imputations of rationality involve complex acts of interpreta-

tion and definition of the situation. A model of this kind can be used to explain collective action, as in Mancur Olsen's work [1965] and in "rational choice Marxism," and to reconceptualise issues in the analysis of social structure, as attempted by James Coleman [1990], and fundamental issues in philosophy and social theory, notably by Hollis and Elster. Hollis's co-authored book with Steve Smith is a friendly battle between his position and other, more structural conceptions of explanation, represented by Smith [Hollis and Smith, 1990]. In a rather different approach, Habermas also points to the centrality of rationality issues for the social sciences. As he puts it at the beginning of *Theory of Communicative Action* [1984: 7], "...*any* sociology that claims to be a theory of society has to face the problem of rationality simultaneously on the *metatheoretical*, *methodological*, and *empirical* levels."

On the other hand, and it is a big hand, there is clearly a lot of work in the philosophy of language, mind, action and so on which is remarkably remote from these connections and which operates in a more naturalistic way without attention to the social. I would wish to distinguish between good and bad reasons for this. A good reason, I suggest, is that a focus on the cognitive and the psychological may reasonably mean resisting strong conceptions of socialisation. A psycholinguist may argue with good reason that the really interesting thing about language acquisition by infants is that that they have the capacity to acquire language at all, rather than the relatively trivial social details *between* the acquisition and use of different languages or those between monoglots and polyglots. It is similarly undeniable that a large part of our behavioural capacities are the result of nature rather than nurture, and there may be good reasons for attending to the former rather than the latter.

Among the bad reasons for the separation, I suggest, are the notion, particularly strong though, I think, weakening, in analytic philosophy, that there is a clear-cut difference between conceptual issues, which are the concern of philosophers, and empirical issues in the individual sciences. When I learned philosophy at the end of the 1960s, I was taught, for example, that there was something called the "causal theory of perception," with a strong implication that it was somehow missing the point and blurring the distinction between the conceptual and the empirical. Such clear-cut separations were undermined by Quine and, later, Rorty in philosophy, Kuhn in history of science, and a number of philosophically inclined sociologists ranging from Heideggerian ethnomethodologists and social constructionist sociologists of science to Marxist realists. This convergence of philosophical and sociological concerns can be found, for example, in the work of Anthony Giddens and other leading contemporary theorists. *The Constitution of Society* [Giddens, 1984] can be read alongside Bhaskar's *Possibility of Naturalism* [1979] or Margaret Archer's *Realist Social Theory* [1995] and their ostensible disciplinary affiliations (to philosophy in Bhaskar's case, sociology for the other two) are a matter of emphasis rather than substance.

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THE ORIGINS OF ETHNOMETHODOLOGY

Michael Lynch

There are several ways to reconstruct the origins of ethnomethodology, some of which are more compatible with that approach than others. An account of its origins not only is of historical interest, but it also is important for understanding what ethnomethodology *is*, in terms of its identity, meaning, and significance. In this chapter, I will develop four different accounts of ethnomethodology's origins: a discrete origin story that situates ethnomethodology in an intellectual biography; a conventional academic history that traces ethnomethodology back to lines of social theory and philosophy; an account of a revolutionary "break" with the existing social sciences; and, finally, a substantive genealogy that identifies ethnomethodology with the "things" it investigates. My aim is twofold: first, to provide a record of ethnomethodology's origins; and, second, to convey ethnomethodology's distinctive perspective on its own origins.

What is Ethnomethodology?

Ethnomethodology has been a source of bafflement, frustration, and outright hostility for many of its interpreters — especially the doyens of American sociology who pronounced opinions about it in the 1960s and '70s.[1] Moreover, the development of the field has involved a confusing divergence between different lines of work, many of which have become intertwined with other disciplinary and sub-disciplinary developments in sociolinguistics, science and technology studies, workplace studies, organizational studies, and various topical specialties within sociology. Consequently, the question "What is ethnomethodology?" continues to arise, despite the fact that for the past 40 years it has been asked and answered many times over.

The word "ethnomethodology" was coined by Harold Garfinkel, the "founding father" of the field, and the word is now officially enshrined in the English language, as indicated by its place in the *Oxford English Dictionary*.

> *Sociol.* [f. ETHNO- + METHODOLOGY.] A style of sociological analysis associated with H. Garfinkel (b. 1917), which seeks to expose and analyse the methods by which participants in a given social situation construct their commonsense knowledge of the world. [*OED*, 1989: 425]

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[1] See, for example, a review symposium in the *American Sociological Review* on Garfinkel's [1967] *Studies in Ethnomethodology*, which included Coleman [1968]. Also see Goldthorpe [1973], Coser [1975], and Gellner [1975]. Goldthorpe's and Coser's pieces touched off critical exchanges (see Benson [1974], and Goldthorpe [1974]; Zimmerman [1976], and Coser [1976]).

The word still invites confusion and consternation, and the *OED* definition leaves much to the imagination.

The first chapter of Garfinkel's *Studies in Ethnomethodology* [1967] is titled "What is ethnomethodology?" However, readers who seek a concise, extractable, and transportable definition are likely be disappointed, as the chapter that addresses the question is rife with some of the thickest (and, arguably, the best) prose in the history of social science writing. Some readers were inclined to scream in protest at "[t]he determinedly esoteric and often impenetrable language" [Goldthorpe, 1973, 449]. The closest thing to a definition is: "I use the term 'ethnomethodology' to refer to the investigation of the rational properties of indexical expressions and other practical actions as contingent ongoing accomplishments of organized artful practices of everyday life" [Garfinkel, 1967, 11; also see p. vii].

This definition may seem circular, as it implies that "practical actions" are to be investigated as "accomplishments" of "practices of everyday life." Moreover, the assignment of "rational properties" to "indexical expressions" (linguistic expressions whose sense depends upon the local context of use) disrupts common expectations about what *rational* can possibly mean. How could "rational properties" be construed as "contingent [non-rational?] ongoing accomplishments"? It is possible to understand that the circularity is less than vicious, and that the proposition about "rational properties" makes sense (see [Garfinkel and Sacks, 1970]), but to do so requires a lengthy explication of the themes of indexical expressions, reflexivity, and accountability, and a deeper engagement with the studies and demonstrations that fill the pages of *Studies in Ethnomethodology*. A definition cannot possibly substitute for such engagement, and yet definitions continue to be demanded.

Thirty-five years later in his second book, and again in the first chapter, Garfinkel [2002: 91] returns to "THE QUESTION" as he calls it, though this time he uses a lighter and more ironic touch:

> Ethnomethodology gets reintroduced to me in a recurrent episode at the annual meetings of the American Sociological Association. I'm waiting for the elevator. The doors open. I walk in. THE QUESTION is asked, "Garfinkel, what IS Ethnomethodology?" The elevator doors close. We're on our way to the ninth floor. I'm only able to say, "Ethnomethodology is working out some very preposterous problems." The elevator doors open.

> On the way to my room it occurs to me that I *should* have said Ethnomethodology is respecifying Durkheim's lived immortal, ordinary society, evidently, doing so by working out a schedule of preposterous problems. The problems have their sources in the worldwide social science movement. They are motivated by that movement's ubiquitous commitments to the policies and methods of formal analysis and general representational theorizing and by its unquestionable achievements.

Of course, had Garfinkel given his interlocutor the longer answer, he could expect to be met with a blank stare and nervous exit from the conversation. To a casual reading, the more elaborate answer is no less elusive than the quip in the elevator. Later, we shall pursue some of the terms in this answer, especially the idea that the "preposterous problems" taken up in ethnomethodology "have their sources in the worldwide social science movement." But for the moment, Garfinkel's "definition" of ethnomethodology is likely to seem preposterous.

Perhaps a better way to begin to come to terms with ethnomethodology — both the word and the research programme — is to tell an origin story. Fortunately, there is one on record.

1   A DISCRETE ORIGIN STORY

Ethnomethodology is unusual among sociological research programs, because it can be traced to a deliberate attempt to launch a novel research program. According to his own account, Harold Garfinkel coined the term "ethnomethodology" in the 1950s, in an attempt to bring about a radical shift in sociological perspective. Most other sociological research programs have had less discrete and deliberate beginnings. Symbolic interactionism, for example, is traced back to George Herbert Mead's teachings in social philosophy, but the sociological approach that goes by that name was a retrospective construct by Mead's students rather than an explicit program of his own devising. "Conflict theory" is a gloss used by textbooks to lend coherence to an amalgamation of Marxist and Weberian themes and theses, and "rational choice theory" is a spin-off of neoclassical economics. Perhaps the closest parallel to the "invention" of ethnomethodology, and one that is not incidental in its history, was the effort by Talcott Parsons in the 1940s to re-invent sociology. The story is told that during a meeting at Parsons' house attended by some of his prominent colleagues, Parsons' young son marched through the gathering, banging a drum and exclaiming "Sociology starts here!" However, Parsons' ambition was not to invent a novel perspective, but to synthesize the entire field, and to present his inclusive synthesis as the culmination of sociology's internal development. Garfinkel's "invention" — though its origination no doubt becomes more coherent and intentional when viewed retrospectively — was a reaction to the orthodoxy that Parsons, his students, and allies successfully wrought for sociology from the 1940s through the early '60s.

Ethnomethodology's origin story first appeared in print in *The Purdue Symposium on Ethnomethodology* — a transcribed symposium at Purdue University [Hill and Crittenden, 1968]. It was excerpted in a 1974 anthology [Turner, 1974], and has been recounted in a number of other sources [Heritage, 1984, 45; Lynch, 1993, 4; Rawls, 2002, 4-5]. The Purdue Symposium was staged as an encounter between proponents of ethnomethodology and other sociologists who were variously interested or disinterested in the topic. Garfinkel, who was the main exponent of ethnomethodology at the symposium, began the session with a story about how he came up with the word while participating as a postdoctoral researcher in a

study of an audiotaped jury deliberation (this was part of a larger study of the American Jury).[2]

> I was interested in such things as jurors' uses of some kind of knowledge of the way in which the organized affairs of the society operated — knowledge that they drew on easily, that they required of each other. At the time that they required it of each other, they did not seem to require this knowledge of each other in the manner of a check-out. They were not acting in their affairs as jurors as if they were scientists in the recognizable sense of scientists. However, they were concerned with such things as adequate accounts, adequate description, and adequate evidence. They wanted not to be "common sensical" when they used notions of "common sensicality." They wanted to be legal. They would talk of being legal. At the same time, they wanted to be fair. If you pressed them to provide you with what they understood to be to legal, then they would immediately become deferential and say, "Oh, well, I'm not a lawyer. I can't be really expected to know what's legal and tell you what's legal. You're a lawyer after all." Thus, you have this interesting acceptance, so to speak, of these magnificent methodological things, if you permit me to talk that way, like "fact" and "fancy" and "opinion" and "my opinion" and "your opinion" and "what we're entitled to say" and "what the evidence shows" and "what can be demonstrated" and "what actually was said" as compared with "what only you think he said" or "what he seemed to have said." You have these notions of evidence and demonstration and of matters of relevance, of true and false, of public and private, of methodic procedure, and the rest. At the same time the whole thing was handled by all those concerned as part of the same setting in which they were used by the members, by these jurors, to get the work of deliberations done. That work for them was deadly serious. (Garfinkel, in [Hill and Crittenden, 1968, 6–7]; reprinted in [Garfinkel, 1974, 15–16].)

This origin story alerts us to a particular sense of the word "ethnomethodology" as, literally, peoples' methods; in this case, jurors' methods of deliberation. Ac-

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[2]Garfinkel [2002, 79-80] provides a more concise account and acknowledgement:

> Ethnomethodology began with the announcement by Saul Mendlovitz and me after we finished our studies of audiotaped jury deliberations in the federal district court in Wichita, Kansas. That research was arranged by the Chicago Law School faculty. Edward Shils was the instrumental party with the Ford Foundation and Fred Strodtbeck directed the project. That Strodtbeck chose Mendlovitz and me brought us at the project's end to Ethnomethodology for which I offer my continuing thanks. In our presentation to the annual meetings of the American Sociological Association in 1953 we recommended that there be added to anthropology's guide to the "ethno" sciences, a guide that ran from EthnoAstronomy to EthnoZoology, one more "ethno" side of social science preoccupation with science—Ethnomethodology.

cordingly, "methodology" becomes a topic for investigation, and is not limited to the professional techniques or procedures of a social science discipline. Consequently, "ethnomethodology" is as much a name for *what* ethnomethodologists study, rather than (as the *OED* would have it) a name for their common "style of analysis." There is, of course, nothing original about making a topic of reasoning, reasoned talk, and knowledge. The ancient discipline of logic, and the modern fields of cognitive psychology, linguistic philosophy, and the sociology of knowledge, among many others, all have had their say about "lay" reasoning, as well as the varieties of "magnificent methodological things" associated with law, science, and other specialized practices. If this were all there was to it, ethnomethodology would be a new name for what social scientists had been investigating all along: how beliefs, ideologies, practical know-how, and discursive competencies gear into and contribute to the organization of societies. However, in his programmatic writings Garfinkel goes to great lengths to distinguish ethnomethodology from any and all social science investigations of human reasoning: he insists that standards of logic, empirical adequacy, and causal analysis are endogenous properties of the methods studied rather than independent metrics available to professional social scientists for evaluating, undermining, or supplementing those methods.

Perhaps another anecdote — involving another study of courtroom events at a much later time — might help clarify the orientation to "reasons" and "reasoning" adumbrated in Garfinkel's origin story. In 1979, I was involved in a study of court processes with James Wilkins and Augustine Brannigan at the Centre of Criminology, University of Toronto.[3] In one particular study, we attended a Superior Court trial, and were granted permission to meet with the judge in his chambers immediately after he pronounced the verdict (there was no jury in this trial). Wilkins, Brannigan, and I came prepared to ask the judge about how he interpreted the testimony, and how he arrived at the verdict, but before we could even ask a question the judge admonished us by saying that if we expected him to give us reasons for his decision, we should know that *he already had given his reasons in court.* He forthrightly refused to offer any "reasons" other than those he pronounced openly in the public space of the courtroom. If we were in search of "real reasons", which would stand in ironic contrast with the formal reasons given in court, we were looking in the wrong place. Moreover, the judge insisted that the reasons he gave in court *were the real reasons.* His insistence placed us in a dilemma that is all-too-familiar for social scientists: either we could accept his account at face value, thus deferring to the judge's authority, or we could attempt to undermine his account by claiming, somehow, that we had access to a hidden, behind-the-scenes, misrecognized, or even unconscious source of the actions we witnessed in the court. But how could we get access to such hidden "reasons" (inadmissible reasons, tacit motivations, or even non-rational *causes* of reasons)? Even if the judge had confided in us and confessed a hidden motive behind his

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[3]The project took place in 1979, and was headed by Professor James Wilkins, Centre of Criminology, University of Toronto. Publications that came out of the study include Brannigan and Lynch [1987] and Lynch [1997].

verdict, why would we trust *that* account any more than we would trust the "official" account he gave earlier in the courtroom? There are, of course, many tried and true inferential and technical ways to bypass an agent's control over the accountability of their actions, but Garfinkel's origin story — and the very idea of ethnomethodology — suggests a radically different alternative to all of these inferential procedures. The alternative he recommended is to investigate *practices* of accountability *in context*. Accordingly, the formal, publicly stated reasons given in court are neither accepted at face value nor undermined by reference to real reasons, real motives, or real structural conditions known to the sociologist and ignored (or denied) by the agent in question.

A parallel to this situation can be found in social studies of science. It has long been noted that scientists' published accounts of methodology (in, for example, autobiographies by notable scientists,[4] or in sections of research papers devoted to recounting the protocols, equipment, and other materials) do not provide literal accounts of the actual practices of experiment. In the title of a famous essay, Peter Medawar [1964] went so far as to ask "Is the scientific paper fraudulent?" — answering: "Yes; it misrepresents scientific thought." At the time, philosophical and sociological accounts of "scientific thought" had a symbiotic relationship with official versions of methodology. Philosophical accounts of the logic of empirical inquiry, and sociological accounts of the norms of science were remote from the practices of laboratory or field research, but they performed a legitimating function for the natural sciences. Characterizations of rational procedures, universalistic aims, and the communal sharing of results helped advance the claim for public support of basic research with few strings attached. The campaign was successful as natural and social science research garnered increased public support and underwent tremendous expansion in the postwar period. However, at the height of this expansion in the 1960s a "revolution" (most prominently represented by Thomas S. Kuhn's [1962] *Structure of Scientific Revolutions*) disrupted the harmonious relationship between the sciences and the philosophy, history, and sociology of science. Philosophy of science broke free of its "underlaboring" effort to develop rational reconstructions of scientific truth; history of science conspicuously sought professional distance from scientists' personal reminiscences and reflections; and sociology of science took on the programmatic task to conduct empirical research on the actual practices of experimentation. This revolutionary movement in science studies was sometimes heralded as a "sociological turn" because of the emphasis on the way scientific knowledge depended upon agreements and resolutions of controversy produced through contingent social processes, which were not adequately described through rational reconstruction. Historians closely examined laboratory notebooks in search of discrepancies between actual experimental runs and published results; sociologists examined historical controversies, in search of

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[4]Interestingly, one of the major sources of the insight about the discrepancy between official versions of science and the actual "messiness" and contingency of scientific practice is itself an autobiographical story: James D. Watson's [1968] personal account of the discovery of the double helix (see especially the commentaries in the Norton Critical edition [Watson, 1980]).

evidence that "closure" was not a simple consequence of a crucial test, or an agreement among all (reasonable, knowledgeable) parties; and ethnographers conducted "laboratory studies" in search of an "actual", closely observed, situationally located science that contrasted with published, rationalist versions. On the basis of such research, it became commonplace (and still is commonplace) to say that scientific practices in their actual habitats are quite messy — fraught with ad hoc judgment and improvisation, contentious, unsettled — in contrast to the tidied-up versions one reads about in research reports and edifying biography.

Ethnomethodology often is identified with the "new" sociology of science, because it was mentioned prominently as a source of methodological guidance and theoretical insight for laboratory studies [Latour and Woolgar, 1979; Knorr-Cetina, 1981], and because Garfinkel and his students themselves conducted studies that were contemporaneous with, and thematically related to, laboratory studies [Garfinkel *et al.*, 1981; Lynch *et al.*, 1983; Lynch, 1985; Livingston, 1986]. However, while it is easy enough to gloss over the differences, there is a significant difference between ethnomethodological studies of natural science and mathematics and the interest expressed in most other sociological and cultural studies of scientific practices. Moreover, this difference has to do with the very meaning of "ethnomethodology". Literally, "ethnomethodology" means people's methodology. Associated with the array of ethnoscience approaches developed in the 1940s — ethnobotany, ethnopharmacology, etc. — it would mean a study of local "native" conceptions, terminologies, taxonomies, explanations, and practices in the domain of "methodology." A contrast between "native" beliefs and classifications, and those assumed by an investigator (and the investigator's readers) educated in the relevant domain of scientific knowledge, provides a basis for elucidating distinctive, contextually meaningful constellations of native knowledge. If, as is often assumed, the natural sciences provide the paradigm of methodology that has been emulated by many social scientists and disavowed by others, then an *ethnomethodology of science* would not have recourse to a clear distinction between "native" and "Western scientific" methodologies, since the "natives" already possess strong claims to knowing the relevant methodological prerequisites for their practices. Unlike Garfinkel's jurors, they are unlikely to profess humility in the face of relevant professional concerns with their rationality.

When writing *about* science as a general topic, rather that in connection with their own specialized researches, scientists do sometimes profess ignorance of *philosophy* of science. However, such confessions of philosophical naivety do not admit ignorance about *science*. Instead, as Nobel laureate Steven Weinberg [2001] has expressed it, non-philosophical naïve realism guides the scientists' apprehension of the world revealed through research. As far as Weinberg is concerned, a philosophy that serves to articulate such realism, and even one that problematizes it, is secondary, holding no direct interest or value for the practitioner. Lewis Wolpert [1992] makes a similar argument, though with the added twist that he endows the scientist with a special knowledge that contrasts with (and is inaccessible to) common sense, and he makes no effort to hide his contempt for the various schol-

arly discourses that would question the epistemic privilege he assigns to science. And so, unlike the humble jurors who Garfinkel paraphrases ("I'm not a lawyer, you're a lawyer" [which, by the way, Garfinkel was not and did not claim to be]), scientists like Weinberg and Wolpert who acknowledge that they know little of "science studies" also make clear that they could not care less about such studies.

A problem with Weinberg's treatment of the issue, and even more with Wolpert's, is that they want to have it both ways. They want to be able to write *about* science for a general readership (not just the science they know best, about which there is little mention in their more polemical writings), and yet they also want to invoke their naïve experience as a kind of native authority on epistemological and ontological matters. In other words, they want to invoke an inarticulate basis for knowing, not only how to construct particle physics theories or design embryology experiments, but as a basis for making claims about the *general* nature of science. By driving a wedge between know-how and knowledge-about, it is possible to invoke *docta ignorantia* [Kaufmann, 1944] — the doctrine that one does not (really) know what one knows — in order to privilege an outsider's articulation of the grounds, limits, and contexts of knowledge over the know-how of the embedded practitioner, but in the case of scientific methodology, the analyst faces a formidable challenge. Scientific methodology is commonly understood to be rationally self-possessed, reflexively guided, and subject to rigorous criticism by skeptical colleagues and competitors. Following Medawar, it is possible to suppose that the common presentation of methodology is incorrect and even hypocritical: that it is a convenient self-justificatory rationale; and that scientists are, in fact, unreflexive, myopic in their partisan preference for their own theories and methodological choices, and prone to "forgetting" the history of decisions, choices, negotiations, and battles won and lost, that lie behind their factual accounts. The elucidation of "actual" scientific practices would thus await the entry of a disinterested party, personified by an ethnographer or another type of historical, social, or cultural analyst. Accordingly, the natural scientists studied become unreflective native practitioners and the disinterested analyst brings to light, scientifically, the real nature and causes of their practices [Bloor, 1976].

As signaled, though not yet elaborated, in Garfinkel's origin story, an ethnomethodologist casts the relationship between social analyst and members (whether humble jurors or exalted scientists) in a way that does not accord the analyst with superior insight, reflexive awareness, or perspicacity. Whatever insight, reflexivity, and perspicuity the analyst attains becomes contingent upon that relationship, and indeed is part of the phenomenon. We shall return to this theme in the next section.

2   A CONVENTIONAL ACADEMIC HISTORY

It would be possible to expand the discrete origin story by building more of a biography around it (see [Rawls, 1999; 2002]), but to fully appreciate Garfinkel's "invention" one needs to link his biography to personal and impersonal sources

and intellectual influences, and to take stock of contemporaneous and subsequent developments that involved others, many of whom took ethnomethodology in directions that Garfinkel's neither anticipated when he first came up with the idea nor later approved of when confronted with those developments. A standard way to relate this broader origin story is in a conventional academic history. By "conventional academic history," I mean an intellectual history of ideas, themes, key terms, principles and academic networks characteristic of a discipline, field or subfield, theory, or method. In other words, it is a history of a research program that focuses on a succession of key ideas and leading figures, and takes stock of the individuals and groups that influenced them. The best known, and arguably the best, of the available accounts of ethnomethodology's intellectual origins is John Heritage's [1984] *Garfinkel and Ethnomethodology*.[5] Like other scholars who have looked into ethnomethodology's academic origins, Heritage places primary focus on Garfinkel as the "founding father" of ethnomethodology, and identifies the main sociological and philosophical influences on the research program he developed. Heritage links ethnomethodology to the theory of social action developed by Talcott Parsons (1902–1979), Garfinkel's mentor when he pursued his Ph.D. in sociology at Harvard in the late 1940s. The main philosophical influence is the phenomenology of Edmund Husserl (1859–1938), mediated by Alfred Schutz and Aron Gurwitsch, both of whom had contact with Garfinkel during his graduate studies. Schutz (1899–1959), an Austrian banker who emigrated to the U.S., where he taught at the New School for Social Research, brought phenomenology to sociology. In his theoretical writings, Schutz [1962; 1964] argued that Husserl's [1970] conception of the *Lebenswelt* (lifeworld; lived-world; lived-in-world of daily life) furnished an essential experiential orientation that was, at best, implicit in Max Weber's [1978] writings on social action and George Herbert Mead's [1932] philosophy of the subject. Schutz also corresponded with Talcott Parsons [Grathoff, 1978], in an effort to articulate how phenomenology offered a way to deepen, supplement, and critically complete Parsons' general theory of social action. Schutz also produced a series of exemplary studies, many of which took the form of first-person explications of ideal-typical standpoints and situations within the social world (the world for a fully socialized member acting in the "natural attitude", the world of a stranger coming to terms with a new culture, the world of a concerned citizen, or of a member of an audience attending a play or musical performance). Several of the themes that Schutz elucidated — the reciprocity of perspectives, the retrospective-prospective sense of temporal occurrence, the "multiple realities" of dreams, theatre and scientific theory, and the "natural attitude" of commonsense, everyday reality — were picked up and developed by ethnomethodologists and other sociologists.

Like Schutz, Garfinkel was most interested in Parsons' theory of action, and less interested in the grand theory of "the" social system that Parsons later devel-

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[5]Other general texts on ethnomethodology include [Mehan and Wood, 1975; Leiter, 1980; Benson and Hughes, 1983; Sharrock and Anderson, 1986].

oped.[6] Parsons [1937] presented his theory of action as a synthesis — or, rather, a convergence — of ideas from Weber, Durkheim, Pareto, and Marshall. His theory develops a logical formula — "the unit act" — that defines the minimal conceptual elements of social action: it is necessary that there be an agent or actor with an awareness of the situation; the action must be directed towards a goal or end; the pursuit of that end requires a selection of means. In contrast to caused or mechanical behavior — for example, behavior governed by habit and instinct, or compelled by force — action involves a choice among means. In order to be *social* action, the choice among means must be cast within a system of norms and values, and it does not invariably reduce to a utilitarian calculus of the most efficient means. For one thing, the actor does not have a complete knowledge of the situation — of the available means and the probable consequences of every choice — and for another, the actor's orientation to what is right and efficacious is framed by a gut sense of what is *normal* and *moral* within an internalized system of cultural values.

Both Garfinkel and Schutz were attracted by the minimalist rigor of Parsons' conceptual scheme. Part of the attraction was that, despite its simplicity and formality, its schematic elements resisted collapse into behaviorism, evolutionism, and other deterministic or naturalistic frameworks. In other words, while devised as an elementary unit, the unit act was elementary in a *fundamentally different way* from elementary conceptual schemes in physics, biology, psychology, or classical economics. *Action* in Parsons' formula was not, and could not be, subordinated to more "basic" biological determinants or decomposed into mechanistic elements without losing its integrity *as* action. However, Schutz and Garfinkel were not convinced by Parsons' effort to place all social action under a single logical formula, expressed as a linear means-ends schema. Moreover, rather than conceiving of the actor's "knowledge of the situation" in generalized way, Schutz and, later in a different way, Garfinkel proposed *investigations*. Schutz and Garfinkel came up with recognizable examples of *actions in situ* in which the elements (sources of agency, means, ends, relevant values, situational contingencies) were reconceived as vernacular categories, which are redefined over time in a way that is bound up with interactions with human interlocutors and materials at hand.

Although Schutz never attained the status of a first-rank classical sociological theorist, his teachings and writings were an important source for Berger and

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[6]In his dissertation, Garfinkel identified two paths from Weber's conception of action, one of which was well trodden, and the other less so:

> At least two important theoretical developments stem from the researches of Max Weber. One development, already well worked, seeks to arrive at a generalized social system by uniting a theory that treats the structuring of experience with another theory that designed to answer the question, 'What is man?' Speaking loosely, a synthesis is attempted between the facts of social structure and the facts of personality. The other development, not yet adequately exploited, seeks a generalized social system built solely from the analysis of experience structures. [Garfinkel, 1952: 1]

Luckmann's [1966] *Social Construction of Reality* as well as the burgeoning "social constructionist" movement, and for Dorothy Smith's [1988] feminist "standpoint theory". Garfinkel makes extensive and explicit use of Schutz's ideas in several chapters of *Studies in Ethnomethodology* [1967]. In a chapter of his unpublished *Parsons Primer*, Garfinkel [1960] elucidates and compares the writings of Parsons and Schutz in terms of a set of "pre-theoretical decisions" on fundamental questions about the objectivity of the social world, the possibility of sociological investigations, and the relationship between the sociologist's "scientific" knowledge and the "commonsense" knowledge of the ordinary "actor" studied. Although Parsons was no positivist, he treated action as an elementary part of an objectified social structure, and agency was for the most part subordinated to an hierarchical system of institutions, social roles, and internalized norms and values. Schutz [1964, 81], without obvious hint of irony, used the image of a "puppet", and Garfinkel [1967, 68], with unmistakable irony, spoke of the "cultural dope," when describing the "man-in-the-sociologist's-society who produces the stable features of the society by acting in compliance with preestablished and legitimate alternatives of action that the common culture provides." Both Schutz and Garfinkel proposed a different starting point for investigation. Instead of starting with a model of the overall society that impinges upon actions, they proposed to start where any human being (the social theorist not excepted) already lives: a stream of here-and-now situations, and complex sequences of action involved and engaged in relations with others in an already constituted world. The phenomenologist's chronic problem of intersubjectivity — How do I get there from here? — would not be solved by theoretical fiat. Instead, it provided a starting point, and continual point of return, for investigations.

It is a deep question as to whether a phenomenological approach to *investigations* of an empirical plurality of actions is commensurable with a formal ideal-typical *theory* of action, such as the one put forward by Parsons. Schutz and Garfinkel sometimes seemed to suppose that their investigations could be commensurable with a critically revised version of Parsons's action theory, and yet their writings on the subject provide plenty of reason to suppose that they are deeply incommensurable.

For the most part, Parsons remained indifferent to the directions that Schutz, and later Garfinkel, proposed to take the theory of social action, and his own theorizing became increasingly preoccupied with grand, impersonal schemes of social structure in which "the actor's knowledge of the situation" shrinks to a vanishing point. North American sociology also went in a direction that Parsons helped to catalyze: from the 1940s through the early '60s, the "middle-range" structural-functionalist theory of Robert Merton allied with the survey methodology of Paul Lazarsfeld became the "scientific" coin of the realm. Not every sociologist marched in lock-step. In the 1950s and early 60s several conspicuous voices rebelled against or remained indifferent to the dominant trends in sociology — C. Wright Mills, Alvin Gouldner, Herbert Blumer, Howard Becker, and Erving Goffman, among others — but their efforts tended to be compartmentalized into

the ghettos of "micro-sociology" and "qualitative methods" or ignored for being insufficiently "disinterested" to contribute to a progressive science. Garfinkel certainly was to be numbered among the rebels, and the publication of *Studies in Ethnomethodology* [1967] coincided with, and helped bring about, a turning point in the history of North American sociology. Garfinkel's ambition was clear to his contemporaries, even if his writing was not: he was not about to settle for filling in the "micro" details of the big picture provided Parsons' or any other theorist's overview of the social system, nor would he settle for producing "qualitative" research that would yield insights preliminary to full-fledged "scientific" surveys. Instead, ethnomethodology offered an alternative picture of, and approach to, the entire society, though with a radically different conception of what "entire" and "society" could possibly mean.

To understand where ethnomethodology "is coming from" (in the vernacular sense of those words), it is necessary to distinguish it from Schutz's phenomenological sociology. Thus far, I have allied both approaches in a confrontation with Parsons' theory of action, but it is also necessary to appreciate differences between them. Schutz's analyses of the life-world are imaginative exercises in which he specifies how any one of "us" lives within and relates to an organized world that includes other persons in typical relational categories and routine rounds of activity. Schutz invites his readers to recognize what he says, not by reference to independent objects that they themselves can observe, but by reference to typical experiences of "anybody" in recurrent situations of action. His analyses require readers to reflect upon what it is like to attend a theatrical performance, travel outside one's native land, or relate to others at different levels of intimacy (members of family, friends, work associates, service personnel, etc.). It is not always necessary for every reader to have done what Schutz describes in order to appreciate what he says (for example, many of us will not have had the opportunity to theorize as a scientist, to play music in an ensemble performance, to emigrate to a foreign land), but it is necessary to imagine what it is like for *someone* to act in such situations. From that starting point, Schutz reflects upon what these common experiences assume or presume; he pursues an in-depth analysis of cognitive prerequisites demanded by membership in an (unspecified) ordinary society and its (ideal-typically specified) recurrent situations.

Garfinkel's best known studies are also imaginative, but in a very different way. Where Schutz starts by reflecting upon the most typical, or typified, relations of everyday life, Garfinkel devises a series of interventions in actual scenes of conduct. Often, he required his students to perform such interventions as observational exercises: to bargain for fixed-price goods; to act as a stranger in their own family households; or to persist in seeking explanations ("What did you mean by that?") for the banal utterances of intimates. He calls his interventions "experiments", but they are not methodic tests of hypotheses; instead, they are probes, sometimes akin to practical jokes, that confound typical social categories, disrupt expected modes of conduct, and challenge business as usual. He also pays close attention to "naturally occurring" anomalies, disruptions and troubles, and uses proxies such

as "Agnes" — an avowed intersexed person who was undergoing a sex-change operation when Garfinkel conducted a series of lengthy interviews with her — to articulate rare insight into identities and relations that "normal" members take for granted. Like Schutz, Garfinkel delves into what it means to live in, and relate to others in, a social world, but his investigations (or, rather, his demonstrations) are more like guerrilla theatre than quiet meditations.

A conventional history of ethnomethodology only begins with Garfinkel's re-working of social phenomenology. As Heritage and others have chronicled, and Garfinkel has acknowledged on many occasions, ethnomethodology was taken in an original direction by his students and associates. Garfinkel sometimes speaks of "a company" that carried out the studies he proposed, and initiated original lines of research he did not anticipate. The major line of research that spun off from ethnomethodology is that of conversational (or, as it is usually named, conversation) analysis. Harvey Sacks (1935-1975), who pursued a Ph.D. at University of California at Berkeley in the 1960s, was heavily influenced by Garfinkel (who worked at UCLA) even though Erving Goffman was his thesis supervisor. Sacks and Garfinkel collaborated on a paper [Garfinkel and Sacks, 1970], and some of his early research developed upon some of Garfinkel's main preoccupations (especially, the theme of the "rational properties of indexical expressions", and "conditional relevancy"). Indexical expressions [Bar-Hillel, 1954] are a type of linguistic expression (or, rather, a broad family of expressions including deictic, anaphoric, pronominal, proverbial, performative and an indefinite range of other linguistic terms, phrases, propositions and utterances), which is characterized by indefinite reference to its "object". Often, philosophers and linguists contrast "objective" context-free expressions (such as the canonical example of "Water boils at 100° Centigrade") with expressions that require knowledge of the circumstances of their use ("Is the water hot enough?"). Terms such as "here", "it", "now", and so forth, are further examples of indexicals. As Garfinkel and Sacks (1970) elaborate, indexicals are regarded as a "nuisance" for various programs in logic, machine translation, and survey analysis. In standard logical texts (for example, [Quine, 1966]), one need not read beyond the first page or two to encounter acknowledgement of the need to translate indexical expressions into elements of a code which have strict referential and grammatical uses. Scientific terminology, such as Latin names in taxonomies, also attempt to translate common names, which are subject to regional variation and casual use, to terms that correspond to distinct categories of object. But rather than develop such a terminology, Garfinkel and Sacks propose to investigate how indexical expressions are deployed in everyday situations, and endowed with contextual relevance and sense. Although the research program they proposed included detailed examinations of situated language use, it also held critical implications for the entire field of sociology. Instead of devising a standard terminology (class, status, role, attitude, norm, motive, etc.) with which to map and measure the social world, ethnomethodologists would note (in terms reminiscent of the treatment of "motives" by Mills [1940] and Winch [1958]), that such terminologies were, with few exceptions, derived from the com-

mon language. And, rather than attempting to purify the language in an effort to upgrade common "prenotions" into professional concepts, ethnomethodologists would study how they were constitutively embedded in recurrent practices (such as in Garfinkel's original example of jury deliberations).

"Conditional relevance" is a name for what might be called a general property of collaborative production, which holds profound implications for conceptions of action. It means, literally, that an action performed by someone at a given time and place is conditioned by, and further conditions, the immediately prior and subsequent actions by others. The idea is a refinement on the theme of indexicality, and describes a concrete, readily observable, property of actions-in-sequence. It is of deep sociological interest because it identifies a general property of linguistic meaning and organization that does not reduce to structures of mind but is conditioned by concerted and conventional relations among people. Philosophical conceptions of language, action, and intentionality almost always place an individual agent at the center of production. So, for example, an utterance voiced by a speaker traces back to the intentions of that speaker; or an argument in a text traces back to the author who put it forward. Formal and informal notions of credit, responsibility, and rights rely upon such conceptions of a coherent, mentally endowed agent. However, common usage also includes references to "things" that occur between people. For example, one common meaning of the word "argument" is a disagreement that occurs *between* people. Although it is possible to decompose the elements of an argument by referring to the intentions of one, and then the other, participant, the argument between them subsumes and organizes those contributions. Such an argument has a starting point, it can escalate and "wind down", it may include a phase in which the parties exchange finely attuned reciprocal insults; its organization differs from that of a monologue in which an author puts forward a position and defends it with reasons. As Sacks and his colleagues elaborately show, there is a large domain of conversational "things" that are composed by, and which organize, sequences of utterances by more than one speaker: greetings-and-return-greetings; questions-and-answers; requests-and-acceptances/refusals-of-requests. Sacks used the term "adjacency pair" to describe a common type of conversational sequence that is organized through conditionally relevant ties between an utterance by one speaker and a response by another.

From this convergence with Garfinkel's early programme, Harvey Sacks' own research, much of it conducted in collaboration with Emanual Schegloff and Gail Jefferson, developed in a more technical direction. After Sacks' untimely death in 1975, his colleagues, students, and associates in what, by then, had become an international research program, carried on with the research. In contrast to Garfinkel's preference for "making trouble" (or finding anomalous and disturbed situations) in order to gain insight into taken-for-granted orders of action, Sacks and his colleagues used a technological form of intervention: audiotaped, and later videotaped, occasions of "naturally occurring" telephone calls, dinner conversations, service encounters, and so forth. (Garfinkel characterized such occasions as "naturally organized," as a way of contrasting them to controlled experiments

or mock deliberations; and while a microphone and camera intervenes in such a scene, the occasion is not specifically arranged or organized for research purposes.) Repeatedly replaying and transcribing the tape recordings afforded studious attention to organizational details. By deploying transcription conventions developed by Gail Jefferson, disseminating mimeos of Sacks' lectures and copies of data tapes and transcripts, and through active efforts to develop a cumulative body of findings, Sacks and his colleagues successfully developed an unusually coherent research community. At its worst, conversation analysis can seem akin to stamp collecting — producing dry taxonomies of one or another miniscule structure of talk-in-interaction documented by a collection of recordings — but at its best it delivers vivid insight into the concrete, moment-to-moment production of recurrent orders of social action.

Although Garfinkel often expressed admiration for the work in conversation analysis, he and his students pursued a different research agenda that was, in a word, more "ethnographic". Some research, such as the aforementioned work on science and mathematics, examined highly skilled occupations with limited access and stringent education requirements. Other research by Garfinkel and some of his students examined more public scenes and activities, such as the routine organization pedestrian and automobile traffic. Still other research carried on the "troublemaker" theme, only with more focused attention to embodied activities. In addition to inviting his students to conduct exercises while wearing inverting lenses, or attempting to converse while being confused by an audio-delay feedback device, in the spirit of Merleau-Ponty [1962] but with a more empirical bent, Garfinkel encouraged students to engage with disabled persons, and to pay careful attention to the way they, and others around them, navigated through social time and space [Goode, 1994].

Finally, as part of a conventional academic history, we should take stock of the present state of ethnomethodology. It is fair to say that the history of ethnomethodology demonstrates that it is not, and never was meant to be, a universalistic program that would displace the pre-existing "paradigms" in sociology with something else. From the point of view of the overall discipline of sociology, it is a minor movement, which only recently attained "section" status in the American Sociological Association (there are more than 40 such sections). However, ethnomethodology has also been highly adaptable, and it has worked its way into numerous fields in and beyond the academic social sciences. Though never a dominant approach, ethnomethodology is a presence in such fields as media and communications, pragmatics and sociolinguistics, science and technology studies, criminology and sociolegal studies, management studies, human-computer interaction, and computer-supported cooperative work. The close focus on language and embodied practices, and the emphasis on the incompleteness of formalization, makes it readily adaptable to academic and corporate research on the design and uses of "intelligent" technology. Beyond emphasizing what "intelligent" machines *cannot do*, ethnomethodologists pay close attention to what people do with them, and the situated problems that arise when "smart" machines turn out to have

stupid and annoying design features.

3   A REVOLUTIONARY BREAK

In the previous section I presented an intellectual history of ethnomethodology which connected it with other academic developments. That kind of origin story flies in the face of ethnomethodology's anti-academic and anti-literary tendency. This and the next section will address that tendency, and the sense of origins that it implies. A persistent theme in Garfinkel's writings, which is even more prominent in his lectures and tutorials, is an explicit rejection of academic genealogies. Indeed, he evinces what Stanley Fish [1989] has characterized as a rejection of the academic form of life that sustains that rejection. In Garfinkel's [2002, 121] rather grandiose formulation, ethnomethodology turns away from the entire "worldwide social science movement," as though its practitioners were taking to the streets in order to mix with the vulgar masses in an effort to come up with fresh insight that is not weighed down by encrustations of scholarship and erudite debate. Although Garfinkel is often criticized for his hyper-academic prose style, his writing also makes thematic use of "vernacular" and even "vulgar" expressions. For example, Garfinkel's [1967, 21-24] list of "*ad hoc* considerations" that mediate the application of formal rules and codes to instances of sociological data include an interesting mix of classic rhetorical formulations (factum valet), and vernacular expressions (such as "let it pass" and "enough's enough").

Garfinkel sometimes speaks of the social sciences as "literary enterprises" or "talking sciences," in which flexible language can be used with a free hand to construct and reconstruct endless and varied accounts. In some respects, his posture toward academic (or "professional") sociology is similar to Wittgenstein's (1958) tormented ambivalence toward philosophy: although supremely ambitious in his designs for sociology, Garfinkel fashions himself as a heretic who rejects the totality of the "worldwide social science movement," and yet, as noted earlier, he asserts that the "preposterous problems" taken up in ethnomethodology "have their sources" in that very movement. And, as he sometimes acknowledges, he is campaigning against what has to be one of the most tolerant of disciplinary churches. The strains and contradictions inherent in this stance — a prominent professor of sociology employed in one of the largest and most reputable departments who denounces the very "social science movement" that employs and honors him — pervaded Garfinkel's relations with colleagues and students. This heretical posture was not lost on Garfinkel's critics, who denounced him for being a cult leader and a self-styled revolutionary whose small band of dogmatic followers thumbed their noses at professional sociology. The denunciations launched against Garfinkel and ethnomethodology went well beyond attacks on "ideas" and "positions", and took objection to matters of personal and professional conduct, such as speaking and writing in an esoteric code, circulating unpublished manuscripts among members of the "cult" which were not made available to the larger sociological "public", incivility toward well-meaning colleagues who were deemed "conventional" in their

sociological outlook, and posturing at conferences in the manner of radical-chic rock stars [Coser, 1975; Gellner, 1975].

It would be possible to tell (or re-tell) further stories of personal confrontations, irascible behaviour, and posturing in the service of a "radical" break. Such anecdotes (or, as Garfinkel himself [1996] calls them — "Garfinkel stories") may be amusing, and also indicative of a quixotic attitude that is, or once was, characteristic of ethnomethodology. However, the significance one ascribes to such anecdotes depends upon an assessment of ethnomethodology's radical break with "the worldwide social science movement." If one concludes, as many social scientists and philosophers do, that contrary to the claims and posturing there is nothing really *new* (not to speak of *true*) about ethnomethodology, then the posturing, the sharing of preprints, the esoteric jargon, and so forth, stand out as "social factors" with little intrinsic sense or meaning. Such an account explains (or explains away) a "revolutionary" counter-movement without engaging its (claimed) content. While the history of the human sciences provides plenty of warrant for skepticism — and even cynicism — toward the latest theoretical and methodological programs, all too often, critics of ethnomethodology dismiss it quickly and with an entirely superficial understanding of its research programme. So, when viewing it more charitably, what, then, is radical about it?

One way to specify what is radical is to point to the difference between a phenomenological conception of life-world (*Lebenswelt*) and a vastly more familiar conception of society (and its constituent institutions such as economy and polity). The term "local", and related terms such as "social situation", "situated action", "occasioned", often is used in connection with interactionist and so-called "micro" sociologies. Superficially understood, these terms connote a restricted time and place within a larger social context. One can imagine a satellite with its lens turned toward earth zooming in on the planet, and then on smaller and smaller regions, and finally on a very small locality at the limits of its resolution; or, one may start with a national census, and then literally "home in" on a region, state, city, and postal code, ending at a particular address; one may imagine a local market embedded in a larger economy. Or, when we think in terms of time, we can focus on a particular "point" or "interval" embedded in a larger historical span. None of these changes in scale is adequate to resolve the "locality" implied by the notion of life-world. Lived-time and lived-space are "local" in an entirely different way. A commonplace way to describe this difference is to refer to a "subjective" domain, but that term carries the historical baggage of the Cartesian legacy, with its endless debates about realism and idealism, and its preoccupations with certainty and uncertainty. The life-world — which in Schutz's view is centered around the conscious, acting individual — is not "subjective" in a mentalistic sense of "society in the mind," nor is it unstructured, radically variable from one individual to another, or immaterial. Indeed, the burden Schutz assumes in his analyses of the life-world is to explicate the structure of a social world, centered around an agent, and spreading outward in a series of concentric circles encompassing broader and less intimate associations and involvements. Schutz also explicates non-linear

(retrospective-prospective) temporal properties of the life-world.

As noted earlier, ethnomethodology does not simply elaborate upon Schutz's phenomenological sociology, and it explores conditionally-relevant features that originate in social interaction rather than in a person-centered structure of intentionality. However, there is a family resemblance between Schutz's first-person explications of structures of relevancy, and ethnomethodology's observations of singular examples of discursive activity. Conversation analytic investigations provide rich examples of indexical usage implicating a lived-in world that is ordered and intelligible, but not as an objective domain available at different levels of scale and resolution (see, for example, [Schegloff, 1972]). Consider, for example, an instance of a conversational story discussed by Harvey Sacks [1992, Vol. 2, Lecture 11 (Fall 1971), 483]; quoted in Lynch and Bogen [1996, 163]):

> Stories are plainly ways of packaging experiences. And most characteristically stories report an experience in which the teller figures. And furthermore, in which the teller figures—for the story anyway—as its hero. Which doesn't mean that he does something heroic, but that the story is organized around the teller's experiences.

As an illustration, Sacks gives an example of a story told shortly after the assassination of Robert F. Kennedy:

> Two ladies are talking on the phone and one of them, talking about the helicopter that carried Bobby Kennedy's body back to wherever they took it, says, "You know where the helicopter took off? That was the exact spot where our plane took off when we went to Hawaii." To which the other responds, "Oh for heaven sakes, weren't you lucky. If it had happened when you were going to take off, it would have ruined your trip."

Sacks then observes that the historical event — the "objective reality" in which the speaker features as, to say the least, an incidental character – is turned into an event in her life, structured by her place in the world, her plans and her projects. To say that such connections are matters of "subjective meaning" is a distraction from considering the overtly expressed, materially evident organization of the story.

The notion of radical break sits uneasily — though not necessarily incompatibly — with efforts to connect ethnomethodology to the "classical" sociological tradition. Although Garfinkel often contrasts ethnomethodology with "the classics" of sociology and the mix of Cartesian and neo-Kantian philosophy they articulate, he also endorses a scholarly effort to exposit the "classic roots" of ethnomethodology (Rawls, 1996; Hilbert, 1991). Superficially understood, this is an effort to show that Garfinkel's "reading" (largely through exemplification) of Durkheim on social facts and Weber on social action resurrects a theoretical program that had been misconstrued and misdirected in Parsonian and post-Parsonian sociology. This move to restore original textual meanings that had been lost, forgotten, or corrupted through an intellectual devolution is a familiar scholastic and theological

trope, and it also may seem to fly in the face of the distinctive way Garfinkel [1967] recasts Mannheim's [1952, 53-63] conception of "documentary method of interpretation." Mannheim spoke of a method in historical scholarship through which underlying historical patterns are derived from documentary fragments, whereas, in characteristic fashion, Garfinkel treated the documentary method of interpretation as a ubiquitous sense-making practice in ordinary interaction as well as professional scholarship. Crucially, Garfinkel did not endorse the adequacy of this method, other than by acknowledging its constitutive importance as a method through which people individually and collectively "assemble" a sense of a common background for immediate events. Indeed, his most famous demonstrations of this method exposed its power in the face of what otherwise should seem to be contradictory and random information. One such demonstration was an "experiment" in which hapless undergraduates were set up for a phony counseling session. The subject was told that a counselor, who was not visibly present, would answer a series of yes-no questions spoken into a microphone. Unbeknownst to the subjects, the "counselor's" responses were generated at random, and the following sorts of exchanges resulted:

> SUBJECT: [After elaborating that his Jewish parents were conflicted about whether he should continue dating a gentile girlfriend.] Do you feel that I should continue dating this girl?

> EXPERIMENTER: My answer is no.

> SUBJECT: No. Well, that is kind of interesting. I kinda feel that there is really no great animosity between Dad and I but, well, perhaps he feels that greater dislike will grow out of this. I suppose or maybe it is easer for an outsider to see certain things that I am blind to at this moment.

> I would like to ask my second question now.

> EXPERIMENTER: Okay.

> SUBJECT: Do you feel that I should have a further discussion with Dad about this situation or not? Should I have further discussion with Dad over this subject about dating the Gentile girl?

> EXPERIMENTER: My answer is yes.

> SUBJECT: Well I feel that is reasonable but I really don't know what to say to him. ... [Garfinkel, 1967, 80-8])

This experiment anticipated some of the confusions arising in early artificial intelligence — particularly Joseph Weisenbaum's Eliza program which simulated a psychoanalytic counselor's responses to questions. A surprising result of Weisenbaum's Eliza was not that subjects mistook the programmed answers for a "real" counselor's advice, but that some of them became engrossed in the game, and saw the advantage of relating their troubles to a non-human interlocutor [Weizenbaum, 1976]; also see [Suchman, 1987]. What interested Garfinkel was that the

subjects managed to navigate through a series of responses ("documents") by specifying conditions from their own biographies that made the responses sensible and reasonable. In short, the exercise exposed the resourcefulness of the subjects in their ability to rationalize any answer under the presumptions in force in the "counseling" session. The point of the lesson was not to expose that the subjects' rationalizations were untrue (their truth and untruth were indissociable), but to invite attention to subjects' remarkable ability to assimilate the "documentary" information into a developing biographical situation in the real world.

The implications for scholarship were not lost on Garfinkel: the documentary method is, to say the least, a *flexible* mode of interpretation. With enough will and wit, a scholar should be able to document any number of underlying patterns, whether those patterns are inscribed in the historical past, an individual psyche, or the structure of a contemporary society. The questions Garfinkel raised about it were descriptive rather than normative: He invited investigations of how the documentary method worked in specific circumstances, and remained indifferent to its adequacy or reliability as a social scientific or historical method. Consequently, when confronted with efforts to reconstruct the classical roots of ethnomethodology by affiliating Garfinkel's writings with Durkheim's or Weber's texts, one may be inclined to conclude, first, that the exercise is a facile matter of selecting, juxtaposing, and interpreting "documents", and, second, that it misses the point of Garfinkel's research program. However, the fact that Garfinkel himself endorses such scholarly efforts, may lead his readers to react like the subjects in his experiment: "Well I am actually surprised at the answer. ... Well once again I am surprised. ... Well, this has me stymied. ... Well, I kinda tend to agree with this answer" [Garfinkel, 1967, 81-83]. As Garfinkel elaborates, the students were not stymied for long, as they went on to find *some* sense and narrative coherence in the strings of answers they were given: "In the case of contradictory answers much effort was devoted to reviewing the possible intent of the answer so as to rid the answer of contradiction and meaninglessness, and to rid the answerer of untrustworthiness" [91].

In that spirit, it is possible to dissolve an initial sense of contradiction between Garfinkel's radical disavowal of the "worldwide social science movement" and its efforts to develop reliable accounts of underlying social and psychological processes, and his apparent endorsement of the idea that ethnomethodology's program traces back to Durkheim's "aphorism" that "The objective reality of social facts is sociology's fundamental principle" [Garfinkel, 2002, 65]. Garfinkel goes on to say (ibid) that "Durkheim had it beautifully and originally right," though he objects to the word "principle", preferring "phenomenon" instead. The surface contradictions should seem obvious: Garfinkel (a hero of anti-positivist sociology) endorses Durkheim's most positivistic aphorism; Garfinkel is *performing* the documentary method of interpretation when he avows that ethnomethodology is "working out Durkheim's aphorism" — hardly a radical proposal, one might suppose. However, contrary to what Garfinkel had earlier said about his naïve experimental subjects — that they "rid the answer of contradiction and meaninglessness, and ... rid

the answerer of untrustworthiness" — we must *distrust* what Garfinkel says in so many words in order to dissolve our strong sense of *his* contradictions. He is not "working out" Durkheim's aphorism in the manner of a normal science; he is standing it on its head (or, if one prefers the radical trope, *on its feet*).

Forging ahead with our distrustful reading — designed charitably to rid Garfinkel's program of (apparent) contradiction, and mindful that our very effort must be entangled in contradictions of its own — we need to consider just how Garfinkel proposes that ethnomethodology is a matter of "working out" what Durkheim "was actually talking about" [Garfinkel, 2002, 66]. The substitution of *phenomenon* for "principle" is no minor shift: it is a reflexive maneuver that turns Durkheim's aphorism *itself* into a social phenomenon to be "studied" (cf. Garfinkel and Sacks' [1970]). Much in the way that Garfinkel turns Mannheim's methodological procedure (the documentary method) into an ordinary interpretative practice, he turns Durkheim's methodological principle *for* the science of sociology into an ethnomethodological phenomenon *in* the society. Instead of functioning as an axiom that founds a research programme, or an assumption that is subject to continual debate, Durkheim's "principle" becomes a substantive topic for research.

This transformation is the key to resolving the ambiguity (or, rather, the ambivalence) of ethnomethodology's relation to classical sociological theory, and also its relation to philosophy, including phenomenological philosophy. The recurrent themes or fundamental concepts of social theory feature prominently in ethnomethodology, and yet they are given unusual (or even "preposterous") treatment. These themes include a long, open-ended, list of key methodological, conceptual, and theoretical terms: reasoning, meaning, measurement, observation, identity, class, classification, code, category, sex/gender, and, of course, methodology. In a word, these terms are "respecified" from being topics embedded in long traditions of scholarly discussion, to being vernacular accomplishments (Garfinkel, 1991). The stance toward these topics is empirical rather than scholastic — they are investigated by reference to some-body's tasks-at-hand in a workplace, public street scene, or intimate household conversation.

A similar transformation is in store for "the objective reality of social facts." Durkheim's [1982, 60] "first and most basic rule is *to consider social facts as things*" (emphasis in the original). Contrary to the sense of an *object* for science, implied by expressions such as "the objective reality of social facts," a *thing* is ordinary, implying no privileged condition of observation or test. And, as Bruno Latour [2004] points out, following Heidegger, the etymology of the word "thing" reaches beyond the modern sense of object-out-there to a family of ancient terms for gatherings or assemblies in which parties settled their disputes. We might say that the ancients engaged fractiously in "working out" the objective reality of social facts. Taking this insight back into ethnomethodology: the events of traffic are *things* in just that sense — assemblies or congregations in which, and through which, the objective reality of a traffic jam is made real through the very way the drivers work into it, work through it, or try to work around it. Substituting

"phenomena" for "principle", and "things" for "social facts," allows us to re-read Durkheim's aphorism as: "The objective reality of things is sociology's fundamental phenomenon." This formulation may still leave us to wonder what could possibly be meant by "the objective reality of things." Understood as a positivistic formulation, it could refer to the necessity to develop a rigorous form of scientific test to verify or falsify the objective reality of candidate "things". Sociology's task would thus be to deliver such rigorous tests to upgrade or downgrade the epistemic status of the "things" of common experience. But then, if we realize that sociology's phenomenon is a subset of the society's phenomena, the objective reality of things becomes *society's* fundamental phenomenon.

4   TO THE "MAGNIFICENT METHODOLOGICAL THINGS" THEMSELVES

A distinctive account of origins arises from the focus on "things" in ethnomethodology. Garfinkel and his students sometimes insist that the social science and philosophy literature is not the most important source of empirical and conceptual insight for ethnomethodological research. Instead, they place primary insight on the real-worldly *things* investigated. An injunction to turn "to the things themselves" — whether voiced as a call for a naïve inductivist search for facts or a phenomenological investigation — implies a radical break with intellectual tradition. One attempts to *start out* with what can be found, observed, or witnessed in an engagement with "the world"; to put aside preconceived ideas, to pursue presuppositionless investigations. Instead of tracing ideas to earlier ideas in an intellectual tradition, one locates the motives and *origins* of ideas in a common world. An orientation to *things*, rather than a literary or scholastic genealogy, animates the origin story.

Philosophical and theoretical arguments about the life-world invite an engagement with the "things" that are "ready-to-hand"; things that already surround us, that we take for granted, that we handle without special thematic interest or wonder; things that we count, count with, and count on. The idea that we can turn to these things, open eyed, in an attitude of wonder and without presuppositions, smacks of the most naïve realism, and such emphasis on banality may strike those of us who have attempted to read the likes of Husserl, Heidegger, and Garfinkel as extremely odd, given the conceptual density, difficulty and depth of their writings.

We might figure that phenomenologists and ethnomethodologists should be the first to acknowledge that all perception is theory-laden (Hanson, 1958). However, further attention to this matter may lead us to question just what is meant by the "theories" with which ordinary perception is supposedly "laden" (see [Coulter and Parsons, 1991]). One of the burdens taken up by phenomenologists — for example, Merleau-Ponty in *The Phenomenology of Perception* [1962] — is to explicate a sensibility about the world that avoids the twin traps of intellectualism and naïve realism. Naïve realism is the idea that perception is (or can be made) transparent as an effective means of conveying actual features of the real world to our minds.

Intellectualism, consistent with the neo-Kantian legacy and pervading much of contemporary cognitive science, subordinates what is perceived to an internal schema (a cognitive map, an array of categories, a set of tacit presuppositions, an unconscious structure of dispositions). An important feature of intellectualism is that it draws a close analogy between scientific observation (as described in popularized philosophy of science) and "ordinary" perception. This analogy does not suppose that all perception is scientifically adequate; rather, it uses conceptions associated with modern philosophy of science to describe naïve, and sometimes ineffective, theories and methods. Canonical examples of cargo cults, mass hysteria, and self-fulfilling (or, if not fulfilled, self-perpetuating) prophecies are explained by likening them to bad experiments involving circular reasoning between "hypotheses" and "evidence", and an overwhelming confirmation bias that protects the belief or ritual from (apparently) disconfirming evidence (see [Winch, 1970]). For logical empiricist philosophers, a falsificationist strategy offers a (tentative, fallible) way out of the circle, but for proponents of theory-ladenness there is no escape; scientific observation also is incorrigibly theory laden, and is distinguished only by the degree to which the "fact" of theory-ladenness is made explicit and subjected to criticism.

However, even when one grants that it is absurd even to imagine the possibility of presuppositionless perception (Just *what* would be perceived?), what interests phenomenologists is not the "theories" or "hypotheses" that infuse ordinary perception. In canonical accounts of scientific experiments, hypotheses are formulated in advance of carefully controlled observations. They are discrete statements of the form: If I raise the temperature of this gas by x amount, the volume will expand by y amount. Although there are, of course, many contingencies involved in an actual experiment that complicate the clear acceptance or rejection of even so simple an hypothesis, the hypothesis is itself a relatively discrete statement that is possible to abandon without obliterating the sense of the world in which it is situated. It is quite different in form from the features of objects explicated in phenomenological research: that when I see a house from the front, I apprehend it as having a back, an interior, and so forth (apprehensions that can be exploited by designers of Hollywood sets); that the things around me are apprehended as having phenomenal properties such as "next to", "on" and "above", and orientated properties such as "nearby" and "upside-down". Such phenomenal properties are ubiquitous for experimental scientists as well as anyone else (persons with severe disabilities being partial exceptions), and they cannot be abandoned except in a highly circumscribed way without profoundly disordering the world as we know it. One of the unfortunate offshoots of Thomas Kuhn's conception of revolutionary science is that it can lead us to suppose that a scientific "paradigm" is a cognitive structure with such general scope that there is no getting outside of it without profoundly affecting the world as we know it. No doubt, acceptance of the revolutionary "hypothesis" that the earth is a spherical object that spins on an axis and revolves around the sun has profound consequences for everyday as well as scientific "perception" (though we can still speak of the "sun rising"

without embarrassment). However, regardless of how pervasive the Copernican revolution may be for "modern consciousness", it would be a mistake to trace all orientational properties specified in common language (for example, that something is "far away" or "within reach") to analogous "theories". Accordingly, it *can* make sense to speak of "pretheoretical" phenomenal properties, without also feeling compelled to formulate a tacit "theory" that lies behind them. Similarly, following Polanyi (1958), we can acknowledge that our perceptual and motor competencies involve many "tacit" skills that cannot adequately be specified by rules, but we would not necessarily want to follow Polanyi in saying that "unconscious rules" underpin such skills. If we suppose that *explicit* theories, hypotheses, and rules are restricted in scope, and circumscribed in their uses and usefulness, we need not suppose that these explicit structures provide the appropriate form for "inexplicit" or "unconscious" structures that supposedly lie behind them.

The straightjacket of "intellectualism" can be loosened when we recognize that there is a difference between the explicit terms associated with scientific investigation, and the inexplicit (and perhaps even inexplicable) features of common sense, ordinary language, and the everyday world. Explication need not be organized around the explicit structures (theories, hypothetico-deductive models, cognitive maps, rule-based schemas, and so forth) that initially proved inadequate for specifying their "unexplicated" or "tacit" counterparts. But, then, *how* would we conduct such investigations into the sense of ordinary things? The answer (or, rather, the dissolution of the question) is that "ordinary things" already "contain" the capacity for their explication.

In connection with Durkheim's aphorism, Garfinkel often gives examples from the commonplace (especially for residents of Los Angeles) situation of driving in traffic. He does not deliver a phenomenology of driving, but true to the orientation to *things* he begins to lay out a set of phenomenal properties that are witnessable to drivers in traffic. Many of the things of traffic are regular, nameable, observable, and available to "anybody." They are no more mysterious and no less obstinate than the rocks of the field with which common sense philosophers hope to stub the toes of their idealist opponents. If anything qualifies as a thing, the things of traffic certainly do. And yet, when looked into, these things have some peculiar properties. Traffic engineers often find it valuable to adapt models from fluid mechanics to describe and predict the regular patterns of flows, blockages, and waves in traffic. For example, Garfinkel describes a type of "accordion wave" in a traffic jam that traffic engineers seek to comprehend and manage with elegant physical models. He suggests that what such models miss is the primordial "phenomenon of a traveling wave as endogenously achieved details of structure" [Garfinkel, 2002, 164]. Not only is such a wave witnessable in a distinctive way *to* drivers, it is produced *by* drivers as they drive. And yet the atomic units that compose and "negotiate" these flows themselves give accounts of the actions through which they, together with platoons of other drivers moving together in a quasi-antagonistic "community", produce the regularities of traffic.

One reason for insisting that ethnomethodology's origins are found in the *things*

it investigates is that these things are practical accomplishments and a description of them is procedural. So, for example, conversational analytic studies describe procedures for taking turns in conversation, asking and answering questions, accepting or rejecting complements, and telling rounds of jokes. The descriptions are not normative, although they describe what is normally done in specific circumstances. Such procedural descriptions differ from studies that attempt to correlate specific discursive patterns with "social variables" such as gender, race, occupational status, educational attainment, family role, and so on. The primary interest is to come to terms with the procedures that are "endogenous" to the things in question. Because these "things" are accomplishments, and because a performance of such "things" necessarily exhibits recognizable properties for participants who take part in their production, the injunction to start with such things is less of an ontological claim than a concession to begin where we already live.

Questions of Method

Ethnomethodology is not a method analogous to techniques for designing and administering attitude surveys and analyzing statistical variance in the results; nor is it a "qualitative" method for conducting open-ended interviews, or "triangulating" different kinds of observational data. The "methodology" contained in the word refers in the first instance to the methodic practices, including the reflexive organization of such practices, that are everywhere at hand in public places, homes, and specialized workplaces. These are the organized actions and activities that sociologists study; or rather, they are the practices that compose the organizational features of the ordinary society that sociological data represent. However, it is not enough to say that "ethnomethodology" is a word for the methodic things that ethnomethodologists investigate. *How* do they investigate them? This is an especially difficult question, because it seems to demand a meta-method — a general research practice that would be adequate for investigating all other "methods". From the time of its Comtean inception, sociology has been saddled with a grandiose ambition to encompass all other activities (including all other sciences), but this is not an ambition that ethnomethodologists respect. Instead, as a practical necessity, ethnomethodologists subordinate their methods of investigation to the phenomena investigated (which are themselves methodic practices). They make use of "tools" such as tape recorders, transcription conventions, and analytic vocabularies drawn from the literature in the field, but there are few if any reliable recipes or general sets of instructions. This absence of a formal methodological literature[7] follows from the emphasis in the field on the insufficiency of formal accounts, and from the correlative orientation to "local" and "endogenous" practices that must be mastered *in situ*.

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[7] Textual accounts of how to do conversation analysis have been published, but to my knowledge there is no formal methodological text on how to conduct other modes of ethnomethodology.

5 THE UNIQUE ADEQUACY REQUIREMENT OF METHODS

If there is anything like a single, overarching principle of method for ethnomethod-ological research, it is the "unique adequacy requirement of methods". Garfinkel [2002, 175-6] speaks of this requirement as a "policy" (as opposed to a principle), perhaps as a way to avoid the connotation that it is a transcendental "necessity" rather than a temporal formulation with an intelligible role in a practice. Although he does not tie his formulation to Schutz's [1964, 85] postulate of adequacy, it is difficult to ignore the resemblance. Again, however, there is less of a sense of a logical condition of understanding (of being able to say with some confidence what "the other" is saying and doing), and more of a circumscribed act with which future actions must come to terms, but without being determined by it.

Garfinkel distinguishes a "weak" from a "strong" use of the requirement. The weak use, to put it simply, requires that analysts be, or become, competent at performing the practices they set out to study. Only then can they "recognize, identify, follow, display, describe, etc., phenomena of order* in local productions of coherent detail" [Garfinkel, 2002, 175]. The asterisk after "phenomena of order" is Garfinkel's way of denoting what he has called "tendentious" usage — expressions whose full "meaning" is suspended until later in the argument (and the point at which "later" arrives may itself remain unspecified). This "weak" requirement should be familiar to anthropologists and sociologists who conduct studies in which it is necessary to master a foreign language or specialized vocabulary, live for an extended time among the members of a group studied, or participate in specialized communities of practice. Garfinkel's requirement may encourage a greater degree of "going native" than most ethnographers would abide by, but the rationale should be familiar enough. Much of the research in ethnomethodology (especially when conversation analysis is included in the field) is about manifestly ordinary practices that the researcher can do as a matter of course. And so, the unique adequacy requirement would seem to come with the territory of being able to perform, and recognize, a competent greeting or request in a conversation. However, given the fact that "conversation" is a highly differentiated phenomenon, and that "conversations" among co-workers or family members can be confusing and even unintelligible to overhearers who do not share a locally relevant background, the requirement is not necessarily out of play in studies of "ordinary" activities.

Garfinkel's [2002, 176] account of the "strong" use of the requirement is, to put it mildly, less transparent. He begins by stating that it is "identical with the following corpus-specific finding of EM [ethnomethodological] studies":

> Available to EM research, the finding is used and administered locally as an instruction: *Just in any actual case* a phenomenon of order* already possesses whatever as methods methods could be of [observing], of [recognizing], of [counting], of [collecting], of [topicalizing], of [describing] it, etc., in and as of the *in vivo* lived local production and natural accountability of the phenomenon, if [observing], [recognizing], [counting], [collecting], [topicalizing], or [describing] it is at issue.

To begin to unpack this dense passage, we can note that the lists of bracketed terms (the brackets are a way of denoting a to-be-determined site-specific, as opposed to generic, sense of the terms) are names for actions that are featured in scientific, scholarly, administrative, clerical and many other professional and non-professional investigations. Moreover, there is every likelihood that a study of a "phenomenon of order" (a setting, or constitutive practice, which can be held responsible for instituting order, in however restricted a way) will involve some subset of the list: observing, counting, recognizing, and so forth. The point is not that the practices studied will *interfere with* the practices of studying them; instead, it is that "observing" computer programmers designing software, or "recognizing" that a question delivers an insult to its interlocutor requires the investigator to be privy to the competent performances being "observed." Practices of "observation" (seeing, recognizing, making intelligible, reacting appropriately) already are on the scene. There is no avoiding them if one aims to render an account of the actions "observed".

Both the "weak" and "strong" versions of the unique adequacy requirement beg many questions, and, to my knowledge, Garfinkel has neither addressed nor answered them. Why *unique* adequacy, given the possibility of multiple accounts of the "same" setting? How is criticism possible, when the distance between analyst and member so completely collapses? How much competence is necessary for attaining unique adequacy? What would be the *point* of a uniquely adequate description? Given the fact that experts in the same field often disagree among themselves, and that in their more heated disagreements experts often question the competence of their antagonists [Collins, 1985], "unique" adequacy may seem chimerical. Moreover, if treated as a political – and not just methodological — policy, the "unique adequacy requirement" would seem to protect elites and experts from popular criticism, because they can (as they frequently do) invoke their specialized training and access to non-public (sometimes classified) "intelligence" to restrict access to "reasonable" and "well-informed" discussion.

Recalling Garfinkel's substitution of "phenomenon" for Durkheim's "principle" may help relax the apparent bind that unique adequacy seems to lead us into. Let us suppose that the unique adequacy requirement is less of a methodological principle for ethnomethodologists and more of a constitutive phenomenon to be researched. Considering it in this way does not get rid of the difficulties associated with gaining intelligible access to the phenomena studied, but it enables us to see that there can be no standard answer to the questions of how much is enough, whether criticism is possible, and so forth. Tutorials on such matters would be found in the settings observed — and that "setting" would not necessarily be characterized by common "understandings", political harmony, or consistent membership criteria. Many situated problems would remain, but no single methodological principle would serve very well to articulate what those problems are and how they should be solved.

6   CONCLUSION: ETHNOMETHODOLOGICAL AMBIVALENCE

Many explanations can be given, and have been given, for why ethnomethodology originated when it did, and why it has had such a checkered career over the several decades of its existence. Not surprisingly, such explanations tend to be tendentious, as they follow from the particular theoretical and professional orientations of those who give them. Dismissive "explanations" tend to follow the form of "sociology of error" — invoking social and cultural causes of a social movement that otherwise would have little or no intellectual coherence, originality, or practical value. The four origin stories I have recounted are not dismissals — each in its own way credits ethnomethodology with genuine connections to a history of philosophy, long-standing problems, and/or worldly phenomena. Nevertheless, for ethnomethodologists, any origin story can be a source of consternation.

A recurrent theme in ethnomethodology is ambivalence about origin stories.[8] At times this takes the form of an outright refusal to comply with the critical demand for specifications of philosophical influences and antecedents (see [Lynch, 1999]). Such refusal may seem paradoxical in light of the fact that ethnomethodology's connections to philosophical literature and philosophical issues are more transparent than one finds in many other social science neighborhoods. It is, of course, possible — indeed, it is done all the time, and I have done it here — to ignore ethnomethodologists' own reticence about origins and simply give an account of the connections with the writings of Schutz and other phenomenologists, as well as Wittgenstein's later philosophy of language. However, for reasons that I hope are clear by now, any effort to explain — or, better, to understand — the sources of the reticence may help us to recognize distinctive aspects of ethnomethodology's research programme that would be glossed by a specification of the philosophical and theoretical influences on Garfinkel and other founding figures.

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[8]Robert K. Merton [1976] raised ambivalence to a central theme in the scientific vocation, supplementing his earlier work on the norms of science with a more subtle account of how scientists "oscillate" between contrary sets of norms that can be used to guide or justify a decision to, for example, share research results or maintain confidentiality about them until a later time. Here, when I speak of ethnomethodological ambivalence about philosophical origins, I refer to a studious reticence about philosophical or theoretical matters, and the relevant literatures that address those matters. This reticence takes the form of an indifference to scholarly sources, or even a deliberate effort to "misread" them, while at the same time giving off strong hints that one knows those sources all too well.

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PHILOSOPHY OF ARCHAEOLOGY;
PHILOSOPHY IN ARCHAEOLOGY

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1 DOMAIN DEFINITION AND OVERVIEW

Archaeology crosscuts a number of fields. In some contexts it is treated as an autonomous discipline and is housed in free-standing archaeology departments and institutes, but more often it is taught and practiced as a component of anthropology, art history, or classics. The intellectual traditions characteristic of archaeology in these disciplinary contexts differ substantially from one another. I focus on anthropological archaeology; philosophical debate has been especially active in this context, animated by questions about the scientific standing of the field and anxieties about the status of archaeological evidence. The humanistic traditions of literary and aesthetic interpretation typical of art historical and classical archaeology raise rather different philosophical issues that lie outside the scope of this chapter.

I begin with an overview of the interchange between philosophers and archaeologists — first, the analyses philosophers have developed of archaeology and then, philosophical debates within archaeology — culminating in the formation of a philosophical interfield sometimes referred to as metaarchaeology. I then consider six focal themes in the philosophical debates that have taken shape in and about anthropological archaeology: explanation; evidential reasoning; ideals of objectivity (including relativist challenges and arguments for epistemic pluralism); foundational and ontological questions (social theory, concepts of culture); normative issues (ethics and socio-politics of archaeology); and metaphilosophical questions about the role of philosophical analysis in, and its value to, a field like archaeology.

1.1 Philosophical Engagement with Archaeology

Periodically archaeology has attracted the attention of philosophers. Archaeology or, more specifically, archaeological excavation and stratigraphy figures as a metaphor for philosophical analysis in a range of contexts and there are scattered references in philosophy of science to archaeology as an example of epistemically interesting research practice. For example, Hempel considers the tacit dependence of archaeological inference on laws (e.g., in dating archaeological materials) at the

end of "The Function of General Laws in History" [Hempel, 1942, 48], and philosophers of the life and earth sciences, especially those concerned with evolutionary theorizing, consider the structure and limitations of historical inference based on the archaeological record: Toulmin and Goodfield discuss the formation of contemporary horizons of geological and historical time as a jointly epistemic and ontological problem in *The Discovery of Time* [Toulmin and Goodfield, 1965], and Tucker offers a comparative analysis of biological and historical patterns of inference in *Our Knowledge of the Past* [Tucker, 2004]. Examples of earlier, more systematic philosophical analysis of archaeology include, in the 19th century, Whewell's discussion of comparative archaeology as an example of the "palaetiological sciences," the sciences which deal with objects that are descended from "a more ancient condition, from which the present is derived by intelligible causes" [Whewell, 1847, 637]. And in the interwar period, Collingwood relied heavily on examples of archaeological inference to develop his analysis of historical reasoning — the "logic of question and answer" — in *The Idea of History* [Collingwood, 1946, Epilogomena]. In *An Autobiography* [1939] he makes explicit a number of philosophical lessons he had learned in the course of pursuing, alongside his philosophical interests, a career in the archaeology of Roman Britain. These anticipate a complex of issues that have come to dominate recent philosophical debate in and about archaeology:

Long practice in excavation had taught me that one condition — indeed the most important condition — of success was that the person responsible for any piece of digging, however small and however large, should know exactly what he wants to find out, and then decide what kind of digging will show it to him. This was the central principle of my 'logic of question and answer' as applied to archaeology [Collingwood, 1939, 121-122].

Although archaeology remains very much a minority interest among philosophers concerned with the social sciences, since the 1970s "analytic philosophy of archaeology" [Salmon, 1993, 324] or, more broadly, "metaarchaeology" [Embree, 1992], has emerged as a thriving interfield, spurred by the provocations of the self-consciously positivist New Archaeologists to which I turn shortly.

1.2 *Archaeological Engagement with Philosophy*

Archaeologists have actively engaged philosophical issues and drawn on philosophical analysis of research practice from the time the discipline became established as a university and museum based enterprise in the early 20th century. Early advocates of disciplinary archaeology promoted a scientific approach that closely parallels Chamberlin's influential "method of multiple hypotheses," a practice characterized by comparative testing aimed at the systematic empirical evaluation of competing hypotheses [Chamberlin, 1890]. Key exponents of "saner, more truly scientific methods" in archaeology insisted on the importance of pursuing "definite questions" in preference to the "woefully haphazard and uncoordinated" practices associated with antiquarianism [Dixon, 1913, 563, 565]. Dixon invoked Chamberlin directly and, with Wissler, the advocate of a "real, new archaeology,"

urged a shift of emphasis from the collection of "curious and expensive objects once used by man" to sharply focused anthropological, historical questions "proper to the science of man" [Wissler, 1917, 100].[1]

Critics in the 1930s and 1940s who deplored the "narrow empiricist" tendencies of an archaeology intent on establishing its reputation as a rigorous field science drew on Whitehead and, later, on Teggart and Mandelbaum to make the case for a more expansive, theoretically informed archaeology [Kluckhohn, 1939; Kluckhohn, 1940; Taylor, 1948]. Kluckhohn published one of these discussions in *Philosophy of Science* [Kluckhohn, 1939]. Dewey was an important influence for at least one of those who insisted, in the 1950s, that archaeologists cannot avoid a degree of subjectivism in their research [Thompson, 1956]. And those who reacted against this subjectivism in the 1950s and 1960s were influenced by the "liberal positivism" they associated with Bergman, Kemeny, and Feigl [Spaulding, 1962, 507], later drawing on Hempel, Brodbeck, and Kaplan to delineate the explanatory goals of a scientific archaeology that directly anticipated the arguments of the New Archaeology [Spaulding, 1968, 34]. In this spirit Meggars relied on Reichenbach to develop an argument for modeling archaeology on the theoretically informed practice that she understood to characterize the most successful of the natural sciences [Meggars, 1955]. British archaeologists who shared these commitments to a more ambitious and systematically scientific form of practice likewise drew inspiration from Braithwaite [Clarke, 1968; Renfrew, 1989a].

The dynamic of internal debate in which these philosophical sources figure has long been structured by a central problematic, an *interpretive dilemma* [Wylie, 2002, 117-126], which arises from deep seated epistemic anxiety that the archaeological record is too fragmentary and ephemeral to sustain an anthropological program of research in archaeology. The claims about the cultural past that interest archaeologists *qua* anthropologists inevitably extend beyond what can be securely established on the basis of the surviving material record with which they work. The worry is that, under these conditions, archaeologists must choose between, or have typically migrated toward, two unsatisfactory options. On one hand, a commitment to ideals of epistemic responsibility counsels epistemic caution, often interpreted as requiring that archaeologists restrict themselves to narrowly descriptive goals: the "narrow empiricist" horn of the dilemma. On the other hand, those who are loath to forsake anthropological, historical goals feel compelled to embrace the speculative horn of the dilemma; the alternative to empirical description is to elaborate archaeological narratives that are understood to be a form of interpretive fiction in which contemporary expectations and preoccupations are projected onto the past. Those who treat these options as mutually exclusive and exhaustive — as genuinely dilemmic — typically set the standards of epistemic credibility high and invoke a further premise: that the connections between the surviving material traces that make up an archaeological record and the antecedent events or conditions that produced them are all extremely, and equally tenuous.

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[1]For discussion of the specifics of these early arguments for a self-consciously scientific, anthropological archaeology see Wylie [2002, 25-41].

The *locus classicus* for such an argument is a widely cited discussion note published in the British *Archaeological Newsletter* in 1955 by M. A. Smith, a field archaeologist who was influenced by skeptical themes in British empiricism. She insists that there is "no logical relation" between the social, cultural past and its surviving record, by which she seems to mean no relation of deductive entailment; archaeological interpretation inevitably incorporates "an element of conjecture which cannot be tested" [Smith, 1955, 4-5]. Consequently the Diogenes problem, as she describes it, is inescapable: archaeologists "may find the tub but altogether miss Diogenes" [1955, 1-2], and may have no way of knowing what they have missed. What begins as a problem of contingent underdetermination is thus generalized; the potential for pervasive, undetectable error is inferred from specific instances of error fortuitously detected or counterfactually projected. A domain-wide, if not wholesale, skepticism is thus inescapable; the only alternative to irresponsible speculation is an archaeology characterized by severely curtailed ambitions.

This interpretive dilemma has generated a series of crisis debates that have erupted roughly every twenty-five years since the early 20th century. In this context three strategies of response have been articulated by which, it is hoped, the interpretive dilemma may be moderated or circumvented [Wylie, 2002, 28-41].

Sequent stage approaches. Advocated by optimistic conservatives, these are characterized by an insistence that epistemically responsible archaeologists should make the pursuit of descriptive goals their first priority and must eschew (premature) theoretical speculation. The demands for a more ambitious, problem-oriented archaeology advanced by Dixon and Wissler in the WWI period provoked an early and especially strident defense of this data-first approach [Laufer, 1913, 577]. Later, more nuanced arguments for deferring anthropological and historical questions reflect a conviction that, when a sufficiently rich body of archaeological data had been collected, "broader truths" could be expected to emerge [Wedel, 1945, 386]. In the first instance, temporal, spatial, and formal regularities inherent in the record would become evident, providing the basis for comprehensive typological schemes; these should, in turn, yield insights about the identity of cultural groups represented in the record, and the dynamics of their diffusion, interaction, and transformation over time. Data recovery, description, and systematization is not an end in itself but it is a necessary preliminary, a matter of establishing a secure empirical foundation before venturing historical or anthropological hypotheses. As one sardonic pair of commentators put it, theorizing is thus deferred until a "future Darwin of Anthropology" appears on the scene who can "interpret the great historical scheme that will have been erected" [Steward and Setzler, 1938, 3].

Constructivism. From the outset, critics of sequent stage approaches objected that no theoretically innocent investigation of the record is possible, either as a necessary preliminary or as an end in itself. The identification of material as archaeological, much less the construction of chronological sequences and regional or cultural typological schemes, requires substantial interpretive inference beyond de-

scription of the material contents of the record (e.g., [Steward and Setzler, 1938]). And even if interpretively neutral documentation of the archaeological record was possible, it could not be counted on to yield the kinds of evidence that archaeologists would require when they turn to anthropological and historical questions about the cultural past [Kluckhohn, 1939]. Although Kluckhohn, and Steward and Setzler, were optimistic that a theoretically informed archaeology could successfully engage anthropological and historical questions (they ultimately advocated a form of the integrative option described below), epistemic pessimists have, at various junctures, embraced the skeptical horn of the interpretive dilemma. Given constructivist arguments for recognizing that archaeological data are inevitably theory laden (to use more recent, philosophical language), they conclude that archaeological description and interpretation incorporates an irreducible element of subjectivity [Thompson, 1956], or embrace a methodological pragmatism [Brew, 1946; Ford, 1954b]. More recent critics of the positivist New Archaeology have advocated a politically informed pluralism or relativism [Hodder, 1983; Shanks and Tilley, 1987].

Integrationist approaches. At every juncture at which the merits of sequent stage and constructivist research programs have been debated, internal critics have argued that, rather than accept either of these options, archaeologists should reject the terms of the interpretive dilemma. Typically such critics accept the arguments developed by constructivists; their point of departure is recognition that archaeological evidence is pervasively theory-laden and that interpretive extensions beyond the empirical cannot be deferred to later stages of inquiry. But they consider subjectivist and relativist conclusions to be a *reductio* of any argument from these premises that enforces the turn to speculation. While virtually all (interesting) archaeological claims about the cultural past overreach what can be established with empirical security, it does not follow, they insist, that (all) such claims reduce to arbitrary speculation.

Two insights inform this refusal of the interpretive dilemma.[2] The first is an appreciation that, although archaeological data stand as evidence only under interpretation and can rarely be expected to secure unique and incontrovertible conclusions, the archaeological record does routinely demonstrate a capacity to subvert even our most strongly held expectations about the past. Archaeological evidence may be enigmatic but it is by no means entirely plastic; it may be laden by theory but not pervasively and typically not by the theories that underpin the reconstructive and explanatory hypotheses it is used to support or evaluate. The second insight, by extension, is an appreciation that archaeologists can, and often do, very effectively deploy the recalcitrance of the empirical record, systematically designing archaeological research so as to elicit empirical constraints that sometimes tell quite powerfully and precisely for or against specific interpretive hypotheses. The advocates of a "real, new archaeology" in the first two decades of the 20th century [Dixon, 1913; Wissler, 1917] sketched the outlines of such an approach, and their successors in the 1930s and 1940s elaborated its critical mo-

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[2]The details of this analysis are presented in Wylie [2002, 37-39].

tivation [Kluckhohn, 1940; Krieger, 1944; Taylor, 1948]. Broadly characterized, what they call for is a problem-oriented approach to inquiry in which all stages of the research process — data collecting and analysis, reconstructive interpretation and explanatory theorizing — are integrated around sharply defined problems or, on some formulations, hypotheses that can be empirically tested against the archaeological record.

The New Archaeology of the 1960s and 1970s (also identified as "processual archaeology") was an intervention into this long-running debate that took the form of a particularly uncompromising rejection of the interpretive dilemma. Although represented as an entirely new departure — the New Archaeologists called for a revolution in which properly anthropological and scientific archaeology would finally displace "traditional" forms of practice — in fact this latest new archaeology shared much with earlier attempts to articulate and implement what I have described as integrationist approaches [Wylie, 2002, 41, 57-62]. Like previous advocates of problem oriented, theoretically sophisticated forms of practice, the New Archaeologists were motivated by growing frustration that, although archaeologists had accumulated enormous stores of archaeological data and elaborated finely detailed classification schemes ("space-time systematics"), their labors were yielding little in the way of anthropological insight. At the same time they felt certain that, if they made a concerted effort to put these data to work — to use them as evidence of the cultural past — they should be able do more than offer just-so stories. In fleshing out these latter possibilities, the hallmark of the New Archaeology was its programmatic commitment to the central tenets of logical positivism: they were confident that, if archaeologists implemented a rigorously scientific research program modeled on Hempelian ideals, they would escape the horns of the interpretivist dilemma. The main planks in this programmatic platform were as follows.[3]

1. The central goal of archaeology, as a subfield of anthropology, should be to establish an explanatory understanding of long-term, large-scale cultural process (hence the name, "processual" archaeology). This understanding of cultural process was to be nomothetic; the goal was to grasp the laws that govern the structure and the dynamics of cultural systems, invariant regularities that underlie the complex specificities of human action and historical events [Flannery, 1967]. The reconstruction of past lifeways and historical trajectories was a means to this end, not an end in itself; the enduring laws of cultural process might be glimpsed in these particulars and they were, in turn, to be explained by subsumption under the system-level regulari-

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[3]The acknowledged architect of the New Archaeology, Lewis R. Binford, elaborated these key theses in a series of "fighting" articles, as he later referred to them, that appeared through the 1960s and early 1970s [Binford, 1962; Binford, 1972; Binford and Binford, 1968]. He has since defended them vociferously against post- and anti-processual challenges [Binford, 1989]. Binford invoked Hempel and logical positivism in several contexts but it was a younger generation of archaeologists influenced by him who elaborated the details. In fact, Binford's commitment to positivism is partial and contradictory [Wylie, 1989b]. It serves a rhetorical function in his earliest arguments and receives little development in later work.

ties of which they were instances, in conformity with Hempel's deductive-nomological (D-N) variant of the covering-law model of explanation. Moreover, reconstruction of the details of the cultural past was understood to require law-mediated "retrodiction," as Hempel had described in connection with historical inference [Hempel, 1942].

2. The practice of archaeology was to be rigorously problem-oriented. Rather than formulating interpretive or explanatory hypotheses after the fact to account for the results of an open-ended empirical exploration of the record, prospective hypotheses should be the starting point for inquiry; all aspects of archaeological research were to be designed as a systematic test of their empirical implications. Invoking the conventional positivist distinction between the contexts of discovery and of verification, advocates of the New Archaeology insisted that the inductive, intuitive, speculative considerations that give rise to an hypothesis have no bearing on its adjudication; it should be accepted or rejected strictly on the basis of confirming or disconfirming test evidence, evaluated in the presumptively deductive framework set out by Hempel's hypothetico-deductive (H-D) model of confirmation. The New Archaeology was, then, characterized as a rigorously deductive research program in both aims and practice, by contrast to the imputed inductivism of traditional archaeology.

3. The cultural subject of inquiry was conceptualized in reductively eco-materialist and, in some cases, eco-determinist terms; for purposes of scientific investigation, cultures were to be conceived of as systems of tightly integrated components (social, ideational, material) that, together, mediate the adaptive response of human populations to their material environments [Binford, 1962]. It thus constituted a subject domain that was amenable to causal analysis capable, in principle, of sustaining the search for Hempelian laws of human behavior and cultural process through a practice of testing the (deductive) implications of explanatory hypotheses against the archaeological record.

The New Archaeology provoked intense debate within archaeology which focused, in part, on questions about the adequacy and applicability of the Hempelian models that were the inspiration for its resolute positivism. Philosophers entered the debate when these models were elaborated in a self-described primer for the New Archaeology [Watson, *et al.*, 1971], and in publications on archaeological explanation, observation, and hypothesis testing [Fritz, 1972; Fritz and Plog, 1970; Hill, 1972].[4] In some cases philosophical commentators were sympathetic, or offered friendly amendments: R. A.Watson was an early entrant to this debate who consistently defended the positivist orientation of the New Archaeology against its critics [Watson, 1972; Watson, 1990; Watson, 1991]; and M. Salmon published

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[4]For a more detailed overview of the philosophical arguments that emerge in this highly polemical literature see Wylie [1992; 2002, part 3].

several short articles clarifying key philosophical concepts and distinctions that were widely read and well received [Salmon, 1975; Salmon, 1976]. But others were sharply critical. Two reviews that especially rankled were Morgan's withering critique of Watson, Leblanc, and Redman [Morgan, 1974], and Levin's rebuttal to Fritz and Plog [Levin, 1973], in which archaeologists found themselves chastised, not just for getting the details of philosophical analysis wrong, but for misunderstanding the process and recent history of philosophical debate. Logical positivism had, famously, met its demise; Morgan and Levin, as well as several internal, archaeological critics (e.g., Tuggle, 1972), pointed out that the adequacy of Hempelian models as an account of scientific practice (in any domain) had been decisively challenged by the time they were embraced by the New Archaeologists. Moreover, this "import" exercise, as Morgan described it, was fundamentally misguided; philosophical theories of science could not be expected to provide authoritative answers to methodological questions, especially in a field as remote from the physical and natural sciences that were the focus of philosophical interest as is archaeology.

These corrective, boundary marking interventions generated considerable disaffection among archaeologists, some of whom categorically rejected philosophizing of all kinds on grounds that it was inevitably divisive and largely irrelevant to the real (empirical) work of archaeology. Such themes dominate in Plog's lament, "Is a Little Philosophy (Science?) a Dangerous Thing?" [Plog, 1982], in Renfrew's derisive review of "Isms of Our Time" [Renfrew, 1982a: 8-13], and in Flannery's parody, "The Golden Marshalltown," in which he likens the pretensions of a philosopher elite to the prognostications of self-satisfied sports commentators who have long since lost touch with the gritty realities of actual practice [Flannery, 1982]. In a review of this debate that appeared when hostilities were most marked, Schiffer, a second generation New Archaeologist, made the case that systematic philosophical analysis is indispensable to a field like archaeology.[5] But he urged philosophers to engage the epistemic problems that archaeologists confront in practice — the problems that motivated the New Archaeologists' appeal to Hempelian models — rather than disparaging their attempts to resolve these problems by appropriating philosophical models that were never intended for this purpose [Schiffer, 1981].

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[5]A similar argument had been made by Clarke in the context of debate over the implications of adopting scientific techniques and forms of practice in British archaeology. With the growth of technical sophistication, archaeologists had lost their "innocence"; rather than proceed on the basis of an unexamined framework of epistemic and theoretical commitments, many of them now obsolete, he urged archaeologists to take responsibility for the presuppositions that inform their practice and subject them to systematic critical scrutiny; what this required, he argued, was not the imposition of models developed to make sense of other disciplines but a rigorous "internal philosophy of archaeology" [Clarke, 1973]. In this spirit Fitting argued, in "Plumbing, Philosophy, and Poetry," that archaeologists would be well advised to make the systematic assessment of their presuppositions an integral part of their practice, but he roundly condemned the compulsion, on the part of professional philosophers, to enforce the "ritual purity" of philosophical doctrine [Fitting, 1973].

1.3   The Formation of an Interfield: Metaarchaeology

Despite this fractious debate, a number of archaeologists developed substantial philosophical sophistication, and a growing contingent of philosophers immersed themselves in the specifics of archaeological practice, often working collaboratively with archaeologists to develop constructive analyses that went well beyond critique and correction. The result is a thriving interfield in which archaeologically literate philosophers and philosophically minded archaeologists have explored a much expanded range of philosophical resources, often developing innovative models of explanation and evidential reasoning, ideals of objectivity, and foundational assumptions that do not conform to any established philosophical tradition of thinking about science.

One early focus of attention was a cluster of interpretive and explanatory practices typical of archaeology that had rarely been discussed in any detail in standard philosophical analyses of science; Nickles published an account of singular causal explanation that was based on archaeological examples [Nickles, 1977] and, when Levin turned from critique, he developed an analysis of the inference strategies by which archaeologists ascribe functional significance to specific types or classes of artifact [Levin, 1976]. In the first monograph to appear in this emerging subfield, *Philosophy and Archaeology* [Salmon, 1982], M. Salmon drew on a range of established philosophical models — e.g., Bayesian models of confirmation and W. Salmon's statistical-relevance account of explanation — but substantially reworked them to make sense of the forms of reconstructive inference and functional ascription discussed by Nickles and by Levin as well as a number of other distinctive features of archeological practice: e.g., system-level functional explanation and patterns of theory construction that depend on external sources. Six years later Hanen (a philosopher of science) and Kelley (an archaeologist) published a monograph, *Archaeology and the Methodology of Science*, that further explores the philosophical puzzles generated by archaeological practice [Kelley and Hannen, 1988]. Influenced by Kuhn and by Goodman, they argued that a non-realist constructivism best captures the goals and inferential practice typical of archaeology, but the specifics of the models they proposed (inference to the best explanation and weighted belief revision) derive chiefly from close analysis of a number of extended archaeological cases. The next year a third monograph appeared, *Explanation in Archaeology* [Gibbon, 1989], in which Gibbon (an archaeologist who had undertaken substantial training in philosophy of science) argued that a robust scientific realism is the most promising alternative to the positivism of the New Archaeology. Again, although he cites Harré, Bunge, and the early Putnam as important influences, the majority of his analysis is archaeology-specific; with Kelley and Hanen he argues that if philosophical analysis is to grasp the nuances of archaeological practice, it must be richly informed by an understanding of the social history, the disciplinary culture, and the institutional dynamics that shape this practice.

Two recent monographs build on this tradition of interfield analysis, both by

philosophers of science whose analyses of evidential reasoning are informed by archaeological field experience: Kosso's *Knowing the Past: Philosophical Issues of History and Archaeology* [Kosso, 2001] and my *Thinking from Things: Essays in the Philosophy of Archaeology* [Wylie, 2002]. In *Knowing the Past* Kosso elaborates a model of evidential reasoning he had outlined in several earlier articles, and illustrates how it applies to practice through sustained analysis of examples drawn from a program of archaeological field work on medieval sites in Greece. He argues that evidential claims in archaeology can fruitfully be understood as an inferentially complex form of observation, expanding on the multi-component analyses that have proven necessary to make sense of observational practice in astronomy, high energy physics, and evolutionary biology [Kosso, 1988; 1992]. In the essays assembled in *Thinking from Things*, I make a complementary argument for focusing on the role of the background or auxiliary assumptions that mediate archaeological inference (interpretive and explanatory as well as evidential), initially in consideration of analogical reasoning [Wylie, 1982a; 1985], and subsequently through comparative analysis of examples of field work undertaken by New Archaeologists and by a range of anti- and post-processual practitioners. Despite programmatic differences I find that these exemplify a common (amended) bootstrapping pattern of evidential inference [Wylie, 1986b; 1989a; 1992]. One striking departure from this growing tradition of archaeologically grounded analysis is the prescriptive case that Bell (a philosopher of science) makes for structuring archaeological practice around the tenets of an uncompromising Popperian falsificationism. In *Reconstructing Prehistory: Scientific Method in Archaeology* he proposes a check-list of questions designed to ensure that archaeological hypotheses are testable in a Popperian sense, and that they are subjected to appropriately stringent attempts to refute them [Bell, 1994].

By contrast to these analyses, which presuppose broad support for the scientific ambitions of the New Archaeology, most post-processual critics reject the New Archaeologists' fascination with scientific models of practice altogether and seek philosophical inspiration in philosophical hermeneutics [Hodder, 1982a; 1983; 1991; Johnsen and Olsen, 1992; Tilley, 1993], phenomenology [Gosden, 1994; Shanks, 1992], critical theory [Leone *et al.*, 1987], and various forms of post-structuralist analysis [Tilley, 1990]. Two continentally trained philosophers made early contributions to the philosophical literature on archaeology, although not as interventions into the debate between processual and post-processual archaeologists: Embree (a phenomenologist) undertook a survey-based study of archaeologists' perceptions of "theory" in the late 1980s [Embree, 1989], and Patrik offered an early and incisive analysis of divergent conceptions of "an archaeological record" as, on one hand, a text requiring hermeneutic interpretation and, on the other, a fossil record amenable to physical analysis [Patrik, 1985]. The contributors to a recent, predominantly European collection of essays, *Philosophy and Archaeological Practice* [Holtorf and Karlsson, 2000], expand the scope of this growing tradition of non-analytic philosophy of archaeology, drawing inspiration from philosophical sources as disparate as Wittgenstein (Bintliff), Foucault and

Derrida (Cornell), Feyerabend (Holtorff), Levinas (Hegardt), Butler and Irigaray (Tarlow), Merleau-Ponty (Staaf), and Heidegger (Thomas).

In 1992 Embree argued that this growing body of work had achieved sufficient maturity to be recognized as a subfield which he designated "metaarchaeology": a loose-knit family of research programs that make use of historical and sociological as well as philosophical modes of inquiry (both analytic and continental) to address second order questions about archaeological practice [Embree, 1992]. A year later Salmon distinguished "analytic philosophy of archaeology" from "philosophical approaches to archaeology" [Salmon, 1993, 324], and characterized the former as an established field of practice concerned with "metaphysical, epistemological, ethical, and aesthetic problems that arise in the theory and practice of archaeology" [Salmon, 1993, 323]. All these areas of metaarchaeology continue to grow apace, although conflicting demands for accountability lend particular urgency to analyses of the normative issues that comprise the burgeoning field of archaeological ethics.

2 FOCAL ISSUES AND CENTRAL THEMES

As analytic metaarchaeology has taken shape, six issues are a persistent, or now emerging, focus of philosophical attention: explanatory practice; evidential reasoning; foundational assumptions concerning the nature of the subject of inquiry (concepts of culture, social ontology); normative issues, chiefly ethics issues raised in and by archaeological practice; and overarching metaphilosophical questions about the role of philosophical analyses in a field like archaeology (its intrinsic interest; its practical relevance). I consider each schematically, with the aim of delineating key positions articulated in the past and directing attention to current and emerging debates.

2.1 Explanation

The point of departure for the philosophical debate generated by the New Archaeology was widespread reaction against the prescriptive argument that the goals of archaeology must fit the narrow template of Hempel's covering law model of explanation. In the initial round of debate described above, critics focused on the appraisal of covering-law models (in any application) and their relevance for a field like archaeology, but attention quickly turned to a range of alternative models of explanation. As analyses of explanation have proliferated, it has become clear that archaeologists explain in many different senses and at different levels. One challenge has been to understand how the contents of the archaeological record were produced and what they represent as evidence, a practice that requires reconstructive inference from the contents and configuration of the archaeological record to the specific events, conditions of life, intentional actions, and "formation processes" that produced them. As archaeologists and philosophers have wrestled with the complexities of actual practice, it has become clear that these culture-historical

reconstructions, discredited by the New Archaeologists as merely descriptive, are a form of localized explanation; the system-level processual explanations they identified as the primary concern of a scientific archaeology both depend upon and are continuous with these more modest explanations of the record and its immediate antecedents [Wylie, 2002, 86-92]. While this point seems widely accepted, the range of views about how best to understand archaeological explanation (at any scale) is enormously broad and continues to proliferate.

Systems explanation. An early rival to covering-law models was proposed by archaeological critics who insisted that the dynamics of complex cultural systems could not be understood by appeal to deterministic covering laws; they advocated a systems approach, inspired by Meehan [1968], the aim of which was to develop formal models that capture the underlying structure of interaction between the many variables that constitute particular cultural systems [Flannery, 1967; Tuggle *et al.*, 1972]. In a debate that was joined by archaeologists intent on defending the covering law model [LeBlanc, 1973] and by philosophers urging a broader, more systematically critical view of this model [Salmon, 1978a; 1989], the point was quickly made that explanation on Hempel's covering law model is not necessarily mono-causal or deterministic; on any formulation of the covering law model a number of causal laws may be invoked in a series of nested explanations to account for a complex expanandum and, on later variants of the model, these laws can be statistical and the inference pattern inductive (e.g., inductive-statistical rather than deductive-nomological variants of the covering law model). Moreover, the "systems paradigm" alternative does not escape dependence on law-like propositions if it is to support the prediction and explanation of system states and outcomes; as characterized by the archaeologists influenced by Meehan, this modeling function depends on formal "rules" that link system variables, capturing regularities of interaction and interdependence that have all the characteristics of, and no more robust causal content than, Hempelian laws [Flannery, 1967, 52]. The fundamental problem with Hempel's covering law models, Salmon argued, is that their formal, syntactical requirements incorporate no criteria of relevance for distinguishing between genuinely explanatory and spurious cases in which an explanandum is shown to be an instance that conforms to a projectable pattern. This point was taken by former advocates of covering law and systems approaches (LeBlanc and Read, respectively) who, working within a broadly empiricist, logical positivist framework, argued that lower level covering law explanations should be embedded in a theoretical framework that has the resources to distinguish between accidental and causal regularities [Read, 1978]. They conceived of this theoretical edifice as a hierarchy of increasingly abstract representations of structural patterns that underlie, and subsume, the regularities captured by lower level empirical laws; they gave no account of how theory, thus conceived, would incorporate any additional causal content beyond the statements of empirical regularity they were to subsume.

Causal modeling. Despite the New Archaeologists' official endorsement of Hempel's covering law models, causalist intuitions figure prominently in the ar-

guments they give for advocating scientific modes of explanation in archaeology. In one of the earliest and most influential of these, Binford objects that archaeologists have not, in fact, explained major culture transforming events when they cite antecedent events or conjunctions of factors that are merely correlated with the explanandum event in question. These associations may be accidental; a genuinely explanatory understanding requires an account of the causal mechanisms by which these factors or events brought about the transformation in question [Binford, 1968]. The tensions introduced by appeal to Hempelian laws at this juncture are acute and were one motivation for the early interest in alternative models of explanation (including, initially, a "systems" approach). The laws acceptable to a logical positivist — laws whose content reduces to the systematization of observables — provide no insight into the causal mechanisms or processes that underlie empirical regularities, indeed, Hempel eschewed any such "detour through the realm of unobservables" [Hempel, 1958]. Moreover, it quickly became evident that laws that fit Hempel's account figure almost not at all in archaeological practice, either as objects of inquiry or as the imported basis for explanation.

In practice, a great deal of archaeological research is concerned with building and testing models of widely varying form, scale, and content. An extensive archaeological literature on this practice includes consideration of descriptive, phenomenological modeling (systematizing classificatory schemes), simulation on various scales (ranging from local site use models to long term regional subsistence patterns), and explanatory models, both realistic and hypothetical.[6] While in many cases this practice is heuristic and instrumentalist — it is a matter of modeling conjunctions of factors or events with no concern to capture intervening mechanisms — much is causalist and realist; the aim is to understand how specific conditions of life were produced and sustained or changed. This characteristic orientation of archaeological practice is captured by a number of the models of explanation that have been proposed as alternatives to the covering law and systems approach that dominated early debate. M. Salmon developed a "causally supplemented" statistical-relevance model of explanation, building on the causalist analysis developed by W. Salmon in a series of publications in which he makes the case that explanation must be understood, not as an argument conforming to the formal requirements of one or another variant of the covering law model, but as an assemblage of factors each of which has demonstrated causal relevance to the outcome in question [Salmon, 1982: 113-139; 1978b; 1984].

A more robustly causal approach was advocated by scientific realists who argued that the emphasis of logical positivists and empiricists on "saving the phenomena" should be reconsidered. Rather than treating theoretical constructs as heuristic devices that serve the primary purpose of systematizing observables, philosophers should acknowledge that often the central aim of scientific inquiry is to build theoretical models of unobservable causal mechanisms [Harré, 1970; Harré and Secord, 1972; Psillos, 1999; Wylie, 1986a]. In many respects the ambitions of the

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[6]For an overview, see Wylie [2002, 91-96]; representative discussions include Aldenderfer [1991], Clarke [1972], Flannery [1986].

New Archaeologists seem better captured by this account than by any refinement of logical empiricist models; the potential for such analysis was noted by Mellor in two early commentaries on the arguments made by British archaeologists for a scientific methodology [Mellor, 1973; 1974] and I argued for it in critical analysis of tensions inherent in the New Archaeology [Wylie, 1982b]. But the most sustained case for a realist analysis of archaeological goals and practice was developed by Gibbon, who emphasized the central role of model building and evaluation [Gibbon, 1989, 102-133]. This is would seem to be a particularly promising area for further work given close and, thus far, unexplored parallels between the problems with which archaeologists wrestle in the internal literature on archaeological models and the issues that interest the philosophers of science who have recently turned their attention to modeling practice in other fields [Morrison and Morgan, 1999].

Unificationist models. Although no one has argued the case for understanding archaeological explanation in terms of unificationist models,[7] the intuitions central to such an account of explanation are evident in internal debate about the relative merits of specific archaeological explanations. For example, Renfrew defends his widely influential "demic-diffusion" account of the spread of proto-languages in just these terms [Renfrew, 1989b; 1992; Renfrew and Bahn, 1991]. I have argued that the unification he claims is spurious and that the explanatory power of his account depends on the credibility of underlying causalist claims that have been the primary locus of critique [Wylie, 1995].

Pragmatic, erotetic accounts. Non-realist and broadly pragmatist themes have been prominent in a number of critical responses to both covering-law and causalist models of explanation in archaeology. Morgan took the position, in debate with Watson, Leblanc and Redman, that the goal of science is not chiefly to explain, and certainly not to explain by the subsumption of instances under laws. Explanation is at best an heuristic, a means to the end of acquiring systematic empirical knowledge of the world: "finding out what the facts are" [Morgan, 1973, 260]. Kelley and Hanen subsequently argued that it is a mistake to expect archaeological explanation to conform to any single formula whether it specifies structure (syntactical, logical) or content (causal); explanations in archaeology are best seen as answers to "why-questions" that deploy whatever scientifically credible information is salient in a particular context of debate or puzzlement [Kelley and Hannen, 1988, 217-224]. They did not develop the details of an erotetic analysis of archaeological explanation as such, but analyses of anthropological explanation along lines advocated by Risjord offer rich resources for building on Kelley and Hanen's proposals [Risjord, 2000].

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[7]These were proposed by Friedman and by Kitcher who endorsed a broadly epistemic approach to understanding explanation and advocated these as successors to covering law models [Friedman, 1974; Kitcher, 1976; 1989]; for an overview of these arguments, see Kitcher and Salmon [1989], Wylie [1995, 1-3].

2.2   Evidential Reasoning

A second theme that quickly came to dominate philosophical debate in and about archaeology is concern to explicate the forms of inference by which archaeological data are interpreted as evidence and brought to bear on interpretive and explanatory claims about the cultural past. Initially, again, the discussion was structured by reaction to the deductivism advocated by the New Archaeology. In early critical assessments the point was made repeatedly, both by archaeologists and by philosophers, that the New Archaeologists were mistaken in their conviction that, if they implemented a hypothetico-deductive testing methodology, they could eliminate all reliance on ampliative, inductive forms of inference [Salmon, 1976; Smith, 1977; Wylie, 1982c]. A number of models have been proposed to more adequately capture the inductive complexity of archaeological practice. They include analyses of the abductive and, specifically, analogical reasoning by which archaeological data are interpreted as evidence of the cultural past; the ascription of functions to archaeological sites, features, and artifacts; and, increasingly, the role played by the background and collateral knowledge that mediates these inferences: auxiliary hypotheses, to use Hempelian terminology; "middle range theory," as archaeologists now refer to it [Raab and Goodyear, 1984]; and what Kosso describes as "gap crossers" [Kosso, 1991].

Abductive and analogical reasoning. In an early constructive proposal for reframing the debate about the viability of deductivst ideals, Smith (an archaeologist) argued for a more realistic "hypothetico-analogical" model of evidential reasoning in archaeology [Smith, 1977]. Rather than insist on an unattainable ideal of deductive certainty in testing, he argued, it would be preferable to acknowledge that virtually all uses of archaeological data to test explanatory hypotheses rely on analogical interpretation of these data as evidence. In philosophical arguments that focused specifically on the structure of analogical reasoning, Salmon and I argued that, despite their disclaimers, the New Archaeologists routinely rely on analogical inference. Moreover, their actual practice makes it clear that analogical inference can be closely controlled. It is a mistake to equate analogical reasoning with arbitrary and wholesale projection of the details of ethnohistorically documented cultural practices onto past forms of life that might bear little resemblance to anything familiar from the present or recent past; if these are examples of analogy reasoning at all (as opposed to arguments from a claim of identity), they are weak or fallacious uses of analogy in which no systematic assessment has been made of the relative weight and significance of the analogy on which the argument is based [Salmon, 1975; 1982, 57-81; Wylie, 1982a; 1985]. Shelley has since developed a sophisticated account of abductive reasoning in archaeology that considers the role of visual mental imagery in generating hypotheses about the cultural significance of archaeological material [Shelley, 1996], and a number of archaeologists have published closely specified accounts of how various forms of analogical inference can be controlled [Lightfoot, 1995; Stahl, 1993].

Bayesianism. Salmon proposed a modified Bayesian account as a framework

for understanding the nuanced judgments archaeologists make about the significance of archaeological evidence in order to address the most pressing limitations of the hypothetico-deductive model of confirmation: their lack of criteria of relevance. The chief advantage of a Bayesian scheme is that it makes explicit the considerations that inform nuanced assessments of the import of evidence, specifically, assessments of the degree to which new evidence changes the probability of a hypothesis given the array of existing evidence bearing on it (a measure of prior probability), and of the extent to which particular elements of the evidential base provide a discerning test of the hypothesis (a measure of the likelihood that a given test would generate the evidence in question whether the hypothesis were true or not) [Salmon, 1982, 49-56]. Bayesian models have since enjoyed something of a vogue in archaeological contexts [Buck *et al.*, 1996], although there has been little engagement with the extensive philosophical literature on the viability of Bayesian models of hypothesis evaluation [Earman, 1992; Wylie, 1988].

Inference to the best explanation. Early in the debates generated by the New Archaeology Hanen and Kelley proposed an informal, pragmatic approach to understanding hypothesis evaluation; they found inference to the best explanation models attractive because these emphasize the comparative nature of evidential reasoning and open space for considering a range of non-cognitive factors that inform judgments about the import of diverse, often contradictory, lines of evidence. Hanen and Kelley characterize archaeological reasoning from evidence as an eliminative process, although not in the strict sense advocated by doctrinaire Popperians; the goal is to provide an assessment of the relative merits, specifically, the empirical adequacy, of alternative working hypotheses, not to establish grounds for accepting an hypothesis as true [Hanen and Kelley, 1989; Kelley and Hanen, 1988, 216-219]. In an argument influenced by Goodmanian constructivism and Quinean holism — specifically, Quine's metaphor of the web of belief [Quine and Ullian, 1970] — they make a case for recognizing that, in addition to conventional requirements of empirical adequacy, explanatory power, and internal coherence, the degree to which an hypothesis is consistent with a "Core System" — a set of beliefs and assumptions on which there is broad consensus among practitioners — plays a crucial role in its evaluation [Kelley and Hannen, 1988, 111-120]. Gibbon holds a similar position but, as a realist, he argues that "best explanations" are those which afford the most comprehensive and plausible causal explanation of the available data [Gibbon, 1989, 83, 88-91]. The central features of this comparative, eliminationist approach have deep roots in archaeological practice and methodological reflection; they are anticipated by the early 20th century advocates of a scientific archaeology who appealed to Chamberlin's "method of multiple working hypotheses" (described in the first section of this chapter) and, as Kelley and Hanen demonstrate through a series of case studies, they are prominent in a great many influential examples of research practice.

Falsificationism. Despite the prominence of arguments for eliminationist testing strategies in archaeology, Popperian influences are surprising muted. They are evident among archaeologists who reject the positivism of the New Archaeology

but embrace its scientific ambitions; for example, Peebles draws selectively on Popper, and also Toulmin, in the context of an argument for an ontologically richer but no less empirically rigorous archaeology [Peebles, 1992, 364-367]. As indicated earlier, a strict Popperian approach has been championed by Bell who, in 1994, renewed the New Archaeologists' argument against "inductivism," arguing that hypothesis testing in archaeology should be exclusively a matter of endangering bold conjectures — testing to expose their weaknesses and errors — not a process of building evidential support for hypotheses [Bell, 1994].

Bootstrapping and evidential robustness. By the early 1980s both critics and proponents of archaeological positivism had accepted contextualist arguments (from Kuhn and Hanson) to the effect that evidential claims are inevitably theory-laden [Binford and Sabloff, 1982; Hodder, 1982b]. New Archaeologists and their successors turned their attention to empirical research programs — experimental archaeology, ethnoarchaeology — designed to secure the array of auxiliary assumptions ("middle range theory") that establish causal, functional, symbolic and other connections between the elements of material culture that survive in the record and the kinds of antecedent events or conditions that can be inferred (with varying degrees of reliability) to explain their production and survival in archaeological contexts. Internal metamethodological and philosophical analysis has increasingly focused on questions about how such mediating assumptions function in evidential reasoning and how the credibility of the resulting evidential claims is established and assessed. A number of philosophical accounts have been proposed that incorporate both a normative and a descriptive component, reconstructing the principles that underpin best (evidential) practice in archaeology.

One point of departure for these analyses has been Glymour's bootstrapping model of confirmation; his account of "deductions from the phenomena" throws into relief the central role played by mediating assumptions and background knowledge in bringing evidence to bear on a test hypothesis [Glymour, 1980]. I found this account useful in showing why the reliance on auxiliaries need not entail vicious circularity, even if these auxiliaries are components of the theory under test [Wylie, 1986b]. In archaeological contexts, the conditions under which such circularity threatens are rarely realized; there are few overarching theories that incorporate both the hypotheses archaeologists are interested in testing and the linking principles necessary to interpret data as evidence relevant for testing these hypotheses. Archaeologists typically rely on a wide range of background sources to interpret their data as evidence, few of which are components of — or, more precisely, few of which entail or are entailed by — the explanatory and reconstructive hypotheses archaeologists are interested in testing or, indeed, the broader theories of cultural process presupposed by these hypotheses. It is this potential independence of evidence from test hypotheses that accounts for the recalcitrance of the archaeological record, its capacity to subvert even deeply entrenched assumptions about the cultural past [Wylie, 1989b].

A number of accounts have been developed of the conditions under which epistemically significant independence can be established between (interpreted) evi-

dence and the hypotheses it is used to evaluate. Kosso has proposed an elegant analysis of independence within chains of evidential inference in connection with his account of archaeological observation [Kosso, 1991; 1992; 1993; 2001, 75-89]. I have argued that epistemic independence is established on two dimensions: vertically, between test hypothesis and linking principles (as described above); and horizontally between distinct lines of evidence each of which is constituted by a different body of background knowledge [Wylie, 1996a; 2000a]. Independence on these dimensions complements the assessments of the security of particular lines of evidence that is the focus of archaeological efforts to establish robust experimental and ethnoarchaeological principles on which to base the interpretation of data as evidence. Kosso and I both argue that these models can be generalized well beyond archaeological practice; they are inspired by and extend the analyses of evidential robustness and strategies of triangulation developed by Wimsatt, Shapere, and Hacking, among others [Hacking, 1983; Shapere, 1982; 1985; Wimsatt, 1981]. Kosso emphasizes continuities with the natural sciences [Kosso, 2001, 39-48]; I identify similarities with strategies of ethnographic and historical interpretation [Wylie, 1989a].

2.3   Ideals of Objectivity; Relativist Challenges; Epistemic Pluralism

Disillusionment with the positivism of the New Archaeology provoked an especially acute challenge to objectivist ideals by the mid-1980s, regenerating, in a new formulation, the speculative horn of the interpretive dilemma. Post-processual critics insisted that the theory ladenness of evidence entails a vicious circularity. It must be conceded, Hodder argued, that archaeologists simply "create facts" [Hodder, 1983, 6], and if, on these arguments, archaeological evidence is "always already" an interpretive construct, it cannot function as an independent arbiter of the credibility of interpretive or explanatory claims about the past; there is "literally nothing independent of theory or propositions to test against" [Shanks and Tilley, 1987, 111]. This "hyperrelativism," as Trigger described it [Trigger, 1989b], was reinforced by the results of detailed empirical studies of the "sociopolitics" of archaeology which demonstrate how deeply archaeological thinking has been implicated in and influenced by the power relations constitutive of the contexts in which it is practiced; these include the entanglement of archaeology with colonial, nationalist, and imperialist enterprises detailed by Trigger, and with the interests of intra-national elites described by Patterson, as well as analyses that have exposed pervasive sexist, androcentric, and racist biases [Gero and Conkey, 1991; Gero et al., 1983; Patterson, 1986a; 1986b; Trigger, 1989a]. At their most extreme, post-processual critics concluded that archaeologists should give up all pretense to ideals of value neutral objectivity and candidly resolve to "tell the stories" that need to be told, stories that are politically salient in specific contexts of action [Hodder, 1983; Shanks and Tilley, 1987]. In the event, however, few who endorsed this reaction against the New Archaeology have maintained a consistently relativist stance, if only because it quickly proves to be self-defeating,

politically as much as epistemically [Wylie, 1992, 270-272]. They shifted ground, endorsing a "guarded commitment to objectivity" [Hodder, 1991, 10], a "particular and contingent objectivity" [Shanks and Tilley, 1989, 43], that underwrites their critical reassessments of conventional forms of interpretation and their proposals for more richly humanistic alternatives. In the process of advancing these jointly critical and constructive research agendas post-processualists routinely made effective use of the capacity of the archaeological record to expose error and canalize interpretive theorizing, declaring that, despite being radically a construct, it can very fruitfully be deployed as "a network of resistances to theoretical appropriation" [Shanks and Tilley, 1989, 44]. But despite these reversals and the manifest contradictions they introduce, post-processualists have done little to reassess the premises that initially led to relativist conclusions, or to develop a constructive account of the objectivist ideals they now endorse as an alternative to the positivist, scientistic conceptions of objectivity they repudiate. The analyses of epistemic independence outlined above were developed, in part, in response to this lacuna.

One mediating position that moves in the direction of articulating a principled refusal of the extremes of objectivism and relativism generated by debate over the New Archaeology is the "moderate relativism" for which Trigger has argued since the late 1960s [Trigger, 1978; 1995]. In his most detailed defense of this position Trigger outlines an evolutionary argument to justify the conviction that our best knowledge producing and certifying practices track the truth; humans would not have survived had we not developed perceptual and cognitive systems that provide reliably accurate guidance in the environments we negotiate [Trigger, 1998]. While this may account in very general terms for epistemic success in the tuning of human cognitive abilities, I find it uncompelling as a justification for confidence that the epistemic practices specific to archaeology are reliably self-correcting [Wylie, 2006]. Following Trigger's own lead, as a prominent analyst of the social, political, and economic factors that have shaped research practice, a more promising approach would seem to be a discipline-specific investigation of the conditions under which systematic error has arisen, and been identified and corrected, coupled with close analysis of the strategies by which archaeologists deploy empirical "resistances" in this process of model building, testing, and revision.

Another more common response to the sharply drawn conflict over objectivist ideals has been to endorse a pluralist stance that fosters tolerance for divergent traditions of practice. Confronted with sharp differences in interpretative and explanatory understanding of the past which are, in turn, rooted in fundamental disagreement about the goals and standards of inquiry, a growing number of archaeologists reject the assumption that epistemically credible inquiry must adhere to a unified set of regulative ideals and should be expected to generate results that converge on, a single (true) account of the cultural past. This pluralism is especially prominent among anti- and post-processual archaeologists who advocate more humanistic, "interpretivist" approaches to archaeology [Hodder, 1999], and it is reinforced by challenges from descendant communities, especially indigenous and aboriginal communities, who insist that scientific modes of inquiry should not

be privileged in relation to their traditional understanding of the past.

While such a pluralism is attractive in defusing contentious disagreement, it sidesteps the difficult epistemic questions that arise when divergent research traditions generate substantive disagreements about the past. Epistemic pluralism presupposes a quasi-empirical thesis to the effect that these disagreements often do, or could, reflect epistemic ideals that are literally incommensurable; they arise from research traditions that have such fundamentally different aims and standards of adequacy there is no basis for comparing or adjudicating the divergent forms of understanding they produce. This assumption is rarely explicitly defended although there are strong reasons for suspecting that it is realized, if at all, in a small minority of cases.[8] Where the differences between self-consciously scientific (processual) and deliberately humanistic (anti- or post-processual) archaeologies are concerned, both archaeological and philosophical commentators have argued that the polarizing dynamic of debate has obscured much that they share; in practice, adherents to these programmatically different approaches rely on essentially the same strategies for building evidential claims and the same standards of adequacy in evaluating them [Kosso, 1991; VanPool and VanPool, 1999; Wylie, 1992]. Considering the case more broadly, I have argued that ideals of objectivity are best understood as designating a cluster of epistemic virtues rather than a fixed, foundational standard for adjudicating epistemic differences [Wylie, 2000b]. These include considerations of empirical adequacy, internal coherence, explanatory power, and various forms of consistency with well established bodies of knowledge in related areas, each of which requires interpretation and must be weighed against the others; their implications for practice are by no means fixed and they are open to continuous reassessment and refinement within research traditions. So conceived, the epistemic virtues that constitute objectivity offer numerous bases for comparison between traditions; they do not guarantee definitive resolution of inter-tradition differences, but they counter the presumption that, if no one authoritative (monolithic, foundational) standard can be identified that cross-cuts all traditions, these differences are non-negotiable. In this they open space for what I have described as a "mitigated objectivism" [Wylie, 1996a; 2000b]. Where the crucial points of comparison are evidential, as they often are, the models of evidential reasoning described above offer a finegrained account of the jointly empirical and conceptual considerations (of security and epistemic independence) that are likely to be at issue, systematic adjudication of which can productively stabilize debate in cases of recalcitrant conflict even if they do not constitute a fixed foundation.

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[8]For a parallel argument that addresses the presuppositions of moral relativism, see Moody-Adams [1997].

2.4 Foundational Questions: Concepts of Culture, Social Theory and Social Ontology

Analyses of explanation in archaeology, models of evidential reasoning, and arguments for (and against) objectivism and epistemic pluralism are all, to varying degrees, informed by positions on foundational questions about the nature of the cultural subject matter. Ontological issues arise most explicitly in highly localized debates about the status of typological (and other) archaeological constructs, and they are actively debated (often less explicitly) in connection with the broader theoretical commitments that underpin competing research programs.

Typological constructs. One locus of ontological disagreement in archaeology is a set of questions about the status of typological constructs: are these strictly heuristic devices, problem-specific "tools" useful for organizing and manipulating archaeological data, or do they capture structures inherent in archaeological assemblages that embody the cultural norms, categories, and identities of those who produced and used this material? A sophisticated typological instrumentalism has been advocated by Adams and Adams (an archaeologist and a philosopher), who articulate the philosophical underpinnings of a constructivist view of archaeological typologies that had been proposed much earlier by archaeologists wrestling with the challenges of systematizing rapidly growing stores of archaeological data at mid-century [Adams and Adams, 1991; Brew, 1946; Ford, 1954b]. By sharp contrast, a prominent antecedent of the New Archaeology, Spaulding, defended an uncompromising typological realism in direct and polemical rebuttal to Ford [Ford, 1954a; Spaulding, 1953a; 1953b]. Several New Archaeologists subsequently argued for mediating positions that acknowledge the constructed, problem-specific nature of typological schemes, but urge a qualified realist interpretation of the empirical regularities they capture [Hill, 1972; Hill and Evans, 1972; Krieger, 1944]. In a quite distinctive approach, Gardin (a French archaeologist), advocates a formalist analysis of the inferential strategies underlying all forms of archaeological constructs in connection with which he emphasizes the selective, interpretive dimensions of the process by which archaeologists describe and systematize their data [Gardin, 1980].

Theoretical constructs and ontological commitments. The resolutely materialist, ecosystem models advanced by the New Archaeologists were formulated in reaction against "normative" theories of culture: the view that cultures are essentially systems of shared conventions and cultural norms expressed in the behavior of individual culture bearers and in the material things they make and use [Taylor, 1948]. By deliberate contrast with such "idealism," which gave central place to such inscrutables as agents' intentions and collective beliefs, the New Archaeologists insisted that cultures are, fundamentally, ecologically adaptive systems governed by natural laws. The beliefs and intentions of human agents, and the shared conventions that constitute their cultural lifeworlds, may facilitate the adaptive responses of human groups but should be regarded as causally dependent variables (interchangeable and epiphenomenal); they are explanatorily irrelevant to a

robustly scientific understanding of the cultural systems that mediate the survival
of human populations. As indicated at the outset, the goal of inquiry, on this
research program, was to understand, not specific events and conditions in the
cultural past, but the large scale (generalizable) processes that shape cultural sys-
tems in adaptation to their external environments [Binford, 1962; Flannery, 1967;
Watson and Watson, 1969]. The most reductive and deterministic variants of this
position were decisively challenged within a decade by post- and anti-processual
theorists who made effective use of both ethnographic and archaeological data
to demonstrate that contingent, idiosyncratic features of the ethnographic life-
world can, and routinely do, shape the large scale, long term development of cul-
tural systems, sometimes in adaptively dysfunctional ways [Hodder, 1982b]. Since
that time archaeologists sympathetic to the general outlines of the New Archaeol-
ogy program have advocated substantially more capaciouis theoretical frameworks
[Cowgill, 1993; Hegmon, 2003]; some retain the focus on cultural evolution and
adaptive response but rely on more flexible models drawn from behavioral ecol-
ogy [Bamforth, 1988; Broughton and O'Connell, 1999], while others advocate a
behavioral archaeology [Reid *et al.*, 1975], now expanded to incorporate consid-
eration of symbolically rich, religious and ritual behaviors that had traditionally
been excluded from consideration [Skibo *et al.*, 1995]. At the same time, however,
the ambition of eliminating agency and all "ideational" factors from archaeologi-
cal explanation has been revived and set in a strict selectionist framework by the
advocates of evolutionary or Darwinian archaeology [Dunnell, 1980; 1992; Lyman
and O'Brien, 1998; Maschner, 1996; Tschauner, 1994]. On these models, all forms
of cultural behavior and material are to be explained as elaborations of the human
phenotype in response to specific selection pressures. These claims have gener-
ated intense debate in which the sharpest critics, often behavioral ecologists, draw
attention not only to the archaeological limitations of such an approach, but to
a range of (implausible and often inconsistent) ontological presuppositions that
are rarely stated explicitly and that betray pervasive confusion about the central
tenets of evolutionary theory as developed in biology [Bamforth, 2002; Schiffer,
1996].

The philosopher R. Watson did explicitly advocate a model of ontological re-
duction, along lines advocated by Oppenheim and Putnam [Oppenheim and Put-
nam, 1958], according to which the cultural systems archaeologists study stand
at the top of a hierarchy of ontological complexity, and are ultimately reducible,
step-wise, to constituent social and institutional systems, individual psychology,
cognitive neuroscience, and then various levels of bio-physical phenomena [Wat-
son, 1972]. Few archaeologists have advocated any such comprehensive reduction
program. In fact, although the advocates of strict ecosystem and evolutionary ap-
proaches sometimes seem to presuppose a thesis of ontological unity and theoretical
reduction along lines made explicit by Watson [Dunnell, 1971], they also regard
large scale cultural systems and the cultural processes that operate on and through
them as emergent entities, endowed with causal powers that do not derive from,
and are not presumed to be reducible to, any of their constituents. Binford comes

closest to endorsing such a position in his vitriolic rebuttals to the "individualist" and humanist tendencies he attributes to post-processual critics [Binford, 1989], as do the most strident advocates of evolutionary archaeology when they decry any consideration of the "behavioral" dimensions of cultural systems as causally and explanatorily irrelevant [O'Brien *et al.*, 1998]. Salmon's trenchant critique of the ontological claims implicit in systems models has been influential in this debate [Bamforth, 2002; Salmon, 1978a], but I know of no systematic philosophical analysis of the claims central to this internal debate about evolutionary and eco-system theories and it is sorely needed.

The post- and anti-processual critics who rejected the New Archaeologists' ecosystem models have explored a range of theoretical perspectives that emphasize, variously, individual agency [Collingwood, 1946; Dobres and Robb, 2000; Hodder, 1991; Tringham, 1991]; social networks, hierarchies, and institutions [Meskell and Preucel, 2004]; the symbolic and semiotic (broadly, "ideational") dimensions of cultural lifeworlds [Byers, 1999; Gosden, 1994; Hodder, 1982a; Hodder, 1982b; MacWhite, 1956]; and the cognitive underpinnings of all these aspects of human behavior and cultural life [Gardin and Peebles, 1992; Renfrew, 1982b; Renfrew and Zubrow, 1994; Whitley, 1998]. In the process, the case has been made on many fronts for reconceptualizing material culture in much richer terms than envisioned either by the New Archaeologists, for whom it was an "extra-somatic means of adaptation" [Binford, 1962], or by the "normative" theorists they reacted against. Structural and symbolic archaeologists had reopened the case for considering the "ideational," normative dimensions of cultural life; they urged an understanding of material culture as meaning-encoding and meaning-bearing, its form and structure constituted by a linguistic-like grammar. By extension, the advocates of a more agentic view insist that material culture must also be recognized to actively constitute social and cultural meaning. Although Latour has not been invoked in this connection until recently [Meskell, 2004], this approach seems to presuppose an actant-agent ontology along lines he advocates in *We Have Never Been Modern* [Latour, 1993]. This represents the most radical departure, ontologically, of all the possibilities for theorizing the cultural past currently being explored by archaeologists.

2.5 Archaeological Ethics

The pressure to engage normative issues has been mounting since the early 1970s when a number of factors conspired to fundamentally change the conditions of under which archaeology is practiced. I have argued that these give rise to three constellations of issues that are currently transforming the disciplinary identity and the practice of archaeology [Wylie, 1996b; 2005].

One set of issues, long a concern for archaeologists but now especially acute, is the rapidly accelerating destruction of archaeological resources as a consequence of land development, war, and the demands of an international antiquities market that expanded dramatically in the 1980s and 1990s, now exacerbated by on-line

trading [Green, 1984; Lipe, 1974; Vitelli, 1996]. A conservation ethic, first articulated by Lipe, is the centerpiece of many archaeological codes of conduct and statements on ethics [Archaeological Institute of America, 1991; Society for American Archaeology, 1995; Society for American Archaeology, 1996; Society for Historical Archaeology, 1992]. A number of questions have been debated in connection with this conservation ethic, all of which focus attention on tensions between a commitment to protect archaeological resources, consistent with this ethic, and the research goals of the discipline, when these would be served by the destructive investigation of archaeological sites and material, or by collaborating with commercial salvors or publishing looted and illegally traded antiquities. Lipe has since taken up the question of whether archaeologists are ever justified in excavating sites that are not otherwise endangered [Lipe, 1996], and active debate continues on the question of what responsibilities archaeologists have to avoid entanglement with, or to actively counter, commercial exploitation of the archaeological record [Elia, 1992; 1993; Gill and Chippindale, 1993; Messenger, 1999; Renfrew, 2000].

A second, closely related set of issues has arisen as the requirements of culture resource management reinforce the professionalization of archaeology. A majority of archaeologists are now employed in private industry, by contract firms that provide archaeological assessment services, or by the government agencies that oversee these assessments and manage public sites and monuments. This puts archaeologists in the position of negotiating the conflicting demands of employers, regulatory bodies, various public interest groups, and their responsibility to contribute to the research goals of the discipline of archaeology. Debate about how these conflicts should be resolved are ongoing, and independent codes of conduct have been drafted to provide professional archaeologists guidance under these circumstances [Society of Professional Archaeologists, 1991].

The most high profile and contentious issues with which archaeologists currently grapple have been raised by the governments of archaeologically rich nations, and by descendent communities and other interest groups who challenge archaeologists' rights of access to and use of archaeological sites and material, often on grounds that scientific investigation does not serve their interests in what they regard as their cultural heritage [Dongoske *et al.*, 2000; Gathercole and Lowenthal, 1990; Swidler *et al.*, 1997; Thomas, 2000; Watkins, 2000]. In some jurisdictions demands for repatriation and other forms of control of cultural sites and materials have been enforced by legislation, and a number of archeological societies have instituted codes of conduct that specify archaeologists' obligations to indigenous peoples [Canadian Archaeological Association, 1997; World Archaeological Congress, 1991]. More broadly, issues of accountability have become the focus of intense debate which has, in turn, generated a searching reassessment of disciplinary goals and standards, evident in contention over the ideals embodied in an ethic of stewardship [Lynott and Wylie, 1995; Lynott and Wylie, 2000; Wylie, 2005], and in response to the relativist implications of some of these arguments [Clark, 1996; Salmon, 1997; 1999]. Thus far, most of the work on ethics issues in archaeology has been case-specific or oriented to the articulation of codes of

conduct, and it is almost entirely internal to archaeology. This is an area in which analysis that draws on the resources of philosophical ethics and social/political philosophy has a great deal to contribute.

2.6   Metaphilosophical Issues

Prominent in the exchanges between archaeologists and philosophers is a complex of questions about how metaarchaeology should be defined and situated and what, more broadly, philosophical analysis has to offer a field like archaeology. Some contributors to this growing interfield maintain that there are irreducible differences between the interests of philosophers and archaeologists, even when similar questions seem to be at issue [Embree, 1992; R. Watson, 1991; Clarke, 1973; Flannery, 1982]. Typically, however, a case is made for establishing metaarchaeology as an interdisciplinary venture grounded in both archaeology and philosophy, as well as a range of other science studies disciplines. At the very least, the early, acrimonious exchanges between archaeologists and philosophers made it clear that the interests of neither field would be served unless philosophical analysis is grounded in a robust understanding of the practice of archaeology, its major research programs, and its results. The provocative question now is whether philosophical analysis must also be naturalized in the sense of being grounded in an empirical understanding of the history, the social, political, and economic contexts of practice, and the disciplinary culture and institutions of archaeology. A growing number of the archaeologists and some of the philosophers engaged in interfield exchange argue that conceptual, philosophical analysis is inadequate to address many of the questions that are most pressing for archaeologists or, indeed, the questions central to conventional philosophical inquiry [Wylie, 2000a, 312-313]; it must be supplemented or, indeed, supplanted by (empirical) historical and sociopolitical studies of archaeology. Kelley and Hanen, and Gibbon, made the case for a socially naturalized philosophy of archaeology in the 1980s, and a thriving program of critical social history has since taken shape that documents myriad ways in which the intellectual agenda of archaeology is shaped by the influence of funding institutions and disciplinary reward systems [Patterson, 1995; Paynter, 1983; Wobst and Keene, 1983], as well as by nationalist and colonial interests [Abu El-Haj, 2001; Patterson, 1986b; Trigger, 1989a] and the race, gender, and class divisions that structure both internal disciplinary dynamics and the larger contexts in which archaeology is practiced [Gero and Conkey, 1991; Moser, 1998; Trigger, 1980]. As analytic metaarchaeology expands in these directions it exemplifies the socially naturalizing trends evident in post-postivist philosophy of science:

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RELATIVISM AND HISTORICISM

Ian Jarvie

> The doctrine of cultural relativism is apparently regarded as one of the major achievements of contemporary ethnology by many American anthropologsits...
> David Bidney, 1953

> Relativism has been a persistent feature of social science, and virtually an article of faith in anthropology and sociology.
> Bryan S. Turner, 2005

1 INTRODUCTION

"Relativism" is a general term for the assertion, rarely made without the desire to shock, that certain variables hitherto thought to be independent, are in fact dependent; that is, they vary relative to something else. Characteristically it is asserted of moral claims, and claims bearing truth values. Moral claims and truth claims, it is said, are not self-evident or absolute, but are relative to, for example, the social class or the cultural conventions in which those who utter them are embedded. Morality and truth are not always relativized together but because the self-same arguments are deployed to relativize each one they can be treated together [Jarvie, 1975]. Historicist relativism is that special case of relativism in which morality and truth are relativized to time, or to historical period, or to *Zeitgeist*, or to historical context. (Non-relativist historicism will be discussed below.)

Different relativisms are prominent in anthropology and in sociology. *Cultural relativism* is found primarily in twentieth century American anthropology and flows from the influence of Franz Boas.[1] Its principal advocates were Ruth Bene-

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[1]Scholars differ about Boas's stance *vis à vis* relativism. His aversion to generalization and comparison (his particularism) would never have allowed him to formulate anything so general [Rudolph, 1968, 27]. Yet note the *faute de miuex* argument below in section 8.1 below. Scholars do not differ about the relativism espoused by some of Boas's students. It is sufficient for present purposes to note that Boas lived long enough that he could have distanced himself from the emerging relativism of his followers had he so wanted.

dict and Melville J. Herskovits.[2] It was applied to morality, truth, and, sometimes, knowledge. *Linguistic relativism* was also mostly an anthropological phenomenon, its advocates being Edward Sapir and Benjamin Lee Whorf [Kay and Kempton, 1984]. In fieldwork anthropology there was an almost universal *methodological or descriptive relativism* in the same period. In sociology the most important forms of relativism were the *sociology of knowledge* associated with Max Scheler and Karl Mannheim, and the social construction of reality associated with W. I. Thomas, Alfred Schutz, Peter Berger and Thomas Luckmann, Burkart Holzner, and Tgorny Segerstedt. This was developed and extended in the second half of the twentieth century into a sociology of scientific knowledge known as the *Strong Programme in the sociology of knowledge* and associated with the Science Studies Unit at the University of Edinburgh [Bloor, 1981]. It went contrary to the traditional view that science is absolute (because inductive) that had hitherto exempted scientific knowledge from sociological explanation. The exemption had been granted by Durkheim and his school, as well as Mannheim, following the Marxist tradition. The Edinburgh school refused to do so on principle.[3]

A form of relativism that influenced both anthropology and sociology was *relativism about rationality*. This had at least three prongs of inspiration. There were anthropological studies of apparently irrational behavior (e.g. witchcraft, cargo cults, rain dances) that successfully showed it to have its own rationality.[4] There were comparative and historical sociological studies to the same effect (e. g. religious ritual, crowd behavior, moral panics, soccer hooligans). And there were arguments drawn from philosophy (e.g. Hegel, Wittgenstein, Winch, Taylor) to the effect that judgments of rationality depended upon context and so could not be sustained across contexts (see [Wilson, 1970; Hollis and Lukes, 1982; Yoshida, 2005]). Relativism about rationality is the procedural version of relativism about morality and truth. Rationality concerns the means of resolving disputes, including disputes about what is moral or true. When the claim is made that there are different rationalities, varying with culture and the like, resolution of disputes by rational means is being constrained. Inter-cultural and other wider-ranging disputes are left with no transcendent court of rational appeal [Jarvie, 1983, 1984; Jarvie and Agassi, 1996].

In exploring these various modes of relativism this chapter will concentrate on cultural relativism in anthropology, the strong programme of the sociology of knowledge, the social construction of reality, and relativism about rationality. Historicist relativism will be treated more briefly because, as a special case, it is subject to the same arguments for and against as are other versions of relativism. Each of the forms of relativism chosen has a strong presence in contemporary anthropology and sociology. Also, each has a weak formulation that is not relativistic,

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[2]Spiro [1986; 1992] distinguishes three versions: descriptive, normative, and epistemological. The first two terms will be adopted, the third version has to be further broken down when the reach of discussion extends beyond anthropology.

[3]For further discussion of the sociology of knowledge, including the Edinburgh school, see Zammito, this volume.

[4]For further discussion of the problem of apparent irrationality, see Lukes, this volume.

and a strong formulation that is. It is the latter on which we shall focus, although sometimes clarity demands spelling out both formulations and keeping them separate. After some introductory discussion (section 2) relativism and historicism will be presented in some detail (sections 3, 4, 5 and 6), social and policy implications will be explored (section 7), and relativism in general will be subjected to unsparing critical scrutiny (section 8) and its alternative, the true anthropological situation will be outlined (section 9).

2   FIRST ANALYSIS OF RELATIVISM: PASCAL

A succinct expression of relativism is articulated in this celebrated passage from Blaise Pascal:

> There is almost nothing right or wrong which does not alter with a change in clime. A shift of three degrees of latitude is enough to overthrow jurisprudence. One's location on the meridian decides the truth, that or a change in territorial possession. Fundamental laws alter. What is right changes with the times. Strange justice that is bounded by a river or mountain! The truth on this side of the Pyrenees, error on the other [Pascal, 1670, 294].

Pascal makes a number of points in the quoted passage. First, he is articulating relativism before the sciences of anthropology and sociology had come into being. Hence there are no grounds for the impression anthropologists sometimes give (see [AAA, 1947]) that the doctrine is a monopoly of their avocation or those influenced by it. Taking a still longer view one might even say: relativism is a standard trope in skepticism and hence its roots are ancient. As such, relativism is a version of what is known in the philosophical trade as fideism. Fideism is the view that *the ground for all rational discussion is a (non-rational) leap of faith*: one can be rational within systems of ideas so entered, but not about the entry to them. Rationality then is relative to the system of ideas into which one leaps. When the faith leaped into varies systematically with class position, culture, historical period, and the like, the version of cultural relativism embraced is class, cultural, historical period, and the like.

Secondly, as relativism comes in various versions, it is a class of doctrines rather than a single, unified one. Relativists may appeal, as Pascal writes, to the influence of climes, meridians, property, time, and mountains. Those are the precursors of cultural relativism. Options such as the *Zeitgeist*, culture, and social class were added later.

Thirdly, Pascal is in eyebrow-raising mode. That is, he considers it "strange" that the meridian decides truth or that truth and justice change with the times. His rhetorical eyebrows are raised at the relativists, not at their critics. In his view, justice and truth are absolute. He is shocked that there are those who see no problem when killing is positively sanctioned on one side of a river and negatively sanctioned on the other. Why cannot the customs of the country be subject to

assessment as right or wrong, its judgments as to their truth or falsity examined for truth or falsity? The short answer for the relativist is that what is moral and what is true are relative to something else. Pascal wished for a transcendental assessment, outside of clime and time; the relativist does not accept that there can be such trans-social, trans-cultural, or trans-temporal standards. The differences are incommensurable. The view from nowhere is inaccessible.

The extract from Pascal poses a difficulty of reading that we shall try to dispel by analysis. Pascal mixes what under analysis we may separate out as the normative and descriptive claims of relativism. He *describes* differences about morality and truth and *evaluates* them by suggesting that the situation is rationally incoherent ("Strange justice", "truth on this side of the Pyrenees"). Descriptive relativism, which undergirds doctrines of method (discussed in section 8.5 below), is the factual observation that different cultures and societies differ on questions of value and of truth. These differences cross cultures, social classes, epochs. The normative claim, which Pascal finds it hard to accept, is that these differences cannot be adjudicated. How can the same thing be wrong here but not there? How can the same idea be true here, false there? Which is it, right or wrong; true or false? To ask this question is to demand norms by which to judge conflicts of norms.

3   THE APOTHEOSIS OF RELATIVISM: HERSKOVITS

The American Anthropological Association's 1947 "Statement of Human Rights" [AAA, 1947] can be taken as the apogee of the first wave of relativism. (The second wave comes from interpretivism and postmodernism and is conventionally first identified around 1970.) Subsequently, anthropologists and sociologists became very cagey about saying whether or not they espoused relativism [Kluckhohn, 1955]. After all, the philosophical objections were devastating [Williams, 1947; Schmidt, 1955; Siegel, 1987; Harris, 1992; Cook, 1999]. One looks in vain for unambiguously formulated statements of unqualified relativism. Yet to this day students articulate relativism as an obvious truth and as the moral high ground. Somehow, despite the caginess of scholars and teachers, relativism must come through between the lines. Before the second wave supervened, then, cultural relativism became a doctrine more implicit than explicit, more insinuated than formulated. But there is one well-known exception, already noted by Kluckhohn in 1955. Although philosophically not very satisfactory (see [Bidney, 1953a, 423-29]), the classic statement of untrammeled cultural relativism is by the American anthropologist Melville J. Herskovits in 1955:

> The principle of cultural relativism, briefly stated, is as follows: *Judgments are based on experience, and experience is interpreted by each individual in terms of his own enculturation.* Those who hold for the existence of fixed values will find materials in other societies that necessitate re-investigation of their assumptions. Are there absolute moral

standards, or are moral standards effective only as far as they agree
with the orientations of a given people at a given period in their his-
tory? ...We even approach the problem of the ultimate nature of re-
ality itself. ...Is reality...not defined and redefined by the ever-varied
symbolisms of the innumerable languages of mankind? [Herskovits,
1972, 15][5]

Here Herskovits contrasts relativism not only with its philosophical contrary *ab-
solutism* but also with an anthropological alternative, *ethnocentrism*:

Ethnocentrism is the point of view that one's own way of life is to be
preferred to all others. [21]

He finds that cultural relativism puts value judgments and even reality itself into
question; in so doing, anthropology makes a profound contribution to the analysis
of man's place in the world [15].[6]

Herskovits bundled what we have analysed as two distinct claims into cultural
relativism: moral relativism and cognitive relativism. Moral judgments (e. g.
"cruelty to children is wrong") are "effective only so far as they agree with the
orientations of a given people at a given period of their history". This could
be better stated: the *effectiveness* of moral judgments is a factual matter. That
cultures endorse certain moral standards and that these standards vary from place
to place and over time is not in dispute. The question is whether there are or can be
moral standards by which to judge the local moral standards of cultures. Arguing
that judgments rest on experience and experience is enculturated, Herskovits holds
the answer to be that there can be no transcultural moral assessment of diverse
moral claims.

Yet it was Edward Westermarck, representative of an earlier anthropological
generation than Boas, Malinowski, and Radcliffe-Brown, who was the first to ar-
ticulate ethical relativism in the twentieth century.[7] His argument was that since
ethics were a function of emotional response, and since emotional responses were
induced in the process of socialization, so ethical views could not but systemati-
cally vary from society to society. Societies, he held, were unique concatenations of
individuals, traditions, institutions, and the circumstances with which they must
cope, and hence could not be expected to have the same emotional responses and,
therefore, values [Westermarck, 1932]. In the general obliteration of previous an-
thropology engineered by Malinowski, Westermarck and his priority were more or

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[5]Tennekes notes that Rudolph [1968] presents Herskovits as a "summarizer of what was held by
his predecessors and as the systematic champion of the cultural relativistic doctrine" [Tennekes,
1971, 2n]. Herskovits was on the ad hoc committee that drafted the 1947 AAA "Statement on
Human Rights" (discussed further in section 7, below). See [Bidney, 1953b, 693].

[6]In arguing thus, Herskovits presents relativism as a variant of philosophical idealism. This
was remarked upon by Bidney [1953a, 423].

[7]Readers concerned to pursue moral relativism as such are directed to [Ladd, 1957; 1963;
Moser, 1968; Hatch, 1983; Cook, 1999].

less forgotten.[8]

Even stronger is Herskovits's second claim, about cognition: reality (e.g. "the world is globular, not flat") is "defined and redefined by the ever-varied symbolisms of the innumerable languages of mankind". Since judgments about reality are both a product of enculturation and necessarily articulated in a particular language, it would seem to follow that their *effectiveness* (Herskovits should have said "truth") depends, again, on agreement (not with facts but) "with the orientations of a given people at a given period of their history".

For Herskovits, the proper anthropological attitude to morality and to claims about reality is one of qualified assertion. What the anthropologist can report is, "Cruelty is wrong *according to the X*"; "the world is flat *according to the Y*". A question like "Is cruelty wrong?" cannot be answered outside of a cultural context. A question like "Is the earth flat?" also cannot be answered outside of a cultural context. What makes an answer to either kind of question a true answer is the fact of its endorsement by a culture. Thus the ancient philosophical questions of what is good and what is true are to be answered by factual ethnographic reports from different cultures. That different cultures differ in what they declare good and true shows the Herskovitsian cultural relativist that no culture-transcending answers to the ancient questions are possible. Cultures are the ultimate authorities on morality and truth. Since they differ, there are multiple truths and multiple moralities. Individuals cannot adjudicate these differences and science insists it can only record them. It further follows, given Herskovits's insistence on the cognitive twist, that these multiple cultures with their multiple moralities must, because of their multiple truths, live in separate and different worlds. The idea might be labeled "the cultural construction of reality".

4 SOME PRECURSORS OF RELATIVISM AND HISTORICISM

The history of relativism in general and its special case historicism can be written to great length or to short form depending upon point of view. A sufficiently broad conception will allow it to be treated as ancient and hence to make the history long drawn out; a sufficiently narrow conception can make that history very short. For example, almost any problem of anthropology and sociology can be traced back, should we be so minded, at least as far as the Ancient Greeks and Hebrews. With the problem of relativism it is probably better to stick to the modern world. For only in the modern world have the intellectual arguments of relativists been given practical effect and been urged as guides to behavior, and as principles of public policy. Hence this section is not even an historical sketch, but merely a few nuggets from history to put relativism into intellectual context and to diffuse any element of its shock value that comes from thinking of it as novel. We shall then

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[8]It is instructive to look at Emile Durkheim's review of Westermark's earlier work of 1906, *The Origin and Development of the Moral Ideas*. It is quite scathing. See Durkheim in [Nandan, 1980, 150–59].

look at an exhibit that treats the history as very short.[9]

One of the very earliest expressions of relativism in the European tradition is Presocratic. In certain fragments (DK 16 and 15) Xenophanes reasons to this effect:

> The Ethiops say that their gods are flat-nosed and black
> While the Thracians say that theirs have blue eyes and red hair.

> Yet if cattle or horses or lions had hands and could draw
> And could sculpture like men, then the horses would draw their gods
> Like horses, and cattle like cattle, and each would then shape
> Bodies of gods in the likeness, each kind, of its own.[10]

Scholars usually date Xenophanes to around 570–480BC. Xenophanes's is a descriptive and a normative relativism. It says that groups attribute their own characteristics to the gods in a natural way. It is natural, the argument goes, because were animals able to draw, they would do likewise. We take for granted that the way things are with us is the way things everywhere should be judged. The point being made is this: because of this tendency to naturalize, when we de-naturalize we have to discount for other times and other mores.

By the second century of the modern era the physician Sextus, known as Empiricus, who had become head of the Academy, compiled a survey of the ancient skeptical arguments in which what came to be known as fideism was consolidated. Popkin has traced the way Sextus entered the European tradition of thought in the Renaissance [1960, ch. II; 1970; 1992]. Texts were first established and printed in the 1560s. Eventually central figures such as Michel de Montaigne, Herbert of Cherbury, René Descartes, and David Hume were to study the ancient arguments carefully. Most of them were, of course, intent on refuting or rebutting skepticism — usually in the name of religion, alternately in the name of reason. Whatever the intent, the effect of their commentary was to give wider circulation to these ancient arguments.

It is important for our sense of perspective and shock to realize that the ancient skeptics pushed the arguments much further than most twentieth century social science relativists. Not content with the descriptive relativism of societies, cultures, historical epochs and the like, they pushed on to show that perceptions and judgments varied even from individual to individual. This leads to the conclusion that all things are relative and hence judgment must be suspended "as to what things are absolutely and really existent" (Sextus I, 135).

> We have already argued that all things are relative — for example with
> respect to the thing which judges, it is in relation to some one partic-

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[9]Tennekes noted that, as of 1971, the only "extensive analysis of the origin of the cultual-relativistic way of thinking and the various twists given to it by different anthropologists" was to be found in German [Rudolph, 1968]. This remains the situation. In particular, there are no monographs attempting a full-scale scholarly history. The paucity of histories of anthropology is one manifestation of lingering anti-historicism.

[10]I have used Popper's translation [1963, 12].

ular animal or man or sense that each object appears, and in relation
to such and such circumstance; and with respect to the concomitant
percepts, each object appears in relation to some one particular ad-
mixture or mode or combination or quantity or position... And even
he who asserts that not all things are relative confirms the relativity
of all things, since by his arguments against us he shows that the very
statement "not all things are relative" is relative to ourselves and not
universal (Sextus I 136, 139).

It is tempting to think of radical relativism as radical *tout court.* But if no
transcendental adjudication is possible, and if there is a natural propensity towards
the outlook in which one is raised, then relativism is easy to reconcile with the
conservatism of, say, Burke, or Oakeshott.

What about historicism? By and large, over its short history, anthropology
was initially weakly historicist, then firmly, even vehemently anti-historicist, and
then, finally, its opposition weakened and a new historicism was taken up (weak
and strong). Post-colonial anthropology, for example, could hardly be articulated
without history. Equally, the fusion of opposition to history with strong histori-
cism is self-defeating, which shows the tension inherent in the issue. If we begin
at one of the favored starting points of modern anthropology, the Scottish En-
lightenment, we can construct a story with the following simple shape. The major
Scottish Enlightenment figures, Lord Kames, Lord Monboddo, and Adam Fergu-
son, not to mention Adam Smith and David Hume, were historicists in the weak
sense [Broadie, 2003]. That is, they used the history of society and its institutions
to explain its present state and diversity. It was the idea of progress that kept
them from falling into historicism in the strong or relativist sense. For the Scottish
Enlightenment history — read as a story of progress - functioned as a means of
dealing with the facts of human diversity without embracing relativism. That is,
to avoid treating the descriptive facts as of equal significance and hence as pointing
towards normative relativism, one can rank the descriptive differences and then
argue that this or that system of morality gives rise to a better developed one that
replaces it; much as it is the standard view that each theoretical system in science
gives birth to a successor system that embraces and explains all that went before.
The stories told by the Scots thinkers were evolutionary; that is, from simple be-
ginnings things had developed in progressive ways. Their evolutionary vision was
much elaborated in the nineteenth century, especially after Darwin. Towards the
end of that century, though, the reaction set in with Franz Boas in the United
States, followed, in the early twentieth century, by Bronislaw Malinowski and Al-
fred Radcliffe-Brown in the British Commonwealth, all of whom were suspicious
of evolutionary stories, since they seemed self-serving. Thus, by extension, they
became suspicious of weak historicism as such, namely of the idea that present
conditions are given to historical explanation. They were correct on the logic (by
itself the past cannot explain the present) but on shaky ground in thinking that
what was wrong with historical explanation was that it was conjectural and not
scientific [Jarvie, 1964]. This aversion to conjectural history found its strongest ex-

pression in the structuralism of the mid-twentieth century. Evolutionism was kept alive by Marxists. French authors, especially Michel Foucault, sought to renew Marxism by reinventing the *Zeitgeist* as *epistemes*, that is, temporally specific, fundamentally structuring bodies of knowledge or ways of knowing. Iggers writes:

> More recently the term "New Historicism" has occurred in American literary discussions. . . .They seek to overcome the suppression of the subject and of history in structuralist and poststructuralist thought. They share the postmodernist rejection of historical optimism as it was contained in both German historicist and Marxist thought, but urge a recognition of the "historical and cultural specificity of ideas" largely lost in postmodern thought [Iggers, 1995, 137].

As Mah puts it:

> A term coined by the American English professor Stephen Greenblatt, "New Historicism" seeks to coordinate a period's diverse cultural phenomena as expressions of the circulation of political power, and in this sense is principally inspired by the work of Michel Foucault [Mah, 2002, 163, n. 4].

Let us turn from the longer view of history to look at a source that treats the history of relativism as quite short. We refer to *A Dictionary of the Social Sciences* edited by Julius Gould and W.L. Kolb. This is a 1964 reference book produced under the auspices of UNESCO, edited by two American scholars and drawing on a panel of contributors that includes many of the big names in the social sciences of the time. First, a significant structuring absence: there is no entry on cultural relativism, only on cultural relativity. Secondly, this is not written by Melville J. Herskovits, author of the major 1955 essay already discussed. Rather it is written by Clyde Kluckhohn, patenter of the rival phrase, "Cultural relativity" [Kluckhohn, 1955]. Historical remarks in the entry date cultural relativity back to Franz Boas and its strengthened relativist version to his students R. Benedict and M. Mead. In the *Journal of Philosophy* Kluckhohn, as part of a symposium of three social scientists, had suggested that cultural relativism in its extreme form had been modified under the impact of scientific criticism. To capture this, his preference is to use the word "relativity" to embrace both the strong and the weak versions. He specifically asserts that:

> Few anthropologists[11] would today defend without important qualification Ruth Benedict's famous statement: "...the co-existing and equally valid patterns of life which mankind has carved for itself from the raw materials of existence." In part, I think we must admit, the abandonment of the doctrine of untrammeled cultural relativity is a reaction to

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[11]In a footnote Kluckhohn writes, "M.J. Herskovits is a partial exception; cf. *Man and His Works*, New York, 1948, 76–77".

the observation of social consequences. If one follows out literally and logically the implication of Benedict's words, one is compelled to accept any cultural pattern as vindicated precisely by its cultural status: slavery, cannibalism, Naziism, or Communism may not be congenial to Christians or to contemporary Western societies, but moral criticism of the cultural patterns of other people is precluded. Emotionally and practically, this extreme position is hardly tolerable — even for scholars — in the contemporary world [Kluckhohn, 1955, 663].

Hence his substitution of the word "relativity" for "relativism" — to jettison associations with extremism. Thus the picture of the history given in his *Dictionary* entry is that Boas's students articulated a strong version of cultural relativism, but that by the mid-twentieth century there was some retreat. In the earlier piece Kluckhohn argues that interaction between sociology, psychology, and anthropology was also partly responsible for rectifying the somewhat unbalanced interest in cultural differences as opposed to cultural commonalities. The search for cultural universals was, obviously, an effort to preserve the idea of the unity of humankind in the face of the evidence of diversity. The most obvious universals, the biological, were seldom appealed to in the social sciences because of their connection to evolutionist, social Darwinist, and other views thought to be pernicious.

If the mid-century did indeed see a retreat from untrammeled cultural relativism, what, then, happened? After all, relativism and historicism are alive and well, even flourishing. One obvious factor is that the Harvard experiment in integrating three of the social sciences (sociology, anthropology, and psychology), to which Kluckhohn alludes, did not flourish [Kuper, 1999, 81]. Possibly the career of Clifford Geertz offers a clue. A product of the Harvard program in Social Relations that sought to unify the social sciences, Geertz changed career course a number of times [Geertz, 1999; 2002; Kuper, 1999, ch. 3]. Beginning in English literature, Geertz describes almost as a chapter of accidents how he ended up doing fieldwork in Indonesia as part of a failed team project. Soon he was a leading ethnographer of the Islamic cultures of Indonesia and Morocco. Appointed to the Institute of Advanced Study, relieved of all teaching duties, Geertz increasingly focused on producing essays. He endeavored to explain anthropology to the rest of his culture, and to treat culture as an independent variable, in particular, independent of all social organization and social structure. Somewhere in there he became more interested in anthropology as a literary rather than a factual and scientific enterprise [Kuper, 1999, ch. 3]. His outreach activities were immensely successful and influential. Kuper writes:

> Geertz must surely be taken seriously as a theoretical influence. He has written with great eloquence about a particular idea of culture; he has applied this idea to the analysis of particular cases; and, in the process, he has given the cultural approach a seductive appeal, exciting the interest of many who would otherwise be quite indifferent to anthropological writings [Kuper, 1999, 76; see also 118].

Geertz's influence inside and outside of the social sciences makes clarification of his stance on relativism piquant. Although he has always refused to affirm cultural relativism, many of his readers have found it in his writings. Others, who could not find it, insisted that it should have been there (cf. [Clifford and Marcus, 1986]). In an effort to set the record straight he devoted an invited address to the AAA in 1984 to the issue. His title was "Anti anti-relativism". Logicians equate double negation with affirmation, but the prefix "anti" is not quite a negation. Geertz invites us to read "anti anti" as an exercise in suspension of judgment, as a refusal to endorse relativism and a refusal to reject it. Yet he is unable to stay suspended. He ends up disparaging those engaged in "placing morality beyond culture and knowledge beyond both. This ... is no longer possible" [Geertz, 1984, 276]. As Gellner points out, this is a perfectly acceptable definition of relativism [Gellner, 1992a, 54].[12] Geertz endorses relativism despite himself.

No discussion of relativism in anthropology and sociology can leave out postmodernism, a vaguely demarcated movement of thought that rose to academic prominence in the final third of the twentieth century. Postmodernism dispensed with what it saw as the naïve certainties of modernism, especially the idea of progress, the idea of truth, and the idea that we could ever achieve stability of meaning in texts and societies. The movement was strongest in literary criticism and the social sciences (excepting economics), noted but not everywhere taken seriously in philosophy. Struggling to articulate some crisp tenets to make discussion possible Gellner tried the following:

> The notions that everything is a 'text', that the basic material of texts, societies and almost anything is meaning, that meanings are there to be decoded or 'deconstructed', that the notion of objective reality is suspect — all this seems to be part of the atmosphere, or mist, in which postmodernism flourishes, or which postmodernism helps to spread [Gellner, 1992a, 23].

Despite the difficulties of pinning down the reference of this widely used term, postmodernism, the relativism it insists upon towards cultures, moralities, and world-views is complete. In the shifting field of meanings neither the self nor the other is knowable, hence for anyone to sit in judgment on anyone else, cognitively or morally, is intellectually indefensible; hence practicing it is a form of disrespect and even of oppression. It is under the auspices of postmodernism that relativism spread like a pandemic beyond the boundaries of American anthropology and its sphere of interest into the western academy more generally [Boudon, 2005]. The emphasis on the instability of meaning does not inhibit the assertion of normative relativism. It is also poignant because relativistic claims can be made, or, more often, insinuated, but all modes of discussion and criticism are deconstructed

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[12]Gellner notices that Geertz's phrase "no longer possible" frustrates his audience's desire to be clear about his positon, since it implies that once relativism was possible when in fact the logical, factual, and moral objections are so strong that they do not permit any prior license (see section 6).

as positivistic, phallogocentric, Orientalist, or simply imperialist/colonialist. By these means postmodernism renders relativism into a reinforced dogmatism, that is to say, a doctrine that insulates itself against all possible criticism [Popper, 1945, ch. 23].

5 FURTHER ANALYSIS OF RELATIVISM

Combining Pascal and Herskovits we can find in relativism two different claims about norms, one factual, the other logical. The factual claim is that any adjudicating norm must in fact arise in a society, a culture, or an epoch to which it is relative. Hence its use to adjudicate is biased as a matter of fact. The logical claim is stronger: no adjudication is possible because no norm *can be* without social, cultural, or epochal roots. Hence any adjudication of differences based on it is *necessarily* biased. So although our analytical distinction between descriptive and normative relativism seems at first simple and straightforward it encounters complications. These will be elaborated further, especially in section 8.

Another approach to this point is to note that when anthropologists describe differences of norms they also report *en passant* that each of those sets of norms is held absolutely. Descriptive differences are always evaluated. It is a descriptive fact that most societies and cultures consider their views and values absolute; most cultures do not relativize. Or, more precisely, most cultures relativize the differences between other cultures whilst providing an escape clause for the absolute validity of their own judgments. Relativism without such an escape clause is a meta-claim that derives from the culture of scientific anthropology. Relativists thus put themselves in an awkward position. Societies and cultures usually resolve conflicts of views and values by declaring their own to be correct and those who differ from them to be mistaken. This forces the relativists, trying to be descriptive and uncomfortable with the transcendent, to take a transcendental evaluative position: that any absolutist resolution of conflicts of views and values is itself erroneous. Wanting to reject transcendental evaluative claims, relativists articulate one despite themselves. To put this more sharply: a consistent descriptive relativism almost imperceptibly shades into evaluation either by refusing to evaluate (treating all differences as equal — the Boasian compromise) or by insinuating that the differences and diversity negate evaluation as such (all evaluation is mistaken).

Absolutists have no problem with the factual normative claim, as they can recognize the differences described while seeking for the absolute. The logical normative claim is the one they have to meet. Relativists have a number of options for dealing with Pascalian bias. Herskovits and AAA claim that there is a natural preference for the judgments of one's culture of origin. Relativism seeks to correct this. Another is to blame our language for being unavoidably misleading and/or biased. Epistemological or cognitive relativism is the result. Still another is to try to shift the discussion from what this or that relativist is for to what they are against. This move opens up the possibility of attacking motives (see section 8.7).

Relativists find it important and desirable to shock. This is too often overlooked. If there were no anthropologists engaged in what amount to apologetics for "ye beastly devices of ye heathen" (to use Malinowski's ironic phrase) the issues would never arise. After all, that dependent variables exist is scarcely news. Market prices are a dependent variable, even the price of a standard-setting commodity such as gold. Those who take account of this in their everyday dealings are not labeled relativists. The shock comes from the effort to debunk a putative absolute, that is, to relativize what was hitherto deemed an independent variable. Claims that something or other just is morally right or wrong, that such and such an assertion just is true or false, it is these claims which relativists usually debunk. They shock the parochial and offer what seems a sophisticated, cosmopolitan, even radical *aporia*. This is offered as an antidote to the dreaded disease of ethnocentrism. Much of the frisson undoubtedly comes from the guilty realization that an antidote was needed in the first place. Advocates of relativism vacillate between a passionate earnestness to bring enlightenment and a nonchalant knowingness, not unlike the way parents roll their eyes when children talk seriously of fairies or Father Christmas.

It might be the frisson that leads us to a logical mistake. Descriptive relativism does not entail normative or epistemological relativism. Hence we can deny the latter and affirm the former without contradicting ourselves. Norms are not derivable from facts, not even from facts about norms. Obviously, claims of morality or of truth do vary across social, cultural, or historical boundaries. It does not follow from this that morality and truth are necessarily functions of those factors, or that they ought to be. Equally obviously, people usually follow the moral system or world view they inhabit. It does not follow that they should do so, or that the moral system or world view they inhabit is moral or true.

Consider a claim like the following:

(a) "The so-and-so people hold that it is moral and just to beat one's children"

(b) "The such-and-such people hold that the earth is flat"

Of these claims it is possible to ask, "is (a) a correct moral judgment?" That it is descriptively accurate to the facts of the so-and-so people is not an answer but a fudge. In parallel manner it is possible to ask, "is the final phrase of (b) true?" That it is held to be true by the such-and-such people does not make it true. These questions shift us to the meta- or the transcendental level. This is entirely in order. Those who dispute what the late John Paul II taught about the morality of artificial birth control do not dispute that he held what he held. They deny that he was correct. This again is not the same as asking whether his was a correct reading of Catholic doctrine. For even if he correctly held that Catholic doctrine required the finding that artificial birth control is immoral it does not follow that it is immoral. The inference from "the Catholic church holds this to be immoral" to "this is immoral", is not valid, unless we add a premise like: "all practices the Catholic church holds to be immoral are immoral". Such a premise

would be rejected by many persons of many cultures and rightly so. Descriptive relativism does not entail normative or epistemological relativism. Its results are undamaged by the criticisms of the other two.

As Gellner notes, there are many ways to classify the varieties of relativism besides the simple dichotomy between the descriptive and the normative [1988]. Pascal's list from the seventeenth century has meanwhile been much enhanced. For example there is classification by means of the units to which reference is made (epochs, cultures, races, nations, social classes, ethnicity, individual variation, and even states of an individual); there is classification by kinds of judgment (aesthetic, moral, political, metaphysical, religious, scientific, theoretical, conceptual, even perceptual); and there is the kind of connection asserted to hold. Combination and recombination of all of these could yield an immense number of variants. Few of them have been realized.

Turning now to sociology, let us consider the sociology of knowledge in its weak form and its Strong Programme variant. Like cultural relativism, the sociology of knowledge rests on old ideas. Already in Plato there are passages where someone's ethnicity, class, gender, etc., is used to explain the positions taken. Modern sociologists are more disciplined. They proceed from Marxist/materialist premises and hold that the determining variable of claims to knowledge is social class. This rests on some brief passages in Marx where he suggests that what the bourgeoisie takes for granted differs from what the proletariat takes for granted. In later Marxist parlance, a class naturalizes its own outlook. That is, it takes it to be self-evidently true, part of the natural way of looking at things. The echo of Xenophanes is unmistakable.

Provided the sociology of knowledge is not formulated carelessly as a quite general and hence reflexive and paradoxical relativism, it offers some fruitful directions for research. The colloquial phrase, "he would say that, wouldn't he" indicates that it is common sense that people self-servingly hold that their views are correct. A major critic of the careless and reflexive formulation of the sociology of knowledge, Karl Popper, was nonetheless deemed by three major sociologists to have produced a "fine" example of it free of these defects in his study of Plato's sociology [Hedström, Swedberg and Udéhn, 1998, 359, n.2]. Popper related Plato's ideas, his emotional anxieties, and his style of writing to his biography and to the recent history of his city, Athens. Popper brings out the impact that war and the struggle for power had on the social class to which Plato belonged, as well as on the fate of his teacher, Socrates. Yet Popper did not aim thereby to explain away Plato's ideas. Rather, he treated them with the utmost seriousness and assessed their moral value and their truth. Popper used his sociology of Plato to explain the appeal of those ideas to someone of Plato's background, and the traces this appeal left in his writing, and especially his rhetorical and argumentative tactics.

Avoiding reflexive and paradoxical formulation is not easy, however. One may easily fall into it because of failure to appreciate the pervasiveness of one's own "total ideology", that is, the system of opinions and theories one takes for granted. Unless sociologists of knowledge somehow exempt their own claims to knowledge

from analysis they inadvertently class them as manifestations of just one more total ideology. Popper fastened on this weakness. The advocates of the Strong Programme agreed that he had found a weakness (without acknowledging him). They proposed a solution. It was not, however, to avoid reflexive formulations but on the contrary to insist upon them for reasons of consistency. A science of knowledge cannot exempt its own claims to knowledge from scientific investigation. This reflexive principle is controversial. The advocates of the Strong Programme have insisted that there is no alternative to it and that the consequences must be embraced [Collins, 1981]. One of the aims of being consistent is to place science and its institutions alongside all other institutions for scientific study. Discussion of these matters is invariably bedeviled by the debunking aspect of the Strong Programme. It says: No sociological privileges for science. That is a loaded slogan. But if science is being debunked then so, implicitly, is the Strong Programme as part of it.

In constructing arguments for the Strong Programme its advocates claimed to be drawing on Ludwig Wittgenstein (his later philosophy) and on Thomas Kuhn [1962], the historian of science. They parted ways with the classic sociology of science of Robert K. Merton [1973]. Merton's work was held up as an example of what happens when the older, weak version of the sociology of knowledge is followed. Merton's sociology of science was derided as being apologetic as much as it was sociological [Fuller, 1997, 63–67]. By contrast, the Strong Programme and its associated sociology of science spawned an exciting new ethnography of science that treated science in the same way that investigators treated any other complex of social institutions. Critics of this work focused less on what it described than on the tendency of ethnographers to debunk the mystique of science. It is instructive to contrast, say, Latour and Woolgar's *Laboratory Life*, first edition [1979], with James D. Watson's *The Double Helix* [1968]. Both offer much informative ethnographic detail, both cultivate the slightly shocked reaction. Yet Watson never for a moment suggests anything other than that science is hard work, requires imagination and dedication, and is really about something other than itself: it is in quest of truth. By contrast, the Latour and Woolgar effort made waves just because it deviated from tradition on the central matter of truth and because it demystified the heroic by assimilating the laboratory to bureaucracy.

Like the sociology of knowledge, the social construction of reality was not originally put forward in a strong, relativistic formulation. It was a generalization of the weaker version of the sociology of knowledge, namely that there were systematic patterns in what people took to be knowledge and what they took to be real. After all, these claims had clear roots in Weber and Durkheim, two firm believers in sociology as scientific investigation. The animating idea of the social construction of reality was simple: to de-naturalize actors' taken-for-granted view of the world, to show how it was constructed in the social matrix as part of socialization, and how it was sustained by social and traditional structures. Two books by philosophers, more than twenty years apart, used it without flirting with relativism [Jarvie, 1972; Searle, 1995]. The postmodern claim is that societies do not

just resemble texts but are texts (see section 7). This claim could be seen as one of the many strong and relativistic formulations of the social construction of reality. What it comes down to is the idea that reality is *nothing but* a social construction. That we learn it and hence construct it as part of our social upbringing is not a relativistic thesis. That the reality we inhabit is *nothing but* a social construction has overtones that suggest a charade or a theatrical façade. But this metaphor will not really do, because behind a façade there is something solid, something real. The relativist version of the social construction of reality is that the socially constructed reality is all there is.[13].

6   ANALYSIS OF HISTORICISM

Historicist relativism is simply that variety of relativism that uses time and place as the variables with which morality and truth vary. It is included in the examples given by Pascal. As Webster's *Third International Dictionary* puts it, historicism is:

> a theory that all sociocultural phenomena are culturally determined, that all truths are relative, that there are no absolute values, categories, or standards, and that the student of the past must enter into the mind and attitudes of past periods, accept their point of view, and avoid all intrusion of his own standards or preconceptions.

We can borrow the dichotomy between weak and strong versions of potentially relativistic positions and apply it to historicism. For, this dictionary entry notwithstanding, the word has two principal meanings, stronger and weaker, and one specialized meaning.[14] Only one of these three is relativistic. This dictionary entry then captures a common use of the term to denote strong historicism

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[13]Deliberately set aside is the ticklish question of the stance, *vis à vis* relativism, of the great ethnographer Erving Goffman, doyen of the dramaturgical approach.

[14]Iggers captures the third meaning like this. "Karl Popper in *The Poverty of Historicism* identified the term with the attempts by Hegel and Marx to formulate laws of historical development which were used by the Marxists to legitimize their authoritarian control for eschatological ends. Popper's use of the term has been severely criticized as idiosyncratic, but in fact he distinguished between "historicism" (*Historizismus*) and "historism" (*Historismus*) in the German sense at a time when "historism" was still the current term in the English-speaking world. Only in the 1940s, under the impact of Croce's *storicismo*, did "historicism" normally replace "historism" in English" [Iggers, 1995, 136–37]. Popper remarked: "I have deliberately chosen the somewhat unfamiliar label 'historicism'. By introducing it I hope I shall avoid merely verbal quibbles; for nobody, I hope, will be tempted to question whether any of the arguments here discussed really or properly or essentially belong to historicism, or what the word 'historicism' really or properly or essentially means" [Popper, 1957, 3–4]. If Iggers is correct, Popper's hope fell victim to a shift in usage that more or less coincided with the publication of his major works. Although an important work on Meinecke was translated in 1972 as *Historism: The Rise of a New Historical Outlook* this choice of words was deplored by Harold Mah as one that "no one currently uses" [Mah, 2002, 163, n. 3]. To avoid confusion of weak with strong and specialized with general uses one has to attend especially closely to context where the word "historicism" is used.

but overlooks an equally common use of the term to denote the idea that history and historical explanation are important in the social sciences (weak historicism). Georg Iggers writes:

> In the last few years a considerable number of books and articles have appeared in Germany, the United States, and Italy on the topic of historicism. There is, however, no consensus in this literature on the meaning of the term. A number of writings have dealt with the so-called "crisis of historicism" in the context of the later nineteenth and early twentieth centuries. Here historicism has come to be identified with relativism and loss of faith in the values of modern Western culture. This relativism has been considered a permanent aspect of intellectual life under the conditions of the modern world. A very different literature has identified historicism more narrowly with the historiographical outlook and practices of nineteenth- and to an extent twentieth-century scholarship in the human sciences [Iggers, 1995, 129].

In the weak sense, historicism in anthropology and sociology means no more than an interest in historical explanations, especially a revival of interest therein.[15] The story in anthropology is a trifle intricate. Nineteenth century anthropologists (or proto-anthropolgists) favored schemes of socio-cultural evolution that enabled societies to be ranked by their levels of development. The reaction to this is often called "historicism" and took two main forms: diffusionism and historical or cultural particularism. Diffusionism used historical methods to try to account for social and cultural development by diffusion outward from one or more centers of innovation. This was seen as quite different from models of unilinear evolution. Franz Boas's cultural particularism rejected the grand narratives of both evolution and diffusion as unprovable, whilst remaining agnostic on particular cases. To study these, the anthropologist needed to focus on detailed regional studies where the processes of social and cultural change could be seen at work. This kind of data would provide the basis for sound inductive science. It also yielded the descriptive relativism that Boas's students extended into cultural relativism.

The word historicism was less used in the Old World, where the rejection of both grand and local historical schemes was widespread. In the heyday of British structural-functionalist anthropology, and that of French *structuralisme*, all historical explanations of social phenomena were disparaged. The argument was simple:

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[15]Problems of formulation are apparent in the following, where relativism is not intended but almost makes an appearance. The anti-historicist Pieter Geyl tells us that: '*Historicism,* this was the term that came into use for the apporach to history that was derived from [Ranke's] example. Minus the mystical urge, no doubt; but the abstaining from judgment, the accepting, the acknowledging of no other standards than those supplied by the historical process itself — these came to constitute the spirit in which history was studied. The great personages were seen as the exponents of impersonal forces, driven. *Fert unda nec regitur*' [Geyl, 1955, 21]. We need only add that in the received view Rankean historiography should be driven by primary sources, since only that way could the historian deal properly with past events.

the past was not an explanation of the presence or the functioning of extant so-
cial institutions unless one added some hypothesis that described the mechanism
of their survival. For example, the evolution of the institution of marriage as
a form of inter-family contract functioning to regulate descent, succession, and
inheritance is of little help in explaining why marriage functions as it does in
contemporary Western industrial societies. In some societies the institution may
incorporate those earlier features, but they do not explain its present functions
as, for example, legitimating serial sexual partners, or as a means of demarcat-
ing social respectability, even conservatism, from libertinism and bohemianism.
Controversy about same-sex marriage, to take a particular case, is not settled by
pointing to how marriage functioned in the past because the issue is how the insti-
tution can be adapted and extended to meet current social conditions. Arguments
that this is what the institution was, even arguments that this is what it always
was, do not explanatorily entail any current state of it and hence beg the question.

Be all that as it may, historicism was a desire to reintroduce genuine histor-
ical methods to the social sciences, especially under the rubrics of comparative
anthropology and historical sociology, since most of the social forces deemed to
be at work in the secular trend could only be identified with historical hindsight.
Historicism thus construed did not claim that history explained the present, but
rather that the forces at play in the present could not be identified without some
regard for the past. Weak historicism in this form is uncontroversial.

7 SOCIAL AND POLITICAL POLICY

Untrammeled relativism is taken by many to undergird social and poltical radi-
calism.[16] This is hard to understand and not only because, as we have seen, it is
easily made congenial to conservatism. As indicated above, a correct descriptive
relativism brings out that there is a diversity of absolutisms, not of disavowals
of absolutism. The nihilism implicit in normative relativism renders social and
political radicalism arbitrary and so lacking force. When Herskovits's essays on
relativism and associated topics were posthumously published [Herskovits, 1972]
they were introduced by his colleague, the psychologist and philosopher Donald
T. Campbell. The mode is defensive: Campbell affirms that the criticisms to
which Herskovits's work has been subjected are mistaken and can be resolved (not
a choice of word that shows much philosophical confidence) "by the recognition
that [Herskovits] was an opponent of the still too pervasive ethnocentric moral
absolutism" rather than a nihilist.[17] Thus is the moral high ground seized and
critics tarred with the labels of ethnocentrism and absolutism (see section 8.7).

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[16]This goes back at least as far as Boas. Sidney Hook reports that in the 1930s (Boas died in
1942) Boas was involved with Communist front organizations and was an assiduous follower of
the Party line, even down to sycophancy towards the USSR when his own anthropological ideas
were embargoed there [Hook, 1987, 257–59].

[17]The passage continues, "The message needs restating, both for the social scientist and for
the general public, as there are no signs of abatement in the cultural and moral arrogance of
those cultures with the greatest military and economic power." Dated July 1971, the allusion

Admittedly, no reflective social scientist can doubt that unthinking absolutism, and a kind of naturalized ethnocentrism are both quite deplorable and should be eschewed. The question for the defender of relativism is, however, to show that they can be opposed strongly from no other position than that of relativism. This the defender cannot do. Indeed, to deplore absolutism and to preach relativism is in turn ethnocentric since most cultures in the world are (unthinkingly) absolutist. If we are to correct our own moral errors and have dialogue with members of other cultures then we should not declare in advance that those differences are dependent upon non-intellectual factors.

Discussing 'cultural relativity' in 1955 Kluckhohn admitted that the retrenchment from untrammeled cultural relativism among anthropologists was in part due to perceived social consequences. He did not specify how those consequences had been impressed upon social scientists, but his examples gestured towards the past (slavery), primitivity (cannibalism), and politics (Nazism and Communism). Kluckhohn was an American social scientist, so the omission from his examples of racial segregation and prejudice, a salient feature of the social organization of much of the United States at that time (Myrdal 1944), may be telling (see section 8.3, below).

A similar puzzle is presented by the 1947 intervention of the American Anthropological Association into a major question of social policy being discussed on a world scale, no less. This was the publication, in *American Anthropologist*, of the "Statement on Human Rights", a document submitted to the United Nations Commission on Human Rights by the Executive Board of the American Anthropological Association [AAA, 1947]. The UN was in the process of drawing up its various declarations on human rights and the AAA wanted to have input. There are reasons to think that this document proved an embarrassment to the Association because it incorporated the untrammeled (or strong) Benedict/Herskovits position on relativism uneasily blended with the Harvard culture and personality doctrine favored by Kluckhohn. The advice in the document consisted of a number of claims said to be "findings of the sciences that deal with the study of human culture". The tone of some of the Statement was of warning. It would be easy for a declaration of human rights to be ethnocentric, especially if it focused on the individual: individuals can realize their full potential only in their own cultures, so cultures, not individuals, should be a primary locus for the protection of rights.[18]

This document may be taken to represent the apogee of self-confident cultural relativism. The United Nations was urged to defer to the findings of science and to frame human rights without being ethnocentric. Its centerpiece is the claim that individuals can only develop fully in their own cultures. In effect the authors

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is unmistakable: the war in Vietnam and other extensions of American military and economic power. Relativism is the antidote to this "ethnocentric moral absolutism". Campbell also claims that Herskovits was not a "thoroughgoing" ethical relativist nor an ethical nihilist. He does not provide any evidence, nor does he show how Herskovits could disclaim those positions his formulation of cultural relativism entails.

[18]Gellner comments on the parochialism of the original Declaration of Independence in [1992a, 52].

imply that cross-culturally adopted individuals cannot develop fully. They also imply that exiting from one's native culture must be damaging to one's potential, especially exit in the midst of maturation. Thus those who go into any kind of cultural exile, not to mention those who consciously choose to emigrate to another place with another culture, are to say the least putting themselves at some sort of developmental and perhaps identity disadvantage. Coming from the body representing the anthropologists of a nation most of whose population consists of the descendents of immigrants this statement cannot but seem to be very confused. (We shall return to this point in section 8.3.)

Another striking feature of the document is that it endorses all and any cultures. Its early critics already pointed out that in the drafting of it the authors were aware that not all cultures, and not all features of all cultures, deserved this endorsement [Barnett, 1948; Steward, 1948]. The authors tried to make exceptions, but the wording becomes convoluted:

> Even where political systems exist that deny citizens the right of participation in their government, or seek to conquer weaker peoples, underlying cultural values may be called upon to bring the peoples of such states to a realization of the consequences of the acts of their governments, and thus enforce a brake upon discrimination and conquest. For the political system of a people is only a small part of their total culture [AAA, 1947, 543].

Cultures with bad political regimes, it says, have "underlying values" that can "enforce a brake upon discrimination and conquest". These brake values are, presumably, in conflict with others, less underlying, and politics is only a small part of total culture. So cultures contain conflicts of values that are to be resolved by "realization of the consequences" of some of them; and politics is not the whole story.

What makes this Statement embarrassing is its incoherence, even confusion. It is an effort to speak with the authority of science to tell the UN to pay attention to the latest fad in the Harvard Yard — the integration of the social sciences. It notices that its own formulation endorses all manner of unpleasant and unthinkable regimes. It labels "ethnocentric" fundamental legal ideas about rights belonging to individuals first rather than communities. And, to cap it all, in three discussion notes published in the ensuing years all these problems were exposed and left unanswered [Steward, 1948; Barnett, 1948; Bennett, 1949]. It is difficult to read the subsequent silence by the Executive Board authors as other than embarrassment.

If we read the Statement closely, the embarrassment can be seen to be more than warranted. It advocates relativism while making non-relative moral and cognitive claims. We may ask whether the Statement makes any claims about morality. Yes, both negative and positive: expansion, conquest, exploitation, and missionary activity are all condemned there as "disastrous"; respect for different cultures is there enjoined. We may ask whether it intends these and other claims to

be taken as true: Yes: the findings of the sciences must be taken into account. The document then articulates the following three principles (italics in the original):

1. *The individual realizes his personality through his culture, hence respect for individual differences entails a respect for cultural differences.*

2. *Respect for differences between cultures is validated by the scientific fact that no technique of qualitatively evaluating cultures has been discovered.*

3. *Standards and values are relative to the culture from which they derive so that any attempt to formulate postulates that grow out of the beliefs or moral codes of one culture must to that extent detract from the applicability of any Declaration of Human Rights to mankind as a whole*

[AAA, 1947, 541–42].

These principles are mutually inconsistent and (3) is inconsistent with itself. They also raise the obvious question of the cultural grounding of the factual and moral claims preceding them, and answer it by appeal to the authority of "science." Is science a culture in the required sense? It has common technical languages, such as mathematics and chemical notation. It has a lingua franca, English, that is, if not universal, widespread. Otherwise, it is an international, world-wide network of local, regional, national, international institutions. It claims for its results, not just a world wide truth, but a timeless and universal truth. (Although the Strong Programme objects to this characterization of science, they are far from carrying the day.) If it is a culture it is not a relativistic one.

8 CRITICAL ASSESSMENT

Relativism (including its cultural, historicist, Strong Programme, social construction of reality, and rationality versions) is seen by its proponents as a benign, even obligatory, influence on anthropological and sociological theory and practice, an influence for good elsewhere; by its critics it is seen as pernicious. In this section we will find that the doctrine cannot survive scrutiny and so needs to be explained as itself a product of a particular (sub-)culture rather than as a scientific truth disclosed by social science research.

There is, as indicated earlier, a very large literature on relativism, relatively little of it expository, the great bulk of it critical to one degree or another. If we bring together the various critics, Williams [1947], Schmidt [1955], Moser [1968], Tennekes [1971], Hatch [1983], Siegel [1987], Gellner [1988; 1992a; 1992b; 1992c], Harris [1992], Cook [1999], and so on, we can formulate a critique as follows.

In summary, cultural relativism will be shown to be a beguiling, misleading (8.1), and even incoherent (8.2) doctrine, that has enjoyed a prolonged vogue

(8.3) in twentieth century anthropology and its spheres of influence. Most of its adherents consider it to be factually true and morally exemplary (8.4). They are uncritical of it because of the fruitfulness of its associated method (8.5), and its liberal policy implications (8.6). Critics of cultural relativism are counter-attacked with charges of denying the facts of cultural diversity (8.7) and/or of harboring illiberal tendencies (8.8). In truth, only by repudiating cultural relativism can anthropologists come to terms with the world and the human predicament as they really are (8.9).

8.1 Descriptive Cultural Relativism is Misleading

As previously noted, cultural relativists describing the diverse views on reality and morality of the peoples studied need to avoid giving the impression that their subjects are cultural relativists. On the contrary, most religious communities, and most tribes and tribespeople, like almost everybody else, are not culturally relativist. Devoutly Muslim, Christian, and Jewish societies, for example, are, to the contrary, convinced that they possess *certain knowledge* of how the world is and the *true morality* as revealed to them. Less developed societies and cultures, of the kind that anthropologists formerly specialized in, are equally invariably convinced that their knowledge of the world and of moral value are absolutely correct, and that those who differ from them are in error. With very few exceptions, this is true of all well-known anthropological subjects, including the Coorgs, Berbers, Bushmen, Kwakiutl, Navaho, Nuer, Swat Pathans, Trobrianders, Tuareg, Yanamamo, Yir Yiront, and so on.

Another problem that cultural relativism faces is also ethnographic, namely, whether the doctrine that, "what is real and what is moral are relative to culture", is a tenet of the culture of the *anthropologists* themselves, the one from which they come and which they address. Given what was said above, the answer has to be negative. The polemical and persuasive rhetoric in the discursive cultural relativist literature testifies that it expresses a minority view in the culture that it addresses. The question can also be pursued more narrowly. Anthropology itself derives from a specific tradition in European culture, namely the Enlightenment culture of science. Science has always viewed its claims as transcending all culture. If anything, the tradition of science was one of constantly confronting and attempting to change the views of reality that sustained and were sustained by the culture surrounding it. Recall Descartes in 1637 contemplating the cultural diversity he had observed and writing that many things "although they seemed very extravagant and ridiculous to us are nevertheless commonly accepted and approved in other great nations; and so I learned not to believe too firmly in anything of which I had been persuaded only by example and custom" (*Discourse*, I, 10 (Cottingham)). And again, "Thus it is more custom and example that persuade us than certain knowledge, and for all that, the majority opinion is not a proof worth anything for truths that are a bit difficult to discover" (*Discourse*, II, 16 (Cress)).

It is easy to underestimate the reach of this objection. For anthropologists to adopt a relativist stance towards their objects of study is for them to distort in fundamental ways the ethnographic facts about all scientific endeavor, including their own. For them to adopt a relativistic attitude to their own work and results is to abandon the goal of scientific study, that is, to abandon allegiance to the culture that produced anthropology and in so doing to abandon anthropology as science. People may choose to call what they do anthropology whilst admitting that all they have to offer is local truth, i.e. folk wisdom, i.e. fiction (Leach 1989). The culture of the Enlightenment from which the tradition of anthropology stems would classify this as disingenuous.[19]

If the usual anthropological subjects are absolutists and the culture of science is absolutist, can there be a descriptive relativism that does not blur towards the normative? The fact that there are multiple absolutisms and that they differ from each other provides no support for relativism. Here is the difficulty. Descriptive relativism usually reports two variabilities: judgements vary and so do the norms upon which they are based. To maintain the descriptive/normative distinction one would have to separate the norm actually applied from the norm that should have been applied. But what are we to make of norms not seemingly applied? The anthropological scientist describing the differing views on cause and effect of different societies can hardly endorse all of them. Not to endorse any, including one's own, would be to dissemble, given the project in hand.

What of the anthropological scientist who tries to suspend judgment? If there is no meta-norm that can sit in judgment on all this cognitive diversity (and still more if the possibility of any meta-norm is denied) then it is easy to give the impression that the first-order judgments being described are valid simply in the absence of a second order norm which could decree them invalid. Descriptive relativism is made normative by a *faute de mieux* argument. Recognition of this creates for the anthropologist a crisis of scientific conscience. Some are content to abandon the aspiration to science, usually by disparaging the claims of science as such, and offer their work as history [Evans-Pritchard, 1962] or as a literary exercise [Geertz, 1988; Leach, 1989]. Fiction has been suggested as one template; an obvious alternative is the travel book, especially the modern travel genre in which the author can be as much the subject as the ostensible subject. This abdication of descriptive claims is not the end of this objection.

Ernest Gellner developed, since 1965, another argument to show the falsity of descriptive cultural relativism. In so far as cultural relativism treats different cultures as on a par and as having symmetrical relations it is deeply misleading. What Gellner has in mind is the Big Ditch: the utter transformation of the previous situation by the cognitive break-through to science and technology in early modern Europe:

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[19]Admittedly, in the late twentieth century relativism won some adherents among philosophers of science. This altered the status of the aim of science: there was no longer a consensus but a new area of dispute. Cultural relativists cannot consistently take part in this dispute because they relativise truth and the rational pursuit of truth.

> The existence of trans-cultural and amoral knowledge is *the* fact of our
> lives. I am not saying that it is *good*; but I am absolutely certain that
> it is a fact. It must be the starting point of any remotely adequate
> anthropology or social thought [Gellner, 1992a, 54].

Gellner argues that the true anthropological situation comes about because
in the Axial age certain cultures succeeded in disentangling the transcendental
from the social and indeed proceeded to use the transcendental to sit in judgment
on the social beyond any one community, polity, or ethnic group. This socially
disembodied religion was the precedent for socially disembodied science. One
post-Axial culture was possessed of the idea of the uniqueness of truth, of its own
religious faith. Rationalism was the continuation of exclusive monotheism by other
means, if Weber is correct. The outcome was natural science.

Gellner's descriptive ethnography of natural science stresses four points. 1.
That the claims made by natural science are translatable without loss of efficacy
into any culture and any milieux. 2. In its applied or technological form this
new knowledge has totally transformed the human social condition and the terms
of reference under which humankind lives. 3. In its internal organization the
new learning which makes the new social order possible is both cumulative and
astonishingly consensual. 4. This new learning respects neither the culture, nor
the morality, of either the society in which it was born or those in which it makes
itself at home by diffusion. It is, most emphatically, 'beyond culture and morality'
[Gellner, 1992a, 57–60]. It is science and its associated technology that shapes the
modern world. Relativism is a form of denial.

We shall revisit this in section 8.9.

8.2   *Cultural Relativism is Incoherent*

There is incoherence within cultural relativism between its moral relativism and
its cognitive relativism. Moral relativism says:

> (MR) there can be no transcultural moral assessment of moralities.

Cognitive relativism says:

> (CR) claims to knowledge, such as (MR), can only be locally assessed,
> relative to a culture.

(CR) renders it impossible for (MR) to be a culture-transcending result of scientific
anthropology; it makes (MR) locally true and generally false, hence false.

Cognitive relativism is itself internally incoherent: "Is reality...not defined and
redefined by the ever-varied symbolisms of the innumerable languages of mankind?"
wrote Herskovits. Call this strong cultural relativism (SCR). Either (SCR) is a
reality-claim subject to the symbolisms of the innumerable languages of mankind,
i.e. false sometimes, i.e. false; or (SCR) transcends such limitations and it is true.

If it is the first it is no more than a report on the false outlook of the anthropological tribe; if the latter, (SCR) is a counter-example to itself. So to state (SCR) is either to say something of no philosophical interest or to contradict oneself.

More generally: "all judgments of morality or truth are relative to culture" is a judgment, call it (J). We may now construct a dilemma: Is the truth of (J) relative to a culture? If the answer is yes, then, since (J) is relative to a culture, (J) is not universally or absolutely true. If it is not absolutely true that all judgments of morality and truth are relative to culture, the possibility that there are judgments true independently of culture is not closed and (J) may be absolutely true. If the answer is no, then (J) is a case of a true judgment not itself relative to culture. The existence of one case opens the possibility of a class of such judgments. Thus the very attempt to formulate (J) opens rather than closes what it tries to forbid. Thus cultural relativism fatally affects its own assertion: it cannot be coherently formulated. To say that values are relative to cultures confuses culture with value. Values are used to measure cultures, including the culture that gives birth to them. If values cannot transcend cultures how can cultures engage in self-assessment? When we judge the reality-claims, or moral standards, of our own culture to be wanting, what sort of standards are we invoking? They cannot be merely the "orientations of a given people at a given period in their history" simply because it may be the given orientations of this period that are being challenged in this period. When such criticisms are made, when, in our own society, people ask us to reform our moral outlook (as countless religious and ethical teachers have urged, from Socrates forward), or to be more scientifically realistic (as Galileo did in the *Dialogue Concerning the Two Great World Systems*), they challenge rather than accept the "given orientations". According to cultural relativism this cannot be done; the web of enculturation is inescapable. This claim is as though one were to argue that because humans are a product of their genetic inheritance and their cultural upbringing they can never have a new or independent thought of their own or assess their own conduct, or assess their own means of assessment of ideas or of conduct.[20]

8.3   Explaining the Vogue of Cultural Relativism

Given the manifest ethnographic distortions and logical incoherence of cultural relativism, explaining its vogue poses a profound anthropological problem. Is its popularity in anthropology a question of *credo quia absurdum*? That is, is subscription to it a test of faith, a condition of membership? In so far as its opposite is seen as ethnocentrism ("provincialism" as Geertz 1984 labels it) then the answer is of course "yes". Rejecting ethnocentrism is a mark of the anthropologist.

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[20] Joseph Agassi writes (private communication 23 April 2005): "It is very easy to rectify the incoherence of relativism by the use of the postulate of levels of discourse and by the ascription of absolute truth to some higher-level assertions, especially to the denial of the possibility of the ascription of absolute truth to any lower-level assertions. Obviously, this is formally impeccable and semantically suicidal: if higher-level assertions can be absolutely true, why not some lower-level ones, at least the tautologies among them".

So, subscribing to cultural relativism is a necessary condition for admission to membership of the guild.

The usual argument for rejecting ethnocentrism is that it is parochial, i.e. particular, i.e. unscientific. By contrast, a cultural relativist anthropologist seeks to be universal, i.e. scientific. Discovery of the incoherence of cultural relativism, however, creates a crisis of faith and leads some to hold that in order to escape ethnocentrism we must sacrifice the very idea of approaching society in a scientific manner. All that anthropology can possibly consist in, on this despairing view, is endless contextual iteration and description (Boasian particularism without hope). At times Clifford Geertz seemed to practice this [Geertz, 1973; 1983] but he writes cagily and has been criticized for that by some of his students (as reported and discussed by Leach [1989, 141–42]; and Gellner [1992a, 40–43]). Those who look to anthropology for more than a catalogue of exotica will find this kind of anthropology intellectually lacking. If we are not to explain society and its features by the use of generalizations then of what possible intellectual interest is anthropology? The more radical of Geertz's critics suspect even the descriptive project of ethnocentrism and propose that anthropology consist of reflexive textual analyses of attempts to do anthropology [Boon, 1982; Schweder and Levine, 1984]; Rabinow in [Clifford and Marcus, 1986]; see also [Geertz, 1988; 2002]). The rhetoric of anthropology, rather than social and cultural fact, becomes the leading concern of anthropology.

Perhaps the above account is itself too particularistic and needs to be more anthropological. A British dictionary of anthropology from the 1980s makes a thought-provoking point this way. Cultural relativism is an "approach or theory in anthropology associated with students and followers of Boas in North America" [Seymour-Smith, 1986]. Instead of treating cultural relativism as an intellectual item, as a philosopher is prone to do, that dictionary hints that we should treat it from the point of view of the sociology of knowledge and ask, who holds this set of ideas and what is their interest? That cultural relativism is most clearly articulated first in the United States and was most fiercely defended there is not under dispute. That articulation was, however, outward-looking and was linked with American scholars' sympathy with their subjects of study (Boas himself was an Amerindianist). The evolutionist assumptions of so much nineteenth century social thought invited sharp opposition. At the time of the "Statement on Human Rights" the anthropologists were in line with public policy in deploring the conquest of weaker peoples as the U.S. government strongly backed decolonization by the European states. Cultural relativism was a robust alternative to the rationalization that the colonizers were spreading the benefits of civilization.

Yet there is something missing. The Statement seems not to notice that it provides a legitimate defense for the Jim Crow culture of its own South; and Kluckhohn, writing of the social consequences, points to slavery but not racial discrimination. It is hard to imagine that these anthropologists were other than acutely aware of the shame of their own society. Indeed many of them were in the forefront of criticism of discrimination. Why then was the case of their own

country not worked through with regard to cultural relativism? When we add to this another uncontroversial point, namely that the United States has been called a country of immigrants, it becomes even harder to understand. The United States itself may not represent a culture in the sense required by cultural relativism and by the Statement on Human Rights. It has always been multicultural, whether one looked at it as a cluster of religious denominations, geographical regions, or as successive waves of immigrants, beginning with those who came across the Aleutian land bridge, or one looked at it as a slave-owning society that has partially cleansed itself, or at the waves of immigration of the later nineteenth, earlier and later twentieth centuries. There is an official language, which is one requirement of a culture. Absent is a shared history or a shared religion. There is, it is true, a limited array of shared rituals, what is sometimes known as the American civic religion.

We must face the question squarely: why was cultural relativism not applied reflexively to the United States, in all its social and cultural diversity? A plausible hypothesis might read cultural relativism as a means of coping with national shame. Anthropologists after Boas could not but deplore racial and ethnic discrimination, formal and informal. Other races and other ethnicities were entitled to equal treatment because they were just as good as anyone else considered as culture creating human beings. So far the reasoning is impeccable. The blunder was to formulate the claim in terms of "equal validity" (Benedict), or "standards are relative to culture" (AAA), or "enculturation" (Herskovits). For what these formulations overlook is that the oppressors and discriminators also constitute a culture *by the flexible definition of culture favored by anthropologists*. They thus handed to those they opposed a symmetrical, equally valid, claim they were helpless to defeat. Racists could defend "the Southern way of life". (Perhaps the most consistent use of the way of life argument was in the *Apartheid* period in South Africa.)

What brought about the defeat of the formal system of racism in the United States and later in South Africa? The Statement would direct us to the assertion of "underlying values" that became a brake upon the regimes. It might be clearer to dispense with the metaphor of what underlies what and to note that both of these societies were inconsistent with their own rhetoric and ideals throughout their periods of racial segregation and discrimination. That tension undoubtedly helped bring the regimes down. Yet this is far from the whole story. In both cases the actual downfall came about because transcultural values were asserted and, in the former, implemented with state force; in the latter backed by international sanctions. In the USA a series of constitutional court rulings and legislation led to confrontation with the forces that supported formal and informal racism and state power was used to bring about compliance. It could be claimed that this was a case of underlying values bringing citizens to realize the social consequences of the actions of their governments.[21] Aside from the difficulty this creates for

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[21]It is also notable that in the United States the formal defeat of legal segregation and discrimination did not end segregation and discrimination because the supporters of those institutions

the model of culture, discussed above, it is an inadequate analysis. In the United States the attempt to end racial discrimination was seen as a working out of some of the promises of the Declaration of Rights and the Constitution that had been compromised and fudged for almost two-hundred years. This reference back to the values supposedly embodied in old documents is hardly consistent with most formulations of cultural relativism. The cultural context in which those documents were written was utterly different from those that obtained in the modern, industrialized, large-scale society that was grappling with its own deficiencies. Indeed the treatment accorded those ancient documents much resembled that accorded Mosaic tablets stating, in the words of the Declaration, "We hold these truths to be self-evident. . . ." Self-evident truths, it needs to be said, are truths that transcend culture, class, *episteme*, *Zeitgeist*, and any other variable ever suggested.

The above points look at the American roots of cultural relativism. Yet it is popular far and wide especially, as indicated earlier, amongst students and radicals. This popularity is even broader in the era of postmodern relativism. The art historian E. H. Gombrich has suggested that there are several strands in this love-affair: the attractions of de-mystification, the fear of making mistakes, and the exaggeration of difficulties into impossibilities:

> It probably appeals to the young because it permits its followers to look down on the poor uninitiated who not only believe in Father Christmas and the stork but even in Man and in Reason. It adds a lot to one's self-respect if one has learned to see that all this is humbug, a fairy tale for children, which we have long outgrown. It is an opinion — I believe — which sounds doubly convincing because it is undeniable that in our reading of texts we inevitably run the danger of misunderstandings. Whoever is afraid of doing so can now comfortably withdraw into skepticism and dismiss any striving for understanding as naïve and obsolete [Gombrich, 1987, 690].

8.4   Why Cultural Relativism is Popular

Faced with the choice between cultural relativism and ethnocentrism, anthropologists feel bound to opt for cultural relativism. Ethnocentrism is the view that one's own culture and its values are to be preferred, perhaps as the only correct ones. It is the endorsing of custom and example. Since anthropologists study multiple cultures with multiple values they can hardly adhere to ethnocentrism and keep open minds. Without open minds it is hard to present other cultures sympathetically, in their own terms. In their struggle against ethnocentrism anthropologists see the need to endorse all cultures as valid forms of life, possessing internal coherence and rationality.[22]

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found ways and means to evade state force and continue some, at least, of past practices.

[22]A good example of how the relativist ethos came close to being an article of faith (even though, as noted, robust expressions of it were a rarity) is the controversy that engulfed Colin

That the system of values and cognition of each culture can be so presented goes without saying for an anthropologist. That these systems cannot be ranked on some transcultural scale from primitive to modern, also goes without saying. Aboriginal peoples are not contemporary ancestors; progress in intellectual matters is hard enough to assess; in rules governing human relations it is next to impossible. Most societies make cognitive sense of their world and live by moral rules. The anthropologist should undertake simply to show that this is so.

A problem arises, however, when anthropologists try to draw an inference to the effect that just because a cognitive system makes sense of things there is no position from which to declare some of its assertions erroneous. Our own European history provides many counter-examples. The erroneous view that the night-sky moves around the earth was once official doctrine in our culture and it was criticized and overthrown *from within*. Notoriously, the Ptolemaic system could accommodate all the known facts about the motions of the planets and need never have been given up. Yet it was given up, and for good reasons. This is a phenomenon that cultural relativism is unable to save. The overthrow of the earth-centered view was the outcome of a lengthy struggle over the means by which such culturally entrenched and endorsed ideas could possibly be overthrown (cf. the conflict between Cardinal Bellarmino and Galileo Galilei[23]). What this teaches us is a general lesson. Polarizing the issue between cultural relativism and ethnocentrism creates a false dilemma. There is a *via media*: autonomous and self-critical thinking, or rationality. Ideas are not correct *because they are endorsed by a culture*, and they are not incorrect *because they are rejected by a culture*. Their status is decided by other means. Admittedly, it is more difficult to point to clear-cut and rational advances in moral understanding or even moral behavior that parallel our increased understanding of the physical universe. Yet if we simply explain our sense of moral improvement by caprice or power shifts then we condescend to ourselves and others. One of the reasons we in our culture engage in moral discussion and debate is in the quest to improve not just our moral behavior, but also our moral standards. Why should we withhold this approach from cultural others?

Turnbull over his book *The Mountain People* [1972]. An experienced and initiated member of the anthropological profession, Turnbull's book told a shocking story of acute deprivation and the breakdown of social order. He told his story with sadness and compassion, but also with some horror and without concealing what he considered the reprehensible behavior such breakdown had produced. Perhaps his most shocking conclusion was that the decline and dispersal of the Ik was not to be regretted as they had lost their way. The absorption of the survivors into neighboring societies might be the only way to regain their humanity. This was strong stuff and must have taken some courage to set down. And of course the popular media were much titillated. One can gauge the reaction by reading the symposium in *Current Anthropology* which was led off by an article by Barth, the title of which combines pomposity and an almost Inquisitional mentality: "On Responsibility and Humanity: Calling a Colleague to Account" [1974]. Unsurprisingly Turnbull was a bit nonplussed [1974; 1975].

[23]The allusion here is to the important struggle between Galileo and the Church, the former defending his right to free inquiry, the latter demanding that he bow to the wisdom of the holy church through her earthly representatives and take direction about what could and could not be freely explored and said. See [Segre, 1997].

Two conclusions recommend themselves: anthropologists should couple their opposition to ethnocentrism with an equal opposition to anti-ethnocentrism, that is, neither the other nor their own heritage should be held in disrespect; and respect requires them to report that their own heritage centers on science, technology, and liberalism which in ethnographic fact claim universal standing.[24]

8.5   Cultural Relativism and Method

Cultural relativism construed as the repudiation of ethnocentrism constitutes an attitude highly appropriate for field-work.[25] The attitude it promotes is one of expecting and seeking out difference, assuming and imputing rationality, and suspending judgment and censure. All the best fieldwork should be informed by such attitudes [Jarvie, 1967]. In so far as cultural relativism is merely a name for that methodological approach, as is sometimes claimed, then it is co-terminus with good anthropology. Methods are not true or false, but adequate or inadequate, fruitful or barren, etc. The biggest single impetus to relativism in anthropology and sociology was no doubt its fruitfulness as method. To apply the method we can suspend judgment about whether the variable under investigation is or is not absolute, is or is not dependent on something else. It is sufficient for methodological purposes to adopt the weaker position that we shall proceed *as if that were the case* and see what results we get. On the matter of suspending moral judgment, however, the case is not so simple. Methods may not be true or false but they can be moral or immoral, above all in the investigation of humans. It is likely this consideration that pushes social scientists to try on the one hand to tell it as it is, whilst on the other hand insisting that moral, political, and associated judgments are unavoidable. Equipped with this "as if" attitude translated into a method [Hanson, 1975], cultural anthropologists made the thrilling discovery that there is a wide diversity of moral and ontological claims made around the world. Neither in the judgment and evaluation of actions, nor in considerations of the world picture, do human societies agree with one another (or even within themselves). Those disenchanted with or in rebellion against the values and outlook of their own society find this an exciting aspect of anthropology. Perfectly orderly and admirable societies exist which affirm different values and views of the world. Thus we often find anthropologists implicitly holding up the values and world-views of their subjects as admirable, even superior, to those of industrial/scientific societies, especially their attitudes to nature in general and to the earth in particular. Cultural relativism does nothing to check such romanticism, only noticing the inconsistency of such insinuations can do so.

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[24]For the development of this idea in the context of Critical Theory, see Bohman, this volume.
[25]For discussion of the philosophical issues arising from ethnography, see Risjord, this volume.

8.6   Implications of the Method

The facts of morality and cognition discovered by the cultural relativist method seem to have prescriptive implications: if values and judgments of the real vary with culture, then none is superior, i.e. correct (and the others incorrect). We have seen that there are various problems with this. The ethnographic problem is that cultures mostly affirm their values and worldview as not only correct but as the only correct one: others are incorrect and possibly even wicked. The conceptual problem is this: if cultures hold views about values and about the world that are inconsistent with one another then they cannot all be true.[26] Finally, nothing prescriptive follows from factual premises. The fact that we differ on a matter does not lead to the conclusion that we ought to differ or that differing is good. Moral and cognitive diversity is a problem, not a solution. Whether the earth is the center of the universe, whether slavery should be judged wicked, are not matters to be decided by a poll of views around the world. Most societies in most of history have been mistaken in their views on both matters. It is an ethnographic fact that in our Western culture we take our present positions on the universe and on slavery to have moved nearer the truth compared to the former position embraced by our ancestors. Relativism as method must report this, even though it makes no sense within normative relativism.

Thus the method associated with cultural relativism delivers interesting facts, but cultural relativism as an idea offers a confused interpretation of those facts [Spiro, 1986; 1992, ch. 1]. Can there be, then, a warrant for the same method that does not appeal to cultural relativism? The answer is clear: the general principles of scientific open-mindedness are more than sufficient to warrant the method of fieldwork. Common sense, much of it ethnocentric, provides a huge stock of assertions about other peoples that invite testing and refuting. One hundred years of anthropology also supplies a large number of more refined assertions about the general explanatory principles that govern human social organization, and these too can be subjected to empirical test by the method of fieldwork. Thus fieldwork can be problem-oriented, critical, and opposed to ethnocentrism, without the necessity of any appeal to cultural relativism [Jarvie, 1967]. The critical and open-minded attitude of science is also the best for encouraging the tolerance and respect enjoined by multiculturalism. Not to compare, not to judge, not to debate and criticize the others in the multicultural mosaic is impossible. Both Europe and the Americas have accepted immigrant groups whose cultural practices violate their laws (such as honor killing, feud, and certain bodily mutilations). Tolerance and respect do not consist always in modifying the laws to accommodate the immigrants; they sometimes consist in insisting that the laws are better than the

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[26]This point of logic needs to be insisted upon in light of such a remark as this by Brandt: "a necessary condition for the tenability of ethical relativism" is that in principle two people may assert "contradictory ethical views without either being mistaken" [Brandt, 1954, 11]. Contradictory views, by the definition of contradictory, take opposite truth values. Perhaps Brandt meant to write "contrary views", but that would not save the sentiment, since contrary views cannot both be true but they can both be false.

imported cultural practices which ought, consequently, to atrophy. Sometimes the host society will move towards tolerance, sometimes it will insist that the newcomers and incomers do the same. No doubt these debates and compromises will be subject to reconsideration and reassessment from time to time. This makes them part of the normal negotiation of civil society. The omnitolerance endorsed by cultural relativism can make no rational sense of this process.

8.7   Attacking the Critics of Cultural Relativism

One of the best forms of defense is attack. Thus cultural relativists have a tendency to impute to their critics either ethnocentrism or simple ignorance of just how different the factual and moral judgments of other societies are [Geertz, 1984]. To the philosopher such attacks are worthless because *ad hominem*. A dyed-in-the-wool relativist could reject that judgment in the course of relativizing the whole theory of logic and fallacies which it presupposes. What is more interesting is the sociological fact of the attacks. In his [1984], for example, Clifford Geertz more or less eschews arguments and substitutes jibes at "amateur logicians" (265) and the provincialism of the anti-relativists. What is one to make of this sociologically? One obvious explanation is that many anthropologists, and Geertz among them, think that their subject is predicated on relativism. They mean descriptive relativism, of course; but they also insist that that procedure requires suspension of judgment. Hence the attack is to protect academic turf. Another explanation is that anthropologists see their mission as enlightening the provincial. They are in possession of potent knowledge and resistance to it is to be expected. But that knowledge contains an imperative that it be spread. *Sapere aude!*

What is this potent knowledge that meets resistance and which it is the anthropologist's duty to confront and overcome? Herskovits's argument about language has been strengthened into the extreme claim that languages are so different that translation, never mind evaluation, of cultural differences is impossible. This hardly sits well with well-known facts. Even within the familiar culture of Christian Europe, historically considered, almost all the relevant issues show themselves. That is to say, the cultures of ancient Greece and Rome, for example, are both extremely alien and manifestly ancestral to present-day Europeans.[27] Did we modern Europeans get here from our ancient European there by whim and accident, or was there some sort of learning process, some progress? No-one would deny there were elements of whim and accident, but we would also want to say that in technology, agriculture, cognition, writing systems, and social organization, there has been progress that transcends each unit we choose to consider a culture. The evolution of law and trial, for example, progressively improves through Greece,

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[27]Pace Hegel who gives relativism a reactionary spin: "Every age has such peculiar circumstances, such individual conditions that it must be interpreted, and can only be interpreted, by reference to itself. . . Nothing is shallower in this respect than the frequent appeal to Greek and Roman examples which so often occurred among the French at the time of their Revolution. Nothing could be more different than the nature of these people and the nature of our own times". Quoted in [Gombrich, 1987, 687].

Rome, the Middle Ages, the Reformation, and the Enlightenment. This knowledge and the institutional invention that implements it become part of the heritage of humanity, so that any culture, anywhere, contemplating setting up a state and a system of law, consults the European historical record (among others) for useful ideas.

Hence the claim that relativism is heir to the Enlightenment is false. Relativism is a way of resisting the light the Enlightenment wished to cast into the shadows of superstition, ignorance, and repressive social systems. Relativism aligns with reaction, not, as some naïve conservatives think, with left radicalism.

8.8   Who is Liberal, Who is Illiberal?

Anthropologists accuse each other of illiberal tendencies, for example, of neo-colonialism [Asad, 1973] or they are accused of Orientalism [Said, 1978; cp. Lewis, 1993; Gellner, 1980; 1993]. They accuse themselves of treating the Other with less than the respect due to a fellow-human, including failure to endorse the Others' culture and identity. Some of this stems from relativist premises, but usually it does not. What is clear is that the heritage of the Enlightenment is contested within anthropology and relativism is only one of the sites.

In this contestation anthropologists have to take account of non-relative features of their own culture where the demand to treat people with respect includes the demand to acknowledge differences but also to engage with them robustly. For example, those who try to say that this is a Christian civilization and that it should teach its outlook and values to the young are robustly countered by those who say it is a liberal civilization and should be careful not to impose mainstream or merely majority views on the young. Liberalism requires that we respect and listen to all: it does not involve endorsing anything we consider error. Because condescension is disrespectful, liberalism sometimes imposes a duty on us to point out error.

This dispute about the heritage of the Enlightenment brings out an important point that bears on cultural relativism but applies to a far broader range of sociological and anthropological thinking. It is a deep error to think that societies and cultures are homogeneous and integrated. Or, more precisely, societies and cultures are homogeneous and integrated only under certain descriptions. Conformity to custom hardly ever reaches one hundred per cent. Even a description of a language, the backbone of culture, is a simplification of diversities of usage, vocabulary cluster, dialect and even idiolect. The same is true of generalizations about the ideas held in a culture concerning the world and human conduct. No society is homogeneous in these matters, and in almost all societies they are the subject of incessant discussion and dispute — even in the most simplified and petrified. Thus, *in a strict sense, there is no homogeneous and unified culture to which cognitive and moral assertions can be relativized* (Jarvie 2000). Cultures are not natural units that identify themselves. We identify social and cultural units for a purpose, and for the purposes of cognition and morality we might do well to

view human groups as *shifting arenas of dispute and debate*. For other purposes, we may lump together groups widely spread in time and in space, as when we think of Europe as a culture area, or of science as a tradition with an associated culture that is open to all persons of good will.

If this point is correct it vitiates descriptive relativism since that denies the validity of any but the most particularized descriptions and it denies the reality of the larger units to which judgments are being relativized.

9   THE TRUE ANTHROPOLOGICAL SITUATION

The human predicament as historical sociology discloses it is that there was a huge cognitive leap forward at one time and place, namely Europe. This Scientific Revolution dwarfed most prior cognitive efforts [Gellner, 1965; 1988; 1992a; 1992b; 1992c]. Since then, science has been a progressive and technologically powerful force, far from socially neutral. Coming to terms with its power, its universality, and its indifference to local society and culture raises a deep problem of which relativists have yet to recognize the ethnographic dimensions.[28] What is less clear-cut is the situation in the sphere of value. Our moral language often mimics cognitive language, and we assume there is moral progress: that is, we assume a society of law is better than one without, that law enforcement is better than feuds, that knowledge is a better condition than ignorance, that equality of persons, including females, is better than inequality, that societies which do not kill their citizens are better than those that do. Latterly, and even more of a challenge, have come questions of demography and ecology, where we can argue that societies that curb population growth and minimize the depredation of non- renewable resources are better than those that do not. These are all value judgments that most anthropologists and sociologists endorse and live by and that most of the societies they have studied are very far from endorsing. Finally, we should laud the wish of cultural relativists to combat ethnocentrism, including all sorts of colonialism, old and new, but we should reject their idea that relativism cures all ills: curing them is an ongoing project that requires different intellectual auspices.[29]

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[28]One of the most troubling criticisms of British social anthropology by post-colonial anthropologists is the extent to which the classic ethnographies carefully erased or shaded over the impact of colonization and modernization. For further discussion of this issue, see Risjord, this Volume.

[29]Cp. [Hatch, 1983, 144].

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THE PROBLEM OF APPARENTLY
IRRATIONAL BELIEFS

Steven Lukes

The problem of apparently irrational beliefs arises when we are confronted with another who appears to believe what appears puzzling. Notice that the word 'appears' occurs twice in this formulation. The first use raises the issue of sincerely asserted belief. How can we know what others *really* believe? When people say puzzling things they could be joking or pretending or mimicking or free-associating or reciting or, in general, performing a wide range of acts while expressing themselves in the propositional language of belief.[1] There is obviously much to be said about the issue of how to ascertain whether beliefs are genuine, but in what follows I shall not consider it, assuming, for the sake of the arguments I do want to consider, that we can do so. And so the problem is how to deal with the puzzlement that can arise when we are faced with the sincere beliefs of others.

The puzzlement can range from shallow to deep: from the easily resolvable to the seemingly intractable. People can make simple, and less simple, *mistakes*. Consider this example from the philosopher Richard Grandy (to which I will return):

> Suppose Paul arrives at a party and asserts 'The man with the martini is a philosopher.' And suppose the facts are that there is a man in plain view who is drinking water from a martini glass and that he is not a philosopher. Suppose also that in fact there is only one man at the party drinking a martini, that he is a philosopher, and that he is out of sight in the garden. [Grandy, 1973, 445]

Here we have, as Grandy remarks, an explicable falsehood, in this case easily explicable. There are also failures of reasoning, extensively studied by experimental social psychologists of the 'heuristics and biases' tradition, in which most subjects perform poorly when faced with simple tests of reasoning and judgment, have unwarranted confidence in their reasoning and judgmental powers and are subject to the effects of 'framing' [Nisbett and Ross, 1980; Kahneman, Slovic and Tversky, 1982; Dawes, 1988; Piatelli-Palmerini, 1994; Sutherland, 1994; Baron, 2001]. Also at the shallow end are cases of wishful thinking, ideological bias [Elster, 1982],

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[1]So, for example, when men of the Bororo tribe of Central Brazil say 'we are red macaws', they are, according to Christopher Crocker, seeking "to express the irony of their masculine condition" [Crocker 1977, 192]. But, as Dan Sperber remarks, this metaphorical expression is in turn based on their literal belief in real contacts with spirits [Sperber 1982].

self-deception and indeed simply succumbing to deception. Some of this is captured in George Orwell's classic essay 'In Front of Your Nose', which identifies a "habit of mind which is extremely widespread, and perhaps always has been", which exhibits the "power of holding simultaneously two beliefs which cancel out" and the closely allied power of "ignoring facts which are obvious and unalterable, and which will have to be faced sooner or later" [Orwell 1946, 151] Orwell comments that it is "especially in our political thinking that these vices flourish" and cites, among others, the following instance, which neatly exemplifies both of the indicated powers:

> For years before the war, nearly all enlightened people were in favour of standing up to Germany: the majority of them were also against having enough armaments to make such a stand effective. [Orwell, 1946, 152]

The point, wrote Orwell, is

> that we are all capable of believing things which we *know* to be untrue, and then, when we are finally proved wrong, impudently twisting the facts so as to show that we were right. Intellectually it is possible to carry on this process for an indefinite time: the only check on it is that sooner or later a false belief bumps up against solid reality, usually on a battlefield. [Orwell, 1946, 153]

Crowding the deep end are all those beliefs — variously categorized as religious, mystical, magical, ritual, pre-logical, and the like — on which philosophers, theologians, anthropologists and others have focused their attention in debating the knotty issues of how they are to be interpreted and explained. Here are some examples taken from recent debates: Zande beliefs in witchcraft (witches are identified by consulting oracles by administering poison to chickens), which sometimes stress and sometimes deny its hereditary character, alongside Evans-Pritchard's report that 'Azande do not perceive the contradiction as we perceive it because they have no theoretical interest in the subject, and those situations in which they express their belief in witchcraft do not force the problem upon them' [Evans-Pritchard, 1937, 25]; the Nuer belief that 'twins are birds' [Evans-Pritchard, 1956, 131] and the Yoruba belief that boxes covered with cowrie shells, which they carry around with them, are their heads or souls [Hollis, 1996a,199]; the belief of a wise old Dorze man in the existence of a gold dragon, with a heart made of gold and a horn on the nape of its neck [Sperber, 1982, 149]; the alleged belief of the Hawaiians that Captain Cook was their god Lono [Sahlins, 1995; Obeyesekere, 1997][2]; the belief in *tlahuepuchis* in the Tlaxcala region of Mexico: that infants sleeping with their mothers are killed by bloodsucking witches who can transform themselves into various animals and insects [Nutini and Robers, 1993], discussed in [Risjord, 2000]; the Hindu belief in the reality of rebirth when widows undergo *sati*, or self-immolation [Hawley, 1994], discussed in [Risjord, 2000]; and so on. This list can,

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[2]I have discussed this case in [Lukes, 2003].

of course, be indefinitely extended and should certainly include the story of the Resurrection and the doctrine of transubstantiation.

And then there are all those beliefs considered by Michael Shermer, editor of *Skeptic* magazine and director of the Skeptics Society, in his book *Why People Believe Weird Things: Pseudo-science, Superstition and Bogus Notions of our Time* [Shermer, 2002a] and catalogued in his *The Skeptic Encyclopedia of Pseudoscience* [Shermer, 2002b]. These include belief in extra-sensory perception, near-death experiences, encounters with aliens, and creationism, and also in ritual abuse accusations, myths of racial superiority, and Holocaust denial. Shermer clearly takes all such beliefs as shallow, since his answer to the question of why people believe these things is straightforward: they deceive themselves or are deceived (he gives many examples of the latter) and they reason badly. As he puts it,

> The analyses in this book explain in three tiers why people believe weird things: (1) because hope springs eternal; (2) because thinking can go wrong in general ways; (3) because thinking can go wrong in particular ways. [Shermer 2002a, 8]

It is interesting that Shermer has published another book, *How We Believe: Science, Skepticism and the Search for God* [Shermer, 2003], in which religious belief is subjected to the same skeptical treatment.

But this suggests something that is, in any case, obvious: that the distribution of beliefs across the puzzlement continuum from shallow to deep is highly contestable and reveals as much about the distributor as about what is distributed. Being puzzling is a relational property: a belief is puzzling to one who is puzzled and people will differ over which beliefs are more and which less puzzling. Some will say that it is militant Voltairean atheists like Shermer who are shallow, not the beliefs they seek to debunk; others may find sources of perplexity in the beliefs involving mistakes in perception and reasoning with which I began. Indeed, some may question my very starting point — the puzzlement raised by the sincere beliefs of others — and advocate a more thoroughgoing skepticism: that we should begin by finding our own beliefs puzzling.

The problem of apparently irrational beliefs, as I defined it, is thus a problem that raises, in turn, the question of relativism: of whether *answering* the question of what counts as rational, or non-puzzling, is relative to different perspectives, so that there is a plurality of correct and conflicting answers to it.[3] Or are there (at least some) criteria of rationality that are not just local, shaped by local norms and internal to particular cultures or forms of life? This, as we shall see, is the fulcrum underlying the debates referred to above, on which I shall focus in this essay.

Before doing so, however, I want to discuss another question raised by the topic of these debates, namely, the question of examples. Where do the 'apparently irrational beliefs' typically discussed come from? The answer is that they

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[3]For further discussion of relativism, see Jarvie, this volume.

come from three distinct sources. The first of these is *philosophers*. Typically, philosophers, notably those interested in so-called 'radical interpretation', assume an interpreter faced with the task of translating from a tribe but ignorant of its members' language, culture and psychology, and so they offer radically simplified, under-described suppositions of what this imagined tribe might believe, with the aim of exposing the general requirements of having a language. Thus Quine, for instance, asks his readers to imagine members of such a tribe exclaiming 'gavagai' as a rabbit rushes by and perhaps meaning 'undetached rabbit part' [Quine, 1960]. And thus Wittgenstein, making a quite different argument, imagines some very weird woodcutters:

> 142....People pile up logs and sell them, the piles are measured with a ruler, the measurement of length, breadth, and height multiplied together, and what comes out is the number of pence which have to be asked and given. They do not know 'why' it happens like this; they simply do it like this: that is how it is done — Do these people not calculate?...

> 148. Very well; but what if they piled the timber in heaps of arbitrary, varying height and then sold it at a price proportionate to the area covered by the piles?

> And what if they even justified this with the words: "Of course, if you buy more you have to pay more"?

> 149. How could I show them that — as I should say — you don't really buy more wood if you buy a pile covering a bigger area? — I should, for instance, take a pile which was small by their ideas and, by laying the logs around, change it into a 'big' one. This *might* convince them — but perhaps they would say: "Yes, now it's a *lot* of wood and costs more" — and that would be the end of the matter. — We should presumably say in this case: they simply do not mean the same by "a lot of wood" and "a little wood" as we do; and they have a quite different system of payment from us. [Wittgenstein, 1956, 43-44]

The second source of examples is *social anthropologists*, who report (or used to report) on the exotic beliefs of real far-away tribal peoples. But, as Dan Sperber remarks,

> In most anthropological works...the reader is directly presented with an elaborate interpretation in the form of a consolidated, complex and coherent discourse (with just occasional translations of native statements and descriptions of anecdotes by way of illustration). Such interpretations are related to actual data in poorly understood, unsystematic and generally unspecified ways. They are constrained neither by standards of translation nor by standards of description. They resemble the more indirect and freer forms of indirect speech, where

> the utterances or thoughts reported can be condensed, expanded, co-
> alesced, fragmented, pruned, grafted and otherwise reworded at will.
> [Sperber 1982, 162]

This lack of constraint bears, of course, on our present theme insofar as the anthro-
pologist has preconceptions regarding the question of relativism (which is usually
the case).

The first and second sources are sometimes merged in the literature debating
these issues when philosophers, seeking to illustrate their arguments, appropriate
anthropological texts and organize those arguments around examples whose rela-
tion to actual data is even more poorly understood, exotic examples that they, in
turn, under-describe, and which are therefore, in all probability and to an indeter-
minate extent, fictitious. (A memorable instance where this was established was
noticed by Ernest Gellner [Gellner, 1973] in papers written by Peter Winch and
Alasdair MacIntyre, in which extensive reference was made to cattle among the
Azande, who have no cattle.[4]

None of this, however, is to suggest that the anthropological evidence is not
centrally relevant to our question. As Clifford Geertz rightly observes, in his coyly-
entitled paper 'Anti Anti-Relativism', anthropologists bring news from elsewhere
to curb our provincialism. They insist

> that the world does not divide into the pious and the superstitious;
> that there are sculptures in jungles and paintings in deserts; that the
> political order is possible without centralized power and principled jus-
> tice without codified rules; that the norms of reason were not fixed in
> Greece, the evolution of morality not consummated in England... [and
> that] we see the lives of others through lenses of our own grinding and
> that they look back on ours through ones of their own. [Geertz, 2000,
> 65]

And the arguments of the philosophers, together with their outlandish examples,
whether invented or borrowed, perform the valuable task of posing, in an acute,
pared-down way, problems about the nature of translation and interpretation,
problems that are completely abstract and general.

That is why for the third source of apparently irrational beliefs we do not need
to reach for the exotic or the invented, for they are, as Orwell and Shermer both
see, *all around us.* Many are home-grown (from astrology and horoscopes to the
effusions of talk radio in the United States) and they can only multiply as our
provincialism is challenged by the world's shrinking through global communica-
tions and by transnational migration. The value of drawing on this third source is

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[4]I was myself present at a joint meeting in Oxford of the Socratic Club and the Oxford
Anthropological Society addressed by the two philosophers, at which Professor Evans-Pritchard
himself, after claiming to know little about philosophy, gently pointed this out. Gellner's article
in *The Times Literary Supplement*, commented on the lapse, observing that the index to Evans-
Pritchard [1937] has the following entry: 'Cattle, absence of.' His article, republished (in modified
form) in [Gellner, 1973], was followed by a spate of letters from various philosophers, some of
them angry.

that it yields vivid examples that involve real rather than notional confrontations [Williams, 1985, 160] that directly raise the issue of rationality. Consider, for instance, the following example, cited in Thomas Frank's polemical book, *What's the Matter with Kansas? How Conservatives Won the Heart of America*, of a claim made a year after the 2000 Presidential election. An article appeared in *National Review Online* that appealed to the fact that President Bush won the votes of counties occupying 2,427,039 square miles, while Al Gore only took the votes of 580,134 square miles. This, the article claimed, showed that Bush's vote was "more representative of the diversity of the nation" than Gore's, for

> A look at the county-by-county map of the United States following the 2000 vote shows only small islands (mostly on the coasts) of Gore Blue amid a wide sea of Bush Red. In all, Bush won majorities in areas representing more than 2.4 million square miles, while Gore was able to garner winning margins in only 580,000. [Frank, 2004, 267]

Unlike the impenetrably mysterious world-view of Wittgenstein's woodcutters, in which measuring the area a woodpile occupies is supposedly relevant to a 'quite different system of payment' for the wood, this case offers, not a quite different conception of voting, but an intelligible and explicably self-serving adducing of irrelevant considerations of space to account for the election's outcome.

This leads me directly to the question before us, by raising the issue of intelligibility and explicability. There are, I suggest, two broad approaches to the problem of apparently irrational beliefs evident in the debates of recent decades (and there are several versions of each). Recall that the problem arises with puzzlement. Mark Risjord offers one succinct way of characterizing it. Problems of apparent irrationality, he writes, arise "when interpretation falters. Local action or speech seems irrational in the light of a background understanding" [Risjord, 2000, 2]. Thus,

> One of the ways a problem of apparent irrationality can arise is when the interpreter loses her grip on the subject's reasons for action. She can see what they are doing, but does not comprehend their reasons. Alternatively, she knows what reasons they give, but cannot see how the avowed reasons could be sufficiently motivating. To resolve this sort of difficulty, the interpreter needs to identify what counts as a good or bad reason (for the locals) in such a context. These will be local criteria of rationality. [Risjord, 2000, 153-154]

By this characterization, Risjord clearly identifies himself as an adherent of the first of the two broad approaches, which I shall label 'the localist approach', according to which criteria of rationality — what counts as a good or bad reason — is decided locally and can thus be discovered to vary from context to context: "the question of good and bad reasons for belief concerns only the local criteria of rationality" [Risjord, 2000, 48] and "the principles of rationality guiding the locals

in their speech, belief and behavior might be different from those that guide the interpreter" [Risjord, 2000, 52].

The other broad approach, which I shall, for reasons which will immediately become clear, call 'rationalist,' denies precisely this, maintaining, on the contrary, that what is rational is not discovered but presupposed and that it conditions what is intelligible. According to this second approach, rationality is an *a priori* constraint on translation and interpretation: attributing beliefs, and indeed concepts, to others requires that we view them as largely rational, where 'rational' does not mean rational by our standards, on the assumption that they may live by other and different, local standards. Making sense of others' beliefs or, in another version, their utterances, however aberrant, requires us, on this approach, in Donald Davidson's words, "to find a great deal of reason and truth in them" and irrationality is a 'perturbation of reason' identifiable only against a background of reasonable action and belief [Davidson, 1984, 153; 2001, 99]. Or, as Martin Hollis put it, "some assumption about rationality has to be made *a priori* if anthropology is to be possible; and... we have no choice about what assumption to make" [Hollis, 1996a, 206].[5]

The localist approach (sometimes also labeled 'pluralist' and sometimes 'relativist') was foreshadowed in Lucien Lévy-Bruhl's writings on 'pre-logical mentality' [Lévy-Bruhl, 1910] and the so-called 'Sapir-Whorf hypothesis'; thus Sapir wrote that the "'real world' is to a large extent unconsciously built upon the language habits of the group" [Sapir, 1929, 209]. But it can be seen as having found recent inspiration in a much-discussed essay by Peter Winch, 'Understanding a Primitive Society', first published in 1964 [Winch, 1970]. Winch took Evans-Pritchard's work on the Azande to task for projecting onto them a Western preoccupation with explanation, prediction and control; their witchcraft beliefs are not to be seen as "a theoretical system in terms of which Azande try to gain a quasi-scientific understanding of the world" [Winch, 1970, 93] but rather, Winch boldly suggests, they recall Christian prayers of supplication "in that they do, or may, express an attitude to contingencies; one which involves recognition that one's life is subject to contingencies, rather than an attempt to control them" — and he cites in particular the 'limiting situations' of birth, sexual relations and death. Accordingly, Winch argues, we should not interpret them as making what we know to be attributions of mystical causation and we should not rush to convict them of logical contradictions. It is the "the European, obsessed with pressing Zande thought where it would not naturally go — to a contradiction — who is guilty of misunderstanding, not the Zande" [Winch, 1970, 93]. Moreover, he cites Evans-Pritchard reporting that the Zande is "immersed in a sea of mystical notions", which are "eminently coherent, being interrelated by a network of logical ties, and are so ordered that they never too crudely contradict sensory experience but, instead, experience seems to justify them" [Evans-Pritchard, 1937, 319]. Winch takes this to imply that the Zande are to be understood as following alternative standards of

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[5]For further discussion of translation, as well as Davidson and Quine, see Henderson, this volume.

rationality. In accordance with that idea, he writes sentences such as these: "our idea of what belongs to the realm of reality is given for us in the language that we use" [Winch, 1958, 15] and "the criteria of logic are not a direct gift from God but arise out of and are only intelligible in the context of ways of living and modes of social life" [Winch, 1958, 100].

Winch's essay can be read as expressing a thought that has since become increasingly present in the Zeitgeist: the thought that human beings are subject to normative requirements of rationality, which dictate how one ought to reason, deliberate and act, and that these vary across languages, cultures, perspectives, ways of living, and modes of social life.[6] This thought pervades writings in diverse fields, from social anthropology to the sociology and history of science. So Marshall Sahlins, explaining that the Hawaiian 'natives' thought Captain Cook was their god, proclaims 'Different cultures, different rationalities' [Sahlins, 1995] and Barry Barnes and David Bloor, pioneers of the 'Edinburgh school' of science studies, write that "the compelling character of logic, such as it is, derives from certain narrowly defined purposes and from custom and institutionalized usage. Its authority is moral and social" and they refer to alternative "logical conventions" [Barnes and Bloor, 1982, 45]. Philosophers too have embraced and defended this thought. Thus Stephen Stich's aptly titled book *The Fragmentation of Reason* defends an account of cognitive virtue that is 'floridly pluralistic', arguing that if "it should turn out that different people or different cultures use radically different 'psycho-logics,' or that the revising and updating of their cognitive states is governed by substantially different principles, pluralism will have a firm foot in the door." Moreover, he writes, his position is "relativistic as well, since it entails that different systems of reasoning may be normatively appropriate for different people" [Stich, 1993, 13-14]. And Mark Risjord argues that "an interpretation does not have to assume that the interpreter's and the interpretees' standards of rationality are the same" and that it is possible "to prefer an interpretation that both gives rationality an explanatory role and attributes to the locals criteria of rationality that diverge from the interpreter's" [Risjord, 2000, 59].

The rationalist approach (sometimes also labeled 'universalist') also drew recent inspiration from a much-discussed text of the 1960s: namely, Quine's *Word and Object* [Quine, 1960]. Quine took up the challenge of Lévy-Bruhl's claim that there is a 'pre-logical mentality' to be found among the natives. Performing the thought-experiment indicated above of radical interpretation, where a naïve interpreter faces the speakers of an unknown language, he propounds as a 'maxim of translation' that "assertions startlingly false on the face of them are likely to turn on hidden differences of language", which he defends on the 'commonsense' ground that "one's interlocutor's silliness, beyond a certain point, is less likely than bad translation — or, in the domestic case, linguistic divergence." Quine gives as an

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[6]We should, in passing, note that Winch's position was not unambiguously localist. The sentences quoted above can be interpreted in either a localist or non-localist way; and it is significant that his way of rendering Zande witchcraft beliefs intelligible to his readers is by analogy (albeit admittedly imperfect) with familiar Christian ways of reasoning.

example of 'silliness' a straightforward contradiction, but his point is general and holistic: "fair translation preserves logical laws," [Quine, 1960, 59] and it is sets of beliefs and/or inferences that, if bizarre, call translation or interpretation into question.

Quine's maxim of logical charity was then generalized, developed and refined by Davidson into the so-called 'Principle of Charity,' according to which a charitable translation will be one which 'optimizes agreement,' making the utterances of one's interlocutors come out as largely true. It is a principle which, in Davidson's view, is "not an option, but a condition on having a workable theory' of translation" [Davidson, 1973-4, 19]: we must take others as largely rational if we are to interpret them. This is because what makes interpretation possible

> is the fact that we can dismiss a priori the chance of massive error. A theory of interpretation cannot be correct that makes a man assent to very many false sentences: it must generally be the case that a sentence is true when the speaker holds it to be. . . .So in the end what must be counted in favor of a method of interpretation is that it puts the interpreter in general agreement with the speaker. . . . [Davidson, 1984, 169]

Charity is a condition of interpretation because only by exercising it can we recognize our interlocutors as agents who are capable of reasoning and have largely true beliefs about their environment. And it is only when these beliefs are largely true that we can break into the circle of meaning and belief and arrive at a justified interpretation of their utterances. (Of course, we can always doubt that radical interpretation can enable us correctly to interpret others, but Davidson assumes that language functions as a means of communication only on the basis of interpersonal evidence for what others mean.)

Richard Grandy, in turn, refined the Principle of Charity into the so-called Principle of Humanity, on the grounds that the point of translation is "to enable the translator to make the best possible predictions and to offer the best possible explanations of the behavior of the translatee." But that requires us not to optimize agreement or minimize disagreement over what is true and false, but rather to prefer a translation in which "the imputed pattern of relations among beliefs, desires and the world be as similar to our own as possible" and thus, in the case of Paul and the martini-drinking philosopher in the garden, it is "better to attribute to him an explicable falsehood than a mysterious truth" [Grandy, 1973, 445]. On Grandy's account, if

> a translation tells us that the other person's beliefs and desires are connected in a way that is too bizarre for us to make sense of, then the translation is useless for our purposes. So we have as a pragmatic constraint on translation, the condition that the imputed pattern of relations among beliefs, desires and the world, be as similar to our own as possible. This principle I shall call the *principle of humanity.* [Grandy, 1973, 442-3]

Along parallel lines, Martin Hollis addressed the issue of interpreting alien (or ritual) beliefs by advancing his celebrated 'bridgehead' argument. Unlike Quine, Davidson, and Grandy, he addresses, not the naïve radical interpreter but the working anthropologist. Like them, he argues that the anthropologist "must budget for *a priori* elements which are not optional." These are

> those notions which the natives must be assumed to have, if any identification of their ritual beliefs is to be known to be correct. To get at ritual beliefs the anthropologist works from an understanding of the native language in everyday contexts. To establish a bridgehead — a set of utterances definitive of the standard meanings of words — he has to assume at least that he and the natives share the same perceptions and make the same empirical judgments in simple situations. This involves assumptions about empirical truth and reference, which in turn involve crediting the natives with his own skeletal notions of logical reasoning. To identify their ritual beliefs, he has to assume that they share his concept of 'being a reason for.' There will be better reason to accept his account than to reject it, only if he makes most native beliefs coherent and rational and most empirical beliefs in addition true. These matters are *a priori* in the sense that they belong to his tools and not to his discoveries, providing the yardsticks by which he accepts or rejects possible interpretations. They are not optional, in that they are the only conditions upon which his account will be even intelligible. [Hollis, 1996b, 219]

In short,

> there has to be some set of interpretations whose correctness is more likely than any later interpretation that conflicts with it. The set consists of what a rational person cannot fail to believe in simple perceptual situations, organized by rules of coherent judgment, which a rational person cannot fail to subscribe to. All interpretation thus rests on rationality assumptions, which must succeed at the bridgehead and which can be modified at later stages only by interpretations which do not sabotage the bridgehead.' [Hollis, 1996c, 228]

These various versions of rationalism have in turn been criticized, in various ways, by localists and by others, including those (such as the present writer: see [Lukes, 1970; 1973; 1982; 2002]) in search of some coherent combination and reconciliation of the two approaches that retains what is plausible in each. Thus Quine's logical charity has been criticized for seeming to (1) preclude disputes in the philosophy of logic, (2) eliminate the possibility of logical mistakes and (3) be at variance with the empirical evidence of the 'heuristics and biases' literature referred to above [Cooper, 1975; Gellner, 1970; Morton, 1970; Risjord, 2000]. Davidson's more extensive conception of charity has also been subjected to criticism at various stages in its development. 'Optimizing' agreement appeared to be an obscure

notion, while 'maximizing' it raised the insoluble issue of how one individuates and counts beliefs [Ludwig, 2004; Stich, 1990]. The better idea of minimal disagreement was, as we have seen, criticized and amended by Grandy. More deeply, Davidson's Principle of Charity, and in particular, its claim that the interpretees' empirical beliefs about their environment must, if the interpretees are to be rational, be largely true has been contested. What if they are systematically deceived by Descartes' evil demon? Then, it has been argued, rationality would indeed require

> a large number of true general beliefs as a condition on possessing the concepts involved in any of an agent's beliefs, but these would not be empirical. For example, to possess the concept of red, one would have to believe, indeed, to know, that red is a colour, that red is a feature of the surface of an object, that no surface, viewed from one position, can be two different colours at the same time, that surfaces are extended, that extended objects occupy space, and so on. These propositions are not empirical propositions; they are necessary, and knowable a priori. [Ludwig, 2004, 355]

As for Grandy's Principle of Humanity, according to which, if we are correctly to interpret others' beliefs, we must assume their actual and possible cognitive states to be similar to our own, Stich has succinctly objected that it is a chauvinistic principle: it merely proclaims that 'we ourselves are the measure of all things' and does nothing to disprove the possibility that "people's cognitive processes may be endlessly different from our own — and endlessly worse and endlessly better." Perhaps, Stich suggests, we only call beliefs 'real' beliefs and reasoning 'real' reasoning because we assume that they must be "reasonably similar to our own" [Stich, 1993, 52, 54].

And finally there is Hollis's bridgehead. This has also been called into question by Stich, who (following [Cherniak, 1986]) imagines a people with exotic feasibility orderings for inferences, orderings that are opposite to our own: those inferences we find easy they find hard, and vice versa, so that there would be no common ground of shared inference-making. But to this Hollis could reply, first, that such a thought experiment does not show that they could have massively inconsistent or incoherent beliefs; and, second, that we could not attribute concepts to them unless they were able to recognize the validity of the same simple inferences we do [Biro and Ludwig, 1994; Ludwig 2004]. But why should we, in any case, assume there to be a *fixed* (rather than 'floating') bridgehead — one that consists in a given range of true and rational beliefs? And how are we to arrive at an account of the components of the bridgehead, of the "percepts and concepts shared by all who can understand each other, together with judgments which all would make and rules of judgment which all subscribe to"? For, according to Hollis, if understanding is to be possible, "there must be, in Strawson's phrase, 'a massive central core of human thinking which has no history"' [Hollis, 1982, 75; Strawson, 1959, 10]. Is that true and, if so, how can we ascertain in what the core consists?

One way of approaching this daunting question is to return to the writings

of those anthropologists who, specializing in the study of often alien and apparently irrational beliefs, have reflected upon their means of access to them. One such is Robin Horton, whose work has been largely devoted to developing an appropriate framework for the interpretation of African religious thought [Horton, 1993a]. Horton offers an extremely interesting answer to the question of what it is that "provides the cross-cultural voyager with his intellectual bridgehead," namely what he calls 'primary theory,' which "really does not differ very much from community to community, or from culture to culture, " though there are different versions, covering differing areas of experience, while the overall framework remains constant. Primary theory

> gives the world a foreground filled with middle-sized (say between a hundred times as large and a hundred times as small as human beings), enduring, solid objects. These objects are inter-related, indeed inter-defined, in terms of a push-pull conception of causality, in which spatial and temporal contiguity are seen as crucial to the transmission of change. They are related spatially in terms of five dichotomies: 'left/right'; 'above/below'; 'in-front-of/behind'; 'inside/outside'; 'contiguous/separate'. And temporally in terms of one trichotomy: 'before/at the same time/after'. Finally, primary theory makes two major distinctions amongst its objects: first, that between human beings and other objects; and second, among human beings, that between self and others. [Horton, 1993b, 321]

Primary theory, according to Horton, gives us the world of what Oxford philosophers used to call 'middle-sized dry goods' — "the world of people, animals, sticks, stones, rocks, rivers, and so on. The entities which it posits are experienced as directly given. It is to be understood in the context of socially co-operative exploitation of the environment, mediated by language and manual technology" [Horton, 1993a, 11].

Furthermore, whereas "there is a remarkable degree of cross-cultural uniformity about the way in which the world is portrayed by primary theory, there is an equally remarkable degree of cross-cultural variation in the way it is portrayed in secondary theory." Here "differences of emphasis and degree give place to startling differences in kind as between community and community, culture and culture" [Horton, 1993b, 321]. Its entities are thought of as

> somehow 'hidden.' The idea of the 'hiddenness' of the entities and processes of secondary theory is as central to African thought about gods and spirits as it is to Western thought about particles, currents and waves. Again, when contemplated against the background furnished by primary theory, the entities and processes postulated by secondary theory present a peculiar mixture of familiarity and strangeness. Characteristically, they share some properties with their primary-theory counterparts, lack some which the latter possess, and have many other

> which the latter do not possess. Once more, this blend of the famil-
> iar and the strange is as characteristic of the gods and spiritual forces
> of African world-views as it is of the impersonal entities of Western
> world-views. [Horton, 1993b, 321-2]

Secondary theory is "built up by an analogical extension of the latter's resources, which results in the picture of a 'hidden' world underpinning the 'given' world of everyday. And some at least of its statements have to be given equivalents in primary-theoretical terms in order to make it applicable to the conduct of everyday life" and it is typically "laced with paradox" [Horton, 1993a, 11].

Horton's distinction sheds a revealing light upon the debate between localists and rationalists. On the one hand, it suggests grounds for dissenting from Hollis's and Strawson's conception of a "massive central core of human thinking which has no history." On the contrary, Horton, observes that primary theory

> is well tailored to the specific kind of hand-eye co-ordination charac-
> teristic of the human species and to the associated manual technology
> which has formed the main support of everyday life from the birth of
> the species down to the present day. [Horton, 1993a, 324]

On this basis he hypothesizes that it "must date back at least to the very early days of a co-operative manual technology" [Horton, 1993a, 324]. On the other hand, for the same reason, it also suggest a plausible basis for regarding Stich's speculation that "people's cognitive processes may be endlessly different from our own" as highly implausible. For

> for early human groups, the survival value of the cultural complex
> comprising co-operative manual technology and a language structured
> in terms of primary theory must have been immense. And the sur-
> vival value of all those genetic traits making for the type of cerebral
> organization capable of supporting such a complex must have been cor-
> respondingly great. So, given the working of natural selection on such
> traits over hundreds of thousands of years, the human species may well
> have come to have a central nervous system innately fitted, not just
> for co-operative manual technology, but for the primary-theoretical
> thought and discourse which is essential to it. [Horton, 1993b, 325]

Moreover, human biologists seem inclined to think that

> the brain has elements of genetically-programmed structure and phys-
> iology particularly fitted to seeing, thinking and talking in primary-
> theoretical terms. Again, the psycho-linguists, contemplating the ex-
> traordinary facility with which children learn primary-theoretical dis-
> course under a minimum of deliberate instruction, have felt compelled
> to invoke an element of genetic programming to account for this phe-
> nomenon. [Horton, 1993b, 325]

Pursuing this line of thought[7], we can suggest that the inter-cultural basis of reason and truth that constitutes the bridgehead is composed of the percepts and concepts of primary theory, though varying in its coverage of different areas of experience, and the primary norms or rules of reasoning and inference. Together these specify what is rational in what has been called 'the standard normative sense' [Scanlon, 1998, 19].[8] They enable us to identify and explain apparently irrational beliefs of all kinds — from simple and less simple mistakes of perception and reasoning to self-serving and wishful thinking and ideological bias to the vast and rich panoply of magical and religious beliefs that are to be found in all cultures including our own, though more central and pervasive in some than in others. Primary theory provides the resources out of which such beliefs are constructed, as they transcend its limitations, revealing 'hidden' realms of entities and processes. When they violate its rules of reasoning, they generate what are called 'mysteries' and miracles. Science refines, develops and rigorously adheres to such rules, whereas religion and magic develop others. These are secondary, or local, norms of reasoning, norms that dictate what is and what is not believable. The point of these is to render faith as justified belief and thereby to legitimate as rational the abundant apparently irrational deliverances of secondary theory.

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[7]This converges with Dan Sperber's account in terms of two kinds of belief: *intuitive* beliefs ("typically the product of spontaneous and unconscious perceptual and inferential processes") and *reflective* beliefs (in, for instance, myths, political ideologies and scientific theories). Sperber writes that intuitive beliefs

owe their rationality to essentially innate, hence universal, perceptual and inferential mechanisms; as a result, they do not vary dramatically, and are essentially mutually consistent or reconcilable across cultures. Those beliefs which vary across cultures to the extent of seeming irrational from another culture's point of view are typically reflective beliefs with a content that is partly mysterious to the believers themselves. Such beliefs are rationally held, not in virtue of their content, but in virtue of their source. That different people should trust different sources of beliefs — I, my educators, you, yours — is exactly what you would expect if they were all rational in the same way and in the same world, and merely located in different parts in this world [Sperber, 1996, 89, 91-2].

Where Sperber stresses the shared rationality of trust in authorities, I stress the diversity of the norms that exhibit that trust.

[8]Taking the idea of a reason as primitive, Scanlon defines it as 'a consideration that counts in favor' of something. The 'standard normative sense' invokes the idea of justification. To ask whether something is or is not a reason in this sense is to ask "whether it is a *good* reason — a consideration that really counts in favor of the thing in question," not whether it is or could be someone's 'operative reason', that is, whether someone takes it to be a reason [Scanlon, 1998, 17, 19].

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LANGUAGE AND TRANSLATION

David Henderson

I here discuss some philosophical debates and results that may provide some useful perspective for anthropologists and social scientists. I do not aspire to an exhaustive discussion of all relevant philosophical points, and doubt that that would be either tractable or useful. I will seek to provide a general perspective with implications for a wide range of issues. I leave aside aspects of language or translation that, while they may be of anthropological significance, are also of a sort regarding which anthropologists may be presumed to have significantly greater competence than philosophers.

1 TRANSLATION AND INTERPRETATION

What makes for a good, adequate, acceptable translation? Of course, one could respond matter-of-factly enough: "one that preserves meanings". But, I want to approach the question in a somewhat more epistemological way. Here I argue for the central claim that translation (its adequacy) cannot be well understood except as a part of a more encompassing interpretive and explanatory endeavor. As innocuous as this sounds, it has some significant implications, to be mentioned below. The basic idea is that translation is itself a typically necessary part of inquiry devoted to interpretive and explanatory accounts of individuals or folk — for example, to interpret what is done in some ceremony, one will commonly need to be able to translate what is said there and what is said about such ceremonies and about an indefinite range of associated matters. Ultimately, though, the epistemological adequacy or standing of those translations becomes dependent on (hostage to) the interpretive successes to which they give rise. *Interpretation and translation are holistically interdependent.*

Talk of "translating" or of "a translation" is often somewhat ambiguous. On the one hand, one might be producing a translation *for a language* — this is a matter of developing a general scheme or "manual" for the translation of one language into another. On the other hand, one might be producing a translation of some concrete linguistic production — producing a translation of some document or utterance. Both producing a translation scheme for a language and producing a concrete translation of some utterance or document must be understood as embedded within a larger interpretive and explanatory endeavor, of which these are typically important components. Interpretive understanding typically involves more than merely having a concrete translation of certain utterances or documents. Perhaps you are watching a little piece of a news program while pausing in some

store — George W. Bush is speaking about how he has made America and the
world safer by attacking Iraq. Another person also pauses, then utters, 'Bush est
tres intelligent'. Doubtless the correct translation of so simple a French sentence
is straightforward. But, just as certainly, one is immediately confronted with an
interpretive puzzle. Why would anyone be prompted to make such a statement
when hearing Bush's English prose? What is the person doing? Cracking a small
joke? Testing to see whether you are as stupid as you look? Perhaps the person is
a secret agent, and the sentence is the arbitrary code by which they are to iden-
tify a contact for some clandestine meeting. Interpretive understanding involves
more than merely having a concrete translation for the linguistic productions in
question, more than an application of some general scheme for translating some
public language — although these are certainly evidential bases for an interpretive
understanding. Instead, interpretations have to do with maters such as what is
done as well as what is said. Typically, one must appreciate a range of the relevant
agents' "intensional states" — their beliefs, desires, values, and the like — and
how these would have brought about the practice or practices in question.

1.1   The dependency of interpretation on translation

Let us reflect on some respects in which interpretation is dependent on transla-
tion (general and concrete). Translation is commonly a necessary part of most
interpretation and understanding. This is so because translation commonly deliv-
ers or provides the information that is crucial to an interpretative understanding.
Consider what translation — concrete translation — provides to the interpretive
understanding of a bit of action by an individual agent. Access to what may be ex-
pressed in language is needed for making the fine-grained discriminations of what
is done and thought.[1] Let's substitute in this example. Suppose that our neighbor
arrives home with a new and rather large sports utility vehicle — the sort of thing
that gets 14 (city) and 18 (highway) miles per gallon.[2] What was your neighbor
doing in choosing to buy or lease this vehicle? Was he or she attempting to fit
in with the good old boys at the office or tennis club? Providing for the safe city
transport of small children (insuring that, if someone gets hurt in an accident, it
will be "the other guy")? Attempting to do his or her part in the chore of changing
the environment? Showing a kind of patriotism? Taking advantage of accelerated
depreciation provisions in the business tax code? Perhaps it is a partial product
of a religious conviction that Jesus will soon return in the clouds. There are many
other possibilities, and variations on each. They all have to do with what owning
and driving that vehicle means to your neighbor. To interpretively understand
your neighbor's strange choice, one must sort out such matters. In so doing, one
arrives at a "thicker" or more substantive identification of what the neighbor has
done, and one jointly must come to be able to explain the why of the choice/action.
One might find that the neighbor believes that his or her choice would contribute

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[1] See [Davidson, 1984b, 146-8]; see also [Davidson, 1984a; 1984c; 1980].
[2] These numbers reflect the fuel efficiency of the 2003 Chevrolet Tahoe.

to environmental changes or to the demands on the country's military, and did not acquire the vehicle in order to do such things. One might find that the agent was sensitive to various social aspirations, and that these might have contributed, or that the neighbor had been obsessed with how exposed family members had been to injury in their previous vehicle. But, one has relatively little, relatively crude, relatively poor-grade, access to such matters unless one has the ability to translate (and interpret) their language and concrete utterances. Of course, you may already have some understanding of your neighbor's view on the relevant matters (deities, the environment, power, wealth, safety, what it is to be American, the economy, responsibility to others, ...). But this standing (and revisable) understanding is itself typically dependent on the translation and interpretation of past utterances, those of the neighbor and others.

Such points carry over at the level of the interpretive understanding of a social phenomenon. Just how do we understand the American love for extraordinarily large (on the face of it, pointlessly large) vehicles? Clearly, any such interpretive understanding depends on access to what is expressed in an indefinitely wide range of utterances on the part of the relevant subjects. The take-away lesson might be summarized thus:

Lesson 1: The fine distinctions made in interpreting what is done — in appreciating the perhaps multifaceted character of an action or set of actions — turns upon understanding in a correspondingly fine-grained way intentional states of the relevant agents. This typically cannot be done without access to what is expressed in the agent's language.

An important point regarding interpretation can be grasped when considering the following unanswerable question: What would an agent (just some arbitrary agent) do were the agent to hold some belief, say (for example) that yonder sits a rattlesnake? It is readily apparent that the question cannot be answered. There is no one determinate thing that an "arbitrary agent" would do. One cannot say "what an agent would do" without knowing what else the agent believes and desires. Does the agent desire to prove his faith in a God that will protect him? Or to provide a pictorial documentation of life in such-and-such a wildlife reserve, or to protect a three-year-old who is in tow? One can extend this line of questioning indefinitely — as the potentially relevant beliefs and desires are effectively infinite. Of course, the agent might desire several of these things. Further, for any constellation of desires, and even for any constellation of desires with varying strengths, it would be impossible to tell what the agent with such desires would do — unless we had some take on what further beliefs the agent in question also holds. Does the agent believe that there is unspent film in the camera? That the child is safely in the care of Billy Bob back at base camp? That such snakes are not indigenous to the area? That crossing the path of a rattler in the morning is bad luck? That such bad luck can be dissipated by twirling the snake above one's head exactly three times while saying "Get thee behind me, serpent"? What these possibilities suggest is a kind of holism of cognitive life and of action. What is thought and done will commonly be the joint product of a wide range of relevant thoughts and

considerations — all of one's beliefs and desires are potentially relevant to any decision, for what beliefs and desires are relevant to a given thought or action can depend on what other thoughts and beliefs one has. (Recollecting that one left one's bottle of scotch out on the table may (or may not) be a reason to return to camp, depending on whether one believes that Billy Bob — or one's three-year old — has certain qualities of character.) Associated with this holism of the cognitive, or of the mental, is a holism of agential interpretation. Given background beliefs and desires attributable to an agent, certain behaviors would "make sense" when viewed as such and such an action. Given that the agent believes that the rattlesnake is not indigenous to the region, reaching for the camera would not make sense as a step in the documentation of the "untouched" wildlife in the area, although it might as a step in the side project of documenting the introduction of exotic species.

These formulations in terms of "making sense" may themselves be misleading. What is important is that the various pieces themselves fit together as a whole for the agent — where fitting together need not be a matter of good, desirable, or ultimately appropriate contentful relations or reasoning. We know that human beings are prone to various forms of rationality and various forms of irrationality. These make a difference to how various constellations of beliefs and desires "add up" for a given agent. To agents subject to much uncertainty, looking for hope and hoping for someone with answers, easy stories and convenient formula may provide pieces that "fit together" to satisfy felt needs. That there are others to reinforce the story, assuring each other that it really makes sense, may also help. In any case, it is important that the various "pieces", the various beliefs, desires, hopes, aspirations, make a kind of recognizable sense — that the agents in question are responsive to this. Consequently, that the interpreter comes to find this "recognizable sense" in how the pieces fit together is important for interpretive success. We can then articulate a further take-away lesson:

Lesson 2: To the holism of the cognitive there answers a holism of interpretive understanding. An adequate interpretation of an agent's action is something of "a whole" that "brings together pieces" that must "fit together" to make "recognizable sense."

The above has been largely focused on the level of concrete translation as input to interpretation. Of course this requires that one have a general translation manual to be applied to the relevant agent or agent's utterances. Such a general scheme for translating a language has first to do with a public language, as language is a social phenomenon. This is reflected in recent philosophical treatments emphasizing a linguistic division of labor in which many users may employ concepts with remarkably little understanding of their semantic outlines; they do so while deferring to a community and expert practice that holds in place the concept [Putnam, 1975a]. I can refer to molybdenum, although I know remarkably little about that material; in so doing, I rely on the practices of a scientific community. Occasionally, one will need to conclude that the agent employs a term in an idiosyncratic fashion — one will need something of a special purpose scheme for

treating some piece of the agent's language. But, such modifications in the general purpose scheme for the translation of the natural language spoken by the agent will invariably be isolated. (Of course, dialects may emerge with the fragmentation of linguistic communities.)

The general point, that a general scheme for translation is a necessary resource for the generation of concrete translations which provide necessary input into the interpretation of particulars about individuals (their actions, their beliefs, their motives) carries over to the social level. What a certain social practice amounts to (for example), cannot be determined without a reasonable translation of much of the language that is associated with the practice. In many classical pieces of cultural anthropology, a field investigator struggles to understand certain puzzling practices — say the practices that revolve around the poisoning of young chickens in an apparently ceremonial setting [Evans-Pritchard, 1937]. To have any hope of piecing together an interpretation of such practices, the anthropologist clearly must draw upon numerous concrete translations for the utterances of the people involved, and of helpful informants who may provide commentary concerning the roles and histories of the folk involved. Only then can one appreciate the pattern of questions posed within the ceremony, the apparent attestations of the people involved, and much relevant background such as what misfortunes seem to precipitate the ceremony or whether there are patterns of longstanding social tensions between the parties involved. Famously, all such matters have seemed relevant to understanding what may transpire in the ceremony for which Evans-Pritchard sought an interpretation or understanding. The concrete translations pertaining to or revealing such matters — and which then provide resources for addressing such a difficult case — presumably constitute a resource arrived at on the basis of a generally useful translation manual. This general scheme, and the concrete translations it provides, may need to be refined (more on this soon), but, preliminary to any revisions, it provides working hypotheses regarding crucial information for any interpretation. One may subsequently develop a refined interpretation that has such approximate first translations treated as indirect expressions of what one ultimately finds transpiring. Thus, to be just a little more concrete, suppose that one ends by concluding that the ceremony is valued, at least in part, because it allows a kind of mediation for tensions within the community. Presumably one would only be drawn to this interpretation were one to have concrete translation-based evidence that the parties drawn into the ceremonial context were themselves parties to conflicts of recognizable sorts. Further, the plausibility of this interpretation would also depend on being able to understand what seems on the face of it to be said (the translations of the utterances made within the ceremony) as workable expressions of such tensions and their mediation. (Here I think that it is best to keep distinct (if however, related) the question of translation from the question of interpreting what is expressed. Consider a woman who asserts that her (perhaps former) boyfriend "is a pig." We have no problem treating her expression homophonically (translation) — and at the same time understanding it as expressing something that does not require his full membership in the set of porcine creatures

for the truth of what is expressed (interpretation). While interpretation is not the same thing as translation here, they are clearly intertwined, as the interpretation rests on many translations and turns on the reasonable understanding of what is said under translation, and of what is thereby expressed.)

1.2   The dependency of translation on interpretation

I have been emphasizing in a schematic fashion the dependency of interpretation on translation, and thus how translation must be understood as a part of the interpretive endeavor. There is a dependency in the opposite direction that must also be appreciated: success or adequacy of translation is itself dependent on the interpretive success to which it contributes. The basic point can be approached by supposing that one is applying a generally workable translation scheme for some natural language. Using this scheme, suppose that one begins to encounter certain striking problems in the interpretations of some range of practices and the associated language — that is, what seems prima facie indicated by the concrete translations yielded when applying that general translation scheme leads to interpretive difficulties. This rightly undermines one's confidence in one's general translation manual, at least with respect to the translations of linguistic productions associated with the relevant practices.

Consider a simple case. Suppose that I am interacting with a judicious colleague — a person of careful and balanced expression. In fact, suppose that I have come to judge that my colleague is judicious while employing a largely homophonic translation manual. We can suppose that I have noted some isolated cases were I need to use a nonhomophonic translation scheme, treating her term 'courgette' as equivalent to my 'zucchini', for example. But, now suppose that my colleague asserts that, "This candidate looks quite good", but gives evidence that does not support what would seem to be a high evaluation. Suppose also that my colleague looks pained when I say that I thought her paper was "quite good", as though she had hoped for yet stronger approval. These, and other such cases, may lead me to entertain the hypothesis that the word 'quite' is used differently in my colleague's language than it is in my American-English. The interpretive implausibility of attributing injudicious beliefs to my colleague (who has otherwise seemed so judicious) thus prompts me to revise my translation manual — interpretive difficulties undermine my original translations.

Less close to home, there is the much commented upon case of Tully River natives who seemed under translation to advance an account of the antecedents of pregnancy that would strongly suggest that they were ignorant of the most rudimentary biological facts. (This case, which as the focus of a dispute between Sprio [1968] and Leach [1969], is discussed in [Turner, 1980; Lukes, 1982; Henderson, 1993].) The ignorance seems highly implausible, given the active interest that human societies naturally take in such matters. Thus, the translation raises an interpretive problem. The difficulty might be solved by refining either the interpretation or the translation. Refining one's interpretation, one might insist that

those people really are ignorant of the biological antecedents of pregnancy, then go on to fill out one's interpretive understanding by providing an adequate explanation concerning why they would be so. Such a story would presumably allow us to appreciate some unusual or distinctive state of affairs involving a people who are no more limited or flawed than humans generally. (Such was Spiro's project.) Alternatively, one might interpret these people as literally saying what they seem (given our translation), but thereby expressing something reasonable (perhaps the events cited have more to do with ways of announcing pregnancy than events thought to be causal antecedents). (Such was Leech's proposal.) Until one such interpretive option proves satisfactory, one must recognize that the translation of native utterances *may* itself be flawed, and may be responsible for the interpretive difficulties encountered. Perhaps there are variations in word use that are not distinguished in our translation scheme and reflected in the resulting translation. Were this so, then a better scheme for translation would produce different concrete translations, ones that do not even suggest a possible ignorance of the biological bases of pregnancy. In view of such cases, it is clear that:

Lesson 3: Confidence in translation (general and concrete) is rightly hostage, or keyed, to interpretive adequacy, which is itself associated with explanatory adequacy.

1.3 Reconstructive Translation and Conceptual Schemes

I have been advancing dual theses. First, that translation is commonly necessary for interpretation — providing presumptive pieces of information on which interesting interpretations must draw in understanding meaningful action. Second, that success and adequacy of interpretation is a mark of the ultimate adequacy or satisfactory character of the translations that inform or suggest an interpretation. Dogged inadequacy of interpretation must call into question the associated translation. Put simply, translation and interpretation are intertwined and are ultimately moments in a holistic inquiry directed to understanding others. As a final illustration of such themes, I want to take up an issue that might at first seem something of an aside: what to make of the idea of a *conceptual scheme.* Of course, this issue is interesting of itself — and I hope that I have something useful to contribute here. The discussion to follow also bears, on the issues that I have just been discussing. The evidential interdependence of translation and interpretation is strikingly reflected in a sort of case that had led some to write or talk of conceptual schemes. The cases in question are characterized by certain difficulties of translation. I will argue that, in these cases, the projects of translation and interpretation fuse, or at least become so intertwined as to be difficult to distinguish — these are cases of what I have elsewhere termed *reconstructive translation* (Henderson 1994). The idea of a reconstructive translation will, I think, help us appreciate what philosophical sense might be made of the idea of a conceptual scheme.

In "The Very Idea of a Conceptual Scheme," the philosopher Donald Davidson (1984d) famously criticized the idea of a conceptual scheme as unintelligible, accusing those who posit conceptual schemes — notably, Quine [1960; 1981], Kuhn [1970], and Whorf [1956] — of a kind of confused dogmatism. He argues that such talk is either hyperbole or incoherent. On the one hand, he explains, there is little to be excited about in the real differences that are sometimes alluded to in connection with talk of conceptual schemes. At least the real differences are not so far-reaching as proponents of conceptual schemes have imagined. On the other hand, he insists, it is incoherent to think that one might have a good reason to suppose that there are the radical differences envisioned in much talk of conceptual schemes.

We can begin with some points of wide agreement concerning what would make for conceptual schemes. First, conceptual schemes are like points of view, whether there are any, just one, or many, *if* there *is* one, there *could* be many. Second, as the terminology itself suggests, conceptual schemes are associated with concepts. If two people employ the same concepts, they presumably are using the same conceptual scheme. If two people have different conceptual schemes, they are employing a significantly different set of concepts. Now, concepts are, it seems, semantic entities. One says that the French word 'chien' expresses the same concept as the English word 'dog' because these words have pretty much the same semantics. Such semantics is to be preserved in translation, so concepts are to be preserved in translation. Thus, for two people or peoples to employ different concepts is to employ two languages, where the one is not readily translatable into the other. In any case, some association of conceptual schemes with languages has been central to friends of schemes as different as Quine and Whorf.

Davidson takes up the suggested intranslatability, proposing that conceptual schemes be identified with sets of intertranslatable languages, and that intranslatability of languages provides a necessary condition for scheme differentiation. However, the matter may not be so straightforward, if one is to be fair to those who have written of conceptual schemes. Quine [1960, 76-77; 1981, 41-2], for example, characterizes differentiation of schemes in terms of certain sorts of *differences that show up under translation*. Further, those whose discussion does suggest that scheme identification and individuation turns on translatability may be employing a defensible notion of translation that is importantly different from Davidson's.

Davidson's argument proceeds by establishing a lemma to the effect that there is no "criterion of languagehood" that does not "depend on, or entail, translatability into familiar idiom" [1984d, 192]. From this he concludes that there cannot be untranslatable languages, and thus there cannot be alternative conceptual schemes. But one should be careful here. It seems plausible that there may indeed be some notion of "translatability" which can feature as a criterion of languagehood. It also seems plausible that there is some sense in which what has seemed fairly deep conceptual difference may be marked by failures of "ready translatability" or of "translatability in the most straightforward or strict sense." But, it is not at all clear that the notion of translatability that might be associated with languagehood

is or must be the same notion of (ready or straightforward) translatability that turns out to be intimately associated with conceptual scheme identification and individuation.

To be fair to Davidson we must notice that his argument does not so flatly turn on a definition (of conceptual schemes in terms of untranslatable languages) as suggested above. Davidson also argues that translation is constrained by a strong principle of charity — a principle requiring translators and interpreters to find significant agreement (and rationality) under translation or interpretation. When the agreement turned up in such inquiry is sufficient to satisfy the standards for producing adequate interpretation, Davidson would insist, there is room for only very limited conceptual differences, only very limited difference in belief, and only very limited differences in reasoning. If the allowable or attributable differences were thought to make for difference in conceptual scheme, the presence of such alternative schemes would be unexciting. To adequately assess this point, it will be necessary to delineate a sort of translation that friends of conceptual schemes would think allows us to uncover interesting cases.

Davidson's position mistakenly papers over the real possibility of rather deep differences in beliefs, theories, and concepts — the sorts of differences that have interested many proponents of conceptual schemes. Davidson views Whorf's insistence that Hopi and English cannot be "calibrated" as a case of positing untranslatable languages, and as indicating that, for Whorf, such intranslatability is necessary for conceptual scheme differentiation [Davidson, 1984d, 190]. However, this squares poorly with Whorf's own [1956] discussion in which he freely writes of "the character of the phenomena denoted" by certain verbs, seeks to express "the nature of the change" affected by particular modifying particles, and illustrates the relevant shifts in content using examples in which short Hopi phrases are rendered by relatively sprawling English phrases. The results seem to be what Davidson would recognize as translation. Accordingly, Davidson takes Whorf's own practice to obviously undermine the latter's assertion of intranslatability and insistence that Hopi expresses an alternative conceptual scheme. This is analogous to viewing someone as denying that she is walking even as she is walking. By Davidson's own account of interpretation, this should provide prima facie reason to believe that his account of Whorf is too facile, whatever one ultimately wants to say about Whorf's own work. Surely to see Whorf as providing a translation in the course of arguing that two languages are flatly untranslatable would attribute a sort of silliness that is less likely than poor interpretation.

The implication is clear. We may suppose, with Davidson, that the difficulties that Whorf associates with translation across conceptual schemes, and that Whorf refers to as difficulties or failures of "calibration," constitute one sort of intranslatability. However, the relevant sort of translatability (that associated with ease of "calibration") must be taken as of a particularly "smooth" or "easy" sort — one that plausibly obtains between languages expressing the same conceptual scheme. Immediately, we should add that there are sorts of translatability that do not entail such smooth or ready equivalences — such ease of "calibration."

Perhaps better, we should suspect that the two "sorts" really represent two poles in a continuum of cases in which translation proceeds more or less smoothly by way of ready equivalences. We should charitably seek to delineate, for Whorf and other friends of conceptual schemes, these polar kinds of cases of translation. Then, with Davidson, one might associated translatability of any of these forms — ready or not, smooth or not — with languagehood. At least such a standard (for conceptual schemes in terms of translatability) becomes plausible here (once we have recognized the range of cases that count as translation generally). At the same time, we might associate smooth translation with conceptual scheme identity, and "bumpy" translation, or lack of ready translation, with conceptual scheme individuation.[3]

Davidson criticizes Quine and Kuhn for talking of conceptual schemes while (by his lights, inconsistently) providing for their exposition under translation. But, Quine and Kuhn also seem concerned with the difference between "smooth" or ready translation and difficult or "bumpy" translation. Far from associating paradigms (as conceptual schemes) with intranslatability, Kuhn [1970, 202] insists that the "communication breakdowns" that arise between investigators employing "incommensurable paradigms" can and should provide the occasion for translation. Quine clearly also allows for awkward translation between languages expressing alternative conceptual schemes.

Thus, to appreciate what there might be to the idea of a conceptual scheme, we apparently need to explicate a sort of translatability that, in not being "smooth" or a matter of ready equivalences, marks differences in conceptual schemes. One hint is provided by Quine [1960; 1970; 1981]. While Quine is wary of both posited cultural universals and posited radical differences in conceptual schemes [1960, 77; 1970, 9-11], he does think that there can be crude measures of real cultural differences. His basic idea is that once a translation scheme has been rigged that duly makes our informants out to be conveying generally plausible messages [1970, 1-19], the extent to which we have needed to employ as translations gerrymandered constructions in our home language provides a measure of linguistic differences:

> If we find a language hard to translate, if we find very little word-
> by-word isomorphism with genuine and idiomatic English, then we
> already have right there, in a featureless sort of way, a kind of measure
> of remoteness [1970, 15].

Developing this idea, Quine suggests a "measure of what might be called the remoteness of a conceptual scheme but what might better be called the conceptual difference between languages" [1981, 41-2]. This treatment of "conceptual schemes", which has seen a consistent development since Quine [1960], makes the unqualified suggestion that Quine adheres to a notion of such schemes wedded to intranslatability extremely uncharitable. Again, we must look for a way of de-

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[3]The idea that we should distinguish between two interpretive projects and relate them differently to criteria of languagehood and conceptual schemes has at least one precedent: Rescher [1980].

veloping the more guarded suggestion that conceptual scheme differentiation is associated with certain classes of difficulties or awkwardness in translation.

Quine suggests that there might be deep differences in theories that would result in many of the central concepts of the one theory having no ready parallels in the other. Were we to translate the languages expressing the two theories, we would need to resort to a loose sort of translation in which we "coin new words or distort the usage of old ones" [Quine, 1960, 76]. The reason one would need to do so is that *one would need to reconstruct, within the expressive resources of one's own-language, something of the theory (and central concepts) being translated.* Accordingly, I call such translation *reconstructive translation.*

In setting out a general account of this sort of translational endeavor, a concrete example to which we might recur would be helpful. Early on in *Witchcraft, Oracles and Magic Among the Azande*, Evans-Pritchard [1937, 8-11] sets out how he proposes to treat Zande terms and notions. He provides a table in which he sets out certain central Zande terms, English phrases that he employs as somewhat *uneasy stand-ins* (rough translations) for each of these, and short paragraphs providing "condensed" characterization of the notions in question. The translational stand-ins really serve as proxies for the condensed characterizations. Further, Evans-Pritchard remarks that even the "formal and condensed definitions" that he provides merely facilitate reading his work, forestalling misunderstandings that would otherwise arise from over reliance on the uneasy stand-ins, while a yet more adequate characterization of the Zande notions only emerges in the course of the monograph as a whole. Here is an excerpt from his table [1937, 9]:

Mangu   (1) WITCHCRAFT SUBSTANCE: a material substance in the bodies of certain persons. It is discovered by autopsy in the dead and is supposed to be diagnosed by oracles in the living.

(2) WITCHCRAFT: a supposed psychic emanation from witchcraft-substance which is believed to cause injury to health and property.

Ngua   (1) MAGIC: a technique that is supposed to achieve its purpose by the use of medicines. The operation of these medicines is a magic rite and is usually accompanied by a spell.

(2) MEDICINES: any object in which mystical power is supposed to reside and which is used in magic rites. They are usually of vegetable nature.

There is much that is notable here. But let us begin with this: suppose I were to ask someone with a common Western cultural background, and who had not been exposed to this material already, what word we used to express the concept of an inherited substance in the bodies of certain persons enabling them to cause injury to others by thinking ill of the harmed person. One so queried would typically be at a loss for an answer. The reason is that Evans-Pritchard's glosses reflect the manner in which the Zande concept of *mangu* is embedded in a web of

"theory",[4] and *we do not antecedently have the relevant theory. A fortiori, we do not antecedently possess* either *the Zande concept* or a word for it. Insofar as we have a concept associated with the word 'witchcraft', it is not the Zande notion set out here.[5] In casting about for a clear parallel to the Zande notion of *mangu*, Evans-Pritchard finds none; so, in *reconstructing the relevant portions of the Zande theory, in reconstructing their concept, he finds it necessary to employ a "warped usage"*, if he is to have a short word as the translation of the Zande word.[6]

Obviously, thinking of a simple translation employing Evans-Pritchard's stand-ins, without also keeping in mind his longer glosses and his monograph as a whole, could be quite misleading. *It is the systematic set of reconstructions that really carry the weight in the interpretations produced here.*

Contrast our satisfaction with 'Snow is white' as a self-contained translation of 'Der Schnee ist weiss'. Due to structural similarities in taxonomies and associations — which we associate with antecedently sharing concepts across languages — such translations pretty much stand on their own. We can take the comparably short gloss provided here as a direct translation of the German sentence, and we take the indicated equivalence at face value. *Such direct translation, being associated with ready parallels and shared concepts, is the sort of translation that might plausibly be associated with conceptual scheme identification.*

In contrast to cases of direct translatability, when we employ Evans-Pritchard's stand-ins to arrive at a translation such as, 'He is bewitched by his neighbor', we can only know what to make of that stand-in by knowing the longer reconstructive gloss for which it serves as a shorthand. We would caution others that the English word 'bewitched' as used here is not to be understood as expressing *our* traditional concept of "being acted on by witchcraft"; rather, it points to a Zande notion

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[4]I realize that formulating my account of reconstructive translation in terms of the reconstruction of "theory" and embedded concepts will touch sensitive nerves. It may seem to beg the question on several points that have long been contested in both anthropological and philosophical debates concerning interpretations. Both neo-Wittgensteinians (Winch 1958, 1964) and symbolist anthropologists (such as Leach 1954, Beattie 1964, and Firth 1964) have insisted that religious and magical symbol systems develop with a dynamic quite dissimilar to scientific systems. However, both tend to associate "theory" rather narrowly with scientific theorizing. This much will need to suffice here: whatever the virtues of the contention that religious and related systems develop according to a different dynamic, symbolists themselves typically allow that the systems, at any one time, come to have technological-instrumental usages, and, in many ways, they function in individuals' lives like other sets of "beliefs." As long as we do not too narrowly associate "theory" with scientific theory, then, symbolist reservations can be accommodated.

[5]For instance, the Zande have no role for pacts with some supremely evil being, nor is *mangu* associated with acquired skills at using incantations, potions, trinkets, and so forth. In these respects, traditional western concepts of witchcraft seem somewhat closer to the Zande concept of *ngue*, which itself seems yet more closely parallel to our traditional notion of magic–insofar as the latter is separable from witchcraft in our traditional thought.

[6]Evans-Pritchard's translational stand-ins for *mangu* and *ngue* force us to parallel Zande usage in translation by thinking of witchcraft and witches as largely distinct from magic and magicians. In so doing, Evans-Pritchard's translation itself accentuates differences (as well as similarities) between the Zande "theory" and our traditional "theories".

treated in Evans-Pritchard's reconstruction of an extensive family of concepts and embedding theory. Here, we have a *translation in an indirect or extended sense*.

I find it plausible that the need for such reconstructive translation, where "we find very little word-by-word isomorphism with genuine and idiomatic English", provides "a kind of measure of [conceptual] remoteness" where we must "coin new words or distort the usage of old ones" if we are to have short stand-ins for the foreign terms. But, for my purposes here, reconstructive translation is particularly significant because it makes most vivid the interdependence of translation and interpretation. Evans-Pritchard's *Witchcraft, Oracles and Magic Among the Azande*, the book taken as a whole, constitutes his attempt at interpretively understanding the relevant range of Zande practices. As we have just seen, it also constitutes his suggested reconstructive translation for the relevant portions of the Zande language. It is, in short, interpretation and translation rolled into one. If it fails as interpretation, it fails as reconstructive translation. Its failure as translation would constitute its failure as interpretation. In such cases, interpreting and explanatory understanding of the relevant practices is not to be separated from translational understanding of the associated language.

1.4   Methodological Constraints in Interpretation and Translation: Explanation

I have been highlighting the intimate relation between translation and interpretation, taking note of aspects of their interdependence. Appreciating these matters puts us in a good position to consider a matter with far-reaching implications both in philosophy and in anthropological practice. Philosophers have sought distill out of the practice of constructing a general scheme for translation the central methodological constraints on that endeavor.

Evidence and the principle of charity. In a series of influential writings, the philosopher W. V. O. Quine sought to understand the character of translation by thinking of a linguist coming without intermediaries into a strange linguistic community (see especially [Quine, 1960, chapter 2]). He imagined attempting to construct a translation manual on the basis of patterns in the informants' *speech dispositions*. Patterns of volunteered utterances, and (once the words for assent and dissent were tentatively identified) patterns of assent and dissent, patterns of associations with salient presentations in the immediate environment — these were to serve as the ultimate data for constructing a "translation manual" for the "native language." Quine found it useful to think in terms of what he termed the "stimulus meaning" of a sentence: the stimulus meaning of a sentence is constituted by the set of stimulations that would prompt assent, and the set that would prompt dissent. He used stimulus meaning as a kind of initial *ersatz* meaning. This provided Quine with a somewhat stylized, but uncontroversially real, reference-point — a decent take on what evidentially anchors the development of a scheme for translation, a basis whose reality few would question. Of course, meaning, as commonly conceived, is much richer than stimulus meaning. The difference

between stimulus meaning — this ersatz meaning with its clear evidential warrant — and meaning as commonly conceived provides Quine with a way of gauging the evidential underpinnings of translation-based attribution of meaning in the richer (common) sense. He himself is skeptical, doubting that the fine discriminations one would intuitively draw when thinking about meaning are really supportable. The matter would seem to depend in some measure on what constraints there are on the project of constructing a translation manual from the evidential basis afforded by (let us suppose full and ideal) information about speech dispositions and stimulus meaning.

For some sentences, stimulus meaning takes one farther towards something resembling translation than for others, as Quine detailed. What Quine termed "occasion sentences", are those sentences for which assent and dissent will turn largely on concurrent "stimulations", concurrent salient environmental presentations. (The sentence, 'That is a dog' is an occasion sentence, while 'Automobiles are heavy' and 'God is good' are not, while the sentence 'The mail has come today' is intermediate. See, [Quine 1960, chapter 2], for details.) For occasion sentences, similarity of stimulus meaning provided some significant basis for identifying or associating sentences, and this basis is even more decisive where all or almost all in a speech community would agree having the same stimulus meaning for the sentence — Quine termed such sentences "observation sentences" ('That is a dog' has pretty much the same stimulus meaning for all speakers of English. 'C'est un chien' is similarly an observation sentence within French. The similarity of the stimulus meaning of the two sentences in their respective languages provides a strong basis for translating the one as the other.) But, such dispositions to prompted assent and dissent would do little for sentences not so associated with immediate and salient environmental matters. Once a speaker has come to be disposed to assent to such a sentence, for example, 'Dogs bark' the speaker would assent to it, if to anything, in ways that are largely independent of circumstances, with obvious general classes of exceptions. Thus, for one who will assent to 'Dogs bark' and 'God is good', there is little in their patterns of assent in the face of salient external stimuli that will tell them apart, or indicate which is the appropriate translation for 'Les chiens eboient'. Such "standing sentences" only yield to something resembling translation when one goes beyond drawing rough equivalences between sentences and works instead with sentential components (words or phrases, and grammatical composition). Such moves represent an attempt to capture a kind of "interanimation" of sentences — dispositions with respect to how (dispositions to) assent to or dissent from sentences affect or condition (dispositions to) assent to or dissent from other sentences. This interanimation of sentences makes for what meaning or significance is had by such standing sentences. Sentential components (words or phrases) are significant insofar as they systematically contribute to the significance had by their containing sentences.

I have needed to write the above presentation of central elements in Quine's thought regarding translation using circumspect formulations, for Quine himself holds that the constraints on translation are not themselves adequate to determine

a single correct manual of translation, and that two translation manuals may be equally and maximally good and yet differ in yielding translations for a given sentence that are by no account equivalent in meaning, intuitively understood. Such is Quine's thesis of *the indeterminacy of translation*. It is not easy to refute, and I have not been able to reach a considered opinion on its merits. I do think that the extent of real indeterminacy is not such as needs hobble practicing social scientists. So, I set the question aside for our purposes here.

For now, what I want to take away from Quine's discussion is the simple idea that patterns of speech dispositions are central evidence for constructing a scheme for translating two languages. For sentences that are keyed relatively directly to what is then at hand in one's environment, such dispositions can be revealing. But, even then, there can remain much sorting out to do. Certainly, in moving beyond the crudest equivalences, one must attend to the way dispositions to sentences are keyed to each other, and recurring components. Quine invites us to reflect on the most general constraints that seem to inform this enterprise. Famously, he suggest that a central constraint can be represented by what he terms the *Principle of Charity* — roughly, we are to so translate as to find our subjects to be believers of mostly true things and to be importantly rational. Later, Davidson insists that interpretation and translation are subject to a charitable constraint requiring us to optimize agreement under interpretation. Such charity is already in evidence when drawing a tentative equivalence between 'C'est un chien' and an "observation sentence" with very close stimulus meaning, 'That is a dog'. This will lead to finding significant agreement under translation. It is likewise in evidence when one identifies the other language's particles for conjunction ('and'), disjunction ('or') and the like by attending to the inferential patterns associated with certain short vocables. Doing so will obviously lead one to attribute significant inferential rationality.

Explicability, a fundamental constraint. I will not belabor the point: charity in translation represents a significant constraint. One must, nonetheless, be careful here. There are various ways to understand the constraint. It might be understood as a fundamental constraint, as seems to be Davidson's [1984a; 1984b; 1984c] understanding. Or, it might be understood as a derivative constraint, an understanding suggested by Quine [1970]; Lukes [1982]; Henderson [1987; 1990; 1993], and Risjord [2000]. What I now want to notice is the way in which the understanding of charity as a real but derivative constraint, (1) fits nicely with the above points about the interdependencies of translation and interpretation, and (2) thereby allows us to understand certain interpretive controversies. This integrative perspective is made possible once we see that in interpretation one seeks an explanatory understanding. One seeks an explanatory understanding of why certain folk do what they seem to do, believe as they seem to believe, and so on. Here we may return to a case discussed by Lukes [1982] and Turner [1980], the controversy between symbolists such as Leach [1954; 1969] and intellectualist anthropologists such as Spiro [1966; 1968] concerning the proper understanding of the Tully River natives mentioned above. Again, the natives seem, under translation, to be igno-

rant of the biological bases of pregnancy. In view of the common appreciation of the rudimentary "fact of life" across variously situated human groups, such ignorance would seem to border on an irrational failure to "put certain facts together" to derive the obvious conclusions. This seems to violate the principle of charity. Is that what one finds problematic? The symbolist response is to see the apparent reports as not really reports at all, but rather bits of practice that function to express deep values and attitudes. On such a view, we do not find irrational belief here, for we do not find expressions of belief. To think otherwise would be like accusing Akira Kurosawa of irrationally bad history in *The Seven Samurai*. But this is not the end of the matter, even on most symbolist accounts. Watching one's subjects, it is difficult to escape the idea that what may have arisen as expressive comes to function rather like a belief. Thus, for example, Geertz concedes: "Certainly I was struck in my own work, much more than I expected to be, by the degree to which my more animistically inclined informants behaved like true Tyloreans. They seemed to be constantly using their beliefs to 'explain' phenomena: or, more accurately, to convince themselves that the phenomena were explainable within the accepted scheme of things ..." [1973, 101]. But, coming to believe what would have functioned expressively would itself seem a kind of irrationality. (Consider what one would think of people who came to believe historically accurate those movies which they found expressively compelling — for example, were I to believe that things really happened just as portrayed in *The Seven Samurai* or *Dr. Strangelove* or *Being There*.) Spiro, on the other hand, suggests that the natives do possess false beliefs regarding the matter, and that these are the result of certain cognitive tendencies of a sort like those studied in psychoanalysis. Leech objects that this explanation does not seem plausible, and does not account for why these natives would be ignorant in this rather unusual way. Now, skipping much that could be said by way of dialectical refinements, the general point is that the debate here does not seem to turn on which account would avoid all attributions of irrationality. Symbolists as well as intellectualists commonly get around to attributing some forms of irrationality to those they seek to understand. Instead, *the issue seems to turn on which account makes those studied out to be explicable.* Attributions of inexplicably unusual irrationality — where it is quite implausible that normal human beings would so think or act — seem to undermine interpretations and translations. But a form of common irrationality — one of a sort to which we have good reasons to believe that human beings are subject — is no strike against an interpretation and translation. Of course, we expect certain forms of rationality on the part of ourselves and others, and we are constantly revising our understanding of human cognitive capacities and foibles. Such understandings appropriately condition interpretation, and thus translation. Accordingly, one should conclude that the fundamental principle constraining interpretation and translation is what I have elsewhere termed the *Principle of Explicability:* so interpret as to make one's subject out to be explicable in thought, word, and deed [Henderson, 1993], see also [Risjord, 2000]. Interpretation is constantly being informed by our understanding of humans, which is constantly being refined in terms of dif-

ficulties encountered in interpretation. Since we have expectations for significant rationality (and for nonneglible irrationality), the principle of charity follows as a defeasible, derivative, principle — one subject to ongoing refinement, but always as approximate and open as our evolving understandings of human beings.

2   MEANING, EXTERNALISTIC SEMANTICS, AND TRANSLATION

In this section I want to mention several points from the philosophical literature on what has come to be called "wide content" or "externalistic semantics". Much thinking about content or meaning has supposed that the content of an agent's thoughts could be determined by what went on "within that agent's head" — that the content of the agent's thoughts was dependent on something about that agent alone, perhaps the functional relations among the individual's brain states, and between these and certain sensory states. This view came in for significant challenge beginning with work by Putnam and Kripke in the 1970s. The alternative view was that the content of one's thoughts at least often depended on facts about the individual's community and environment. This was dubbed "wide content" or "externalist semantics." There continues to be much debate over whether all content is "wide", and whether there might be some "internalist" or narrow core.[7] I will not attempt to settle such issues here, but simply try to indicate respects in which externalist semantics may be important for how we can or should translate and interpret others.

2.1   A Starting Place

Since Quine wrote the bulk of his work, the philosophy of mind has arguably come to replace the philosophy of language as the more philosophically central area. Discussions of meaning in language are now readily transformed into discussions of the content of mental states. On balance, this is probably for the best. But, even as the focus of philosophical concern has shifted, some ideas to be found in the work of philosophers such as Quine and Davidson have proved remarkably resilient (although how they are to fit in to a full story of about meaning or content

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[7]One line of debate concerning the significance of externalistic semantics has to do with whether it is applicable beyond a restricted range of concepts — such as the "natural kind concepts" that have commonly served as illustrations (*water, gold, atom* and *elm* have provided standard fare). One finds such concerns reflected in Bealer [1987], for example. A related issue concerns the extent to which externalist insights might themselves be incorporated within an internalistic semantics, one that treats the agent as referring in ways that are internalistically grounded — for example, the individual might refer to kinds internalistically characterized in ways that are reflected in externalist talk of "relevant experts", homogeneous stuffs" and historical interactions and usage. In a related way, some writers seek to find an internalistically accessible core to the semantics of concepts (for example, [Chalmers, 1996; 2002a; 2002b], see also Jackson [1998] and Peacocke [1992]. The points on which I understand myself as relying in this entry would seem to be such as ultimately might be incorporated within an externalist or an internalist approach (although some of my formulations might on their face be more congenial to the externalist).

will be contested, as we will see). As reflected above, Quine approached language and translation as turning on elaborately interrelated dispositions (to utterance or assent and dissent). When we turn to the philosophy of mind, we find that much about the content of mental states still seems to turn on the structure of dispositions, at least in some measure. Davidson, developing certain themes from Quine, discusses what he terms "radical interpretation." As Davidson points out, in interpreting a given agent, three matters must be sorted out jointly: beliefs, desires, and the meaning of the actions and utterances that result. What serves as evidence are the agent's behaviors — verbal and other. There are obviously patterns and structure to these dispositions. Some (and only some) of the relevant patterns and dispositions were highlighted in Quine's discussion of translation. Somehow, the three unknowns (beliefs, desires, meanings) must be sorted out on the basis of this limited evidence. For Quine, linguistic meaning, or at least what there is to meaning, was to turn on a web of conditionally triggered dispositions to assent, dissent, and utterance involving many sentences. For Davidson, patterns of conditional dependence or triggering by internal states of internal states and of acts and utterances are crucial when thinking of beliefs and desires. Which internal states count as beliefs depends on their relation to other states (which themselves count as beliefs, desires, and the like, by virtue of their factual and counterfactual dependencies) — beliefs and desires causally interact in certain systematic ways, ways reflected in everyday intentional psychology. Further, the content of a belief is a matter of such patterned dependencies: "a belief is identified by its location in a pattern of beliefs. It is the pattern that determines the subject matter of the belief, what it is about" [Davidson, 1984c, 168]. Similarly for the content of states that count as desires. It is as though the patterns in speech dispositions to which Quine points are themselves reflections of patterns in internal states interacting in ways crudely captured in commonsense belief-desire psychology. Meaning, that is to say, the content of beliefs and desires (as paradigmatically contentful mental states), is here understood as a matter of those states being conditioned by each other in ways that roughly mirror what is contentfully appropriate.[8]

   To illustrate: consider what it is to have the belief *that water has a high specific heat.* This requires having beliefs about water (one of which is the belief in question). This in turn requires having beliefs about instances of water that are prompted by salient samples in one's common environment, about common sources appropriate to one's environment, about what can be done with it (especially drinking). Were there not some significant range of such dispositions had by an agent, it would seem inappropriate to attribute beliefs about water to that agent. Further, to have the belief that water has a high specific heat would also require beliefs about heat (as an extensive physical magnitude) — beliefs that involve comparing amounts of heat, at least crudely. (One might qualify as having beliefs about heat when one associated a magnitude related to perceived warmth with relative amounts of a given fuel consumed, and tendencies of different fuels to do certain recurring tasks, such as heat (raise the temperature of) a quantity

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[8]This approach gets a particularly articulate development in Peacocke [1992].

of water.) This requires that the agent possess in some degree the distinction between *temperature* (a concept picking out an intensive magnitude) and *heat* (a concept picking out an extensive magnitude).[9] To then have beliefs about specific heat, one would need to have beliefs about the relative amounts of heat needed to raise the temperature of like pieces (masses) of differences materials. (One would need to have comparative beliefs about how it took relatively more heat (fuel) to heat (raise the temperature of) water than, say a like weight of wood, metal, and the like.) (Of course, many of the beliefs just mentioned themselves depend on networks of other beliefs.) This suggests that a lot of folk may have lacked the cognitive capacity to possess such a belief about the specific heat of water, that they could not possess this belief because they did not have dispositions qualifying them as possessing the relevant concepts. (It might be worth noting that somewhat more would seem to be required to have the belief *that H_2O has a high specific heat,* as this would require some acquaintance with the beliefs comprising modern chemical theory. The conceptual-cognitive capacity for such a belief would then seem comparatively rarer.) These remarks on what it takes to have a belief with a certain content are intended to be illustrative of the idea that content is to be understood in terms of interrelated dispositions within an agent. This idea will stand for significant qualification in the discussion to follow.

There are several highly general points that one can distill out of the above.

First, just as we can think of the meaning of linguistic items as what is preserved in good translation, so we can think of the content of mental states as what is preserved in good interpretation. This is as it should be — given the interdependencies of translation and interpretation discussed earlier.

Second, just as certain kinds of structure in dispositions are central to translation, it seems that related sorts of structures are central in interpretation. In summarizing certain of Davidson's ideas, I noted that the content of beliefs and desires are understood as a matter of those states being conditioned by each other in ways that roughly mirror what is contentfully appropriate. That certainly is a decent summation of some of Davidson's ideas — for he is a classic proponent of the principle of charity as a constraint on interpretation. However, if one takes to heart my earlier discussion of the principle of explicability as a more fundamental constraint on interpretation than the principle of charity, one would want to refine this characterization of the pivotal pattern. Explicability, rather than the strict contentful appropriateness, of associations would seem to be crucial in the way beliefs and desires, and actions, are interrelated. The dispositions that are to be reflected in good interpretation need not be dispositions to rationality — as humans are both rational and irrational creatures.

Third, in any case, interpretation as well as translation will key on structure within dispositions. Thus, content will be understood as a matter of structure in dispositions. This is a good point with which to start — but it is deceptively

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[9]This should not be taken to require that the agent possess the concepts of intensive and extensive magnitudes, but that the agent the agent be sensitive to the differences in the concepts that function to pick out such magnitudes.

simple.

2.2 Difference in Translation with Sameness of Individual Dispositions.

Putnam [1975a] and Kripke [1972] independently gave us reason to think that
meaning — what is preserved in good translation (and what fixes reference)[10]
— is not an individual matter: it is not simply a matter of the characteristics,
dispositions, or descriptive understandings *possessed by individuals*; the proper
treatment of one's words is not determined by what goes on "skin in". In a famous
thought experiment, one is asked to imagine two individuals who are molecule
for molecule duplicates. One, call him Abe, is a normal speaker of English (for
simplicity in 1750). The other, Twin-Abe, is on a planet very much like Earth, with
speakers of a language, Twin-English, very like English. There is one difference
between Earth and Twin-Earth: the lakes and rivers on Twin-Earth are filled
with a potable liquid that is not H_2O, but rather something else, abbreviated
as XYZ. Commonly, faced with this thought experiment, one judged that the
English term 'water' refers to the stuff with which our earthly English community
(and related communities) have interacted — thus, water is H_2O. One also tends
to judge that the Twin-English term 'water' refers to the stuff with which their
twin-earthly community have interacted — 'water' in Twin-English refers to XYZ.
Thus, 'water' in Twin-English does not refer to H_2O, and cannot have the same
meaning as 'water' in English. This is so even though all the dispositions of
the folk and their "twins", of Abe and Twin-Abe, are parallel. Abe and Twin-
Abe, in particular, are disposed to make the same transitions between claims, the
same judgments in response to presentations of salient things about them (at least
insofar as these transitions and judgments are thought of in terms of the sentences
they would employ), the glasses from which they would drink, their responses to
burning houses, and the like.

   Interestingly, despite the sameness of their processes skin-in, Abe's word 'water'
and Twin-Abe's word 'water' cannot be translated the one into the other. Perhaps
just as interestingly, there would be no ready English translation for the Twin-
English word 'water'. Philosophers sometimes coin a translation — for example,
't-water' (but this seems something of a placeholder for a translation rather than

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[10]The idea that meaning or content needs to be understood in more externalist ways can be
seen as motivated by holding onto some highly general and reasonably solid points. Translation
should reflect the sameness of meaning across two languages (or at least the close similarity of
meaning). Meaning is what fixes the extension of terms or concepts — insofar as they have
an extension. (Extension is thought of as the things referred to, or the things "satisfying" the
concept, where the concept has a referent.) If there could be two people alike in their speech (or
more general) dispositions and yet differing in what they refer to, then meaning cannot depend
solely on such internal matters. If there could be two people alike in their dispositions and yet we
would find it implausible that their terms are readily inter-translatable, then it is implausible that
meaning or content is fully determined by such internal matters. Relying on thought experiments
about such cases, the externalists argued that meanings must be external. Burge [1979; 1992]
has provided particularly important expositions and developments of the externalist position.

a proper translation).

In any case, Abe and Twin-Abe are remarkably similar skin-in — so much so that, were Abe somehow transported to Twin-Earth without his knowledge (say while sleeping), he would (upon waking) continue applying his English word, 'water', to certain liquids about him on Twin-Earth, and his utterances and inferences would mirror those of his parallel Twin-Abe. He would call XYZ 'water', but he would be wrong. For, 'water' in his language, English, does not apply to XYZ, but rather H_2O. Again, this is not determined just by what goes on inside Abe's head, but by something about the community from which he has taken his clues, to which he has deferred, in acquiring his language. Significant on this score is the fact that that community has engaged with a stuff in its environment. These interactions out in the world seem to be partially constitutive of meaning — they are reflected in judgments about the languages and how to translate them. These loom large enough in good translation that folk who are highly similar skin-in, who might even be molecule-for-molecule duplicates could yet differ in the concepts employed as reflected in good translation.

2.3   Sameness of Translation Despite Significant Differences in the Structure of Dispositions.

Not only might two people who are very similar internally still differ with respect to meaning, but two people who are themselves significantly internally different might yet use terms and concepts that are inter-translatable. Here again we can think of the relevant internal differences as having to do with the dispositions to judgments and transitions involving the relevant concept. The common philosophical illustrations are close to home — rooted in the use of scientific concepts and terms. Dalton, Bohr, as well as contemporary investigators seem to have thought and talked about atoms. We (homophonically) "translate" Dalton into our contemporary talk of atoms without embarrassment. We interpret him as holding important beliefs about atoms. We do so despite there having been massive changes in the relevant understandings of atoms. When we read a reproduction of some piece by Dalton in which he says something like, "Atoms combine in regular combinations", we interpret him as saying and believing that atoms combine in regular combinations — despite significant differences in what we would say and infer about atoms. Content, it seems, can be shared across significant differences in some of the relevant dispositions.

If this is correct (and it must be if people are to be able to talk about a subject while disagreeing about it in important respects), then the meaning of the terms in a language, the concepts there employed, must not be merely some composite of the descriptions that a given speaker, or a community of speakers at a time, would associate with the concept. Ramsey once famously suggested that the meaning of theoretical terms might be understood in terms of "an open sentence" constructed out of the various theoretical claims involving that sentence.[11] But this cannot

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[11]Ramsey's suggestion served as an inspiration for some logical empiricist treatments of the

be right, if proponents of different theories are to be able to argue over a shared subject — which they do. Take the theoretical understanding of atoms to which Dalton was committed — and construct a description out of it, " ... the entities such that ...." That descriptive understanding does not pick out atoms (given what we have since discovered). Our understandings could suffer the same fate. But suppose that they do not. Then a description constructed out of our understanding would pick out atoms, while one constructed out of Dalton's would not. Were such descriptions to constitute the meaning of our respective terms, 'atom', then we talk about different things (we about atoms, and Dalton ... about whatever satisfies a description that, it turns out, nothing satisfies). In contrast, one judges that Dalton believed false things about a subject shared with us — atoms.

2.4   The Linguistic Division of Labor

At least part of the story about how there can be a shared referent for terms that are associated with rather different understandings and thus rather different speech and action dispositions can be appreciated by attending to a kind of linguistic division of labor. I believe that my first really nice bicycle was built around an Italian frame crafted out of a steal alloy containing molybdenum — and so I assert here: it contained molybdenum. Now, I know remarkably little about this stuff — molybdenum. (I once looked it up, to find out what I was missing, but have since forgotten most of what I discovered.) I really could not distinguish a bit of molybdenum from a bit of lead oxide, or from many other fairly homogeneous chunks of hard stuff. Yet somehow I manage to talk about this stuff, and have some beliefs about it, when there is a real sense in which I do not know what I am talking about. It seems that I refer to molybdenum by a kind of deferential practice. I intend to refer to that stuff that the relevant savants have come to isolate and call molybdenum — assuming that they have not been too mistaken in their practice. I rely on experts and their usage to ground my lay usage. This linguistic division of labor (described by Putnam [1975a]) is central to how most of us manage to refer to many things about which we know reasonably little. The relevant dispositions here are two-fold: deferential and coordinating dispositions on the part of the relatively uninitiated, and dispositions possessed by the relevant experts. The relevant dispositions — those that determine that I refer to molybdenum, and that it is correct to take me as believing that there was molybdenum in my bike's frame — are not just mine; rather, they are distributed across the community.

Of course, the relevant dispositions are partially mine. I must have dispositions to defer to the relevant authorities, to learn from them, to roll with their refined usage. I might not have such dispositions. I might, for example, just like the way the word 'molybdenum' feels in my mouth or sounds — and I then might name a

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meaning of theoretical terms. Hempel [1965], provides a prominent example of such developments. All such approaches understand the meaning of the concepts or terms as a matter of associated descriptions — in the empiricist case, associated elementary descriptive elements. All such description theories are, in some measure, challenged by the lines of thought discussed in the present sections.

new puppy Molybdenum. When I assert to my friends that Molybdenum is a cute dog — they would be wrong to correct me — and I have a reason to correct their natural misunderstanding. They need a new entry in their translation scheme for my idiolect. However, in most cases, the standing assumption would reasonably be that I am using the common words of my native language in their standard ways — that I either possess a kind of expert familiarity with the relevant concept, or am disposed to defer to those with more elaborated dispositions. As a consequence of this default assumption, in the absence of evidence of complications, one employs a general translation scheme for the agent's natural language that is informed by the practice and dispositions of the relevant savants within the agent's wider community.

2.5   Experts and More Experts

The preceding paragraphs explain how it is that some subjects may differ in their dispositions in extensive ways and yet be rightly interpreted using a shared concept. The explanation turns on the different roles of experts and nonexperts in a linguistic-conceptual division of labor. The individual experts are here thought to possess dispositions that constitute the relevant concept as the concept that it is — fixing its referent as molybdenum, in the example above. Then we novices defer to these experts and manage thereby to hold beliefs about molybdenum. By itself, however, this does not explain how different *experts* (for example, experts at different times) can be understood as using the same concept despite their differences in dispositions. It does not explain how it can be correct to interpret Dalton, Bohr, and contemporary physicists as all believing "some of the same things" (for example, that atoms combine in regular proportions to form familiar chemicals) or how these thinkers can be rightly understood as thinking and talking about a shared subject (in the example, about atoms) while differing in what they would say and think about their shared subject. There are significant differences in the dispositions of these thinkers — Dalton's utterances and beliefs are related to each other in ways that are rather different from the ways in which Bohr's are interrelated. We thus still need an explanation as to why, taking our cues from experts within our community, we can yet correctly understand (translate and interpret) Dalton and Bohr as believing that atoms combine in regular proportions to form familiar chemicals. They are each experts within their respective communities, and they differ.

Before pursuing this matter of experts across different times, reflect on the differences that can sometimes be found among experts in a community at a given time. The dispositions to utterance and inference had by experts at a time can diverge significantly without undermining the translation and interpretation of those experts as disagreeing about a shared subject. Perhaps some top-of-the-line experts in the community would insist on, and rely on, some claim that they would treat as something central to what it is to be an instance of their subject. (They may, for example, be investigating species and speciation.) There may be other

experts who are not of their own accord inclined to treat that claim as central to being an instance of their subject (species and speciation). In fact, these other experts might not even be convinced of the claims that the first experts take to be so central. These other experts might insist on, and rely on, somewhat different claims that they would treat as something central to what it is to be an instance of their subject (what it is to be a species, for example). Suppose also that these two sets of experts are themselves engaged in respectful, somewhat cooperative and somewhat competitive exchange — one in which respectful argument serves as mutually constraining. I am here reminded of aspects of Laudan's [1977] model for scientific communities organized around problem solving, pursuing multiple research strategies that may nevertheless be mutually constraining. Also suggestive is Kitcher's discussion in *The Advancement of Science* [1993] of the "referential potentials" of terms and of the ways in which these and scientific traditions can develop. Such respectful mutual constraint might provide a relationship between experts who have come to understand different sets of claims about a shared subject. What each understands may be only roughly correct. The truth about the shared subject may lie, in a non-simple fashion, "somewhere in between" their understandings — the truth about species might be some understanding that represents a selective composite of their differing positions, but one which no one of the engaged experts are themselves then quite up to distilling or producing. In the earlier picture of a linguistic division of labor, the individual experts are thought to each possess dispositions that constitute the relevant concept as the concept that it is — fixing its referent, while the rest of us refer by deferentially using the relevant terms. But, in the present picture, no individual expert at the given time needs to possess dispositions that "hold in place" the concept and determine its reference. Instead, it is the dispositions of the joint set of engaged experts that does this. One might say that this would make for a distributive sharing of a single concept within the engaged community.

The relationship just sketched may be thought of as analog at the level of experts to the division-of-labor that obtains between experts and deferential users. The relationship between these experts is not, however, one of deference (as commonly understood) but of respectful engagement and mutual constraint. As things are eventually sorted out, some components of their various constraints may jointly turn out to be significant, adding up to a composite understanding and a composite set of dispositions constituting the concept as the concept that it is. If something like this is possible, then while no one of the top-of-the-line savants at a given time would need to possess a full sensitivity to what fixes the referent of the concept, the community of such inquirers could yet be jointly so sensitive. Indeed, I think that this scenario represents more than a possibility. I find it plausible that this is sometimes just how concepts are possessed.

In cases like those just considered, the top-of-the-line savants would possess the relevant concepts *only jointly*, and any one of them would only possess the concept partially. That is, no one of them individually would have dispositions that constitute the relevant concept as the concept that it is. The mutually engaged

understandings and practices within an engaged community may jointly "hold in place the concept", enabling the deferential use of relative novices. To write of "a concept being possessed only distributively" is perhaps a somewhat jarring way of putting the matter — but perhaps it is only so because there has been an individualistic presumption to talk of concept possession.

Once one allows for the possibility that the dispositions that constitute a concept as the concept that it is are distributed within a community at a time, not possessed by any one individual, I think that we can recognize another, yet more radical case: one in which the state-of-the-art experts in a community at a time fail to have joint dispositions that fully or accurately fix the referent of the concept they employ. Just as the dispositions of any individual expert at a time may fail to "hold in place a concept", so the dispositions that are jointly possessed at a time may not, of themselves, "hold in place a concept" or "determine its referent." The idea is this: suppose that the set of relative conceptual competents within the community at a time do not even jointly or distributively realize a full sensitivity to what qualifies a certain part of the world as satisfying the concept that they employ — but that they stand open and engaged with ongoing inquiry in a way that has them respectfully, if argumentatively, correctable by descendent communities and the considerations that will there be turned up. Here the engagement spoken of above, between experts within a community of inquirers at a time, is to be thought of as extended to an historically ongoing engagement with earlier and later stages of that community. Here, perhaps the later community may be said to possess a sensitivity to the what ultimately constitutes the relevant concepts, and one might say that the *evolving community across time distributively possesses* such a sensitivity or understanding. (Again, Kitcher's discussion of concepts and scientific development is suggestive.) For example, it is plausible that there are considerations relevant to fixing the extension of the concept *cause* that neither Hume nor any of his contemporaries individually or jointly appreciated — considerations that arose only in the twentieth century with quantum mechanics and the idea of probabilistic causation. Yet Hume offered his arguments to the generations, and initiated an argument in which we honor him by correcting him. To correct him, we must treat him as using the concept of *cause.*

These last few paragraphs have been somewhat exploratory. Their object was to take certain ideas in wide philosophical currency — regarding a linguistic division of labor, for example, and to suggest ways of extending these ideas to accommodate cases in which the "experts" at a time may differ among themselves, or differ with later experts. In all this there are certain central ideas. First, while dispositions to utterance and inference seems important in making it correct to translate or interpret some agents as believing or desiring, or entertaining, or doubting, some content — there is room for significant difference in the dispositions that an agent must have in order to possess the belief, desire, doubt, or thought in question. Second, in part, this is because there is a kind of deferential relationship that allows relative novices to defer to relative experts — and manage to think and talk about a subject thereby. In such cases, the proper translation or interpretation

of the novice cannot be settled without attention to these social relationships and to the dispositions of the relative experts. But, experts at a time may differ, and experts may differ across time. My suggestion has been that we think of a kind of engagement in an ongoing community of inquiry, and of an openness to correction, as providing a kind of social relationship at the level of experts that, like the linguistic division of labor, allows us to understand how differences in dispositions may not necessitate diverging interpretations or translations. Here again, the translation or interpretation would need to attend to dispositions within the ongoing community, rather than merely to the dispositions of an individual (when interpreting that individual). These points reflect ideas also found in Brandom [1994].

2.6 Lessons

Much of the philosophical thought discussed in section 2 has taken as its illustration and inspiration the working of concepts in our contemporary western community. Many of the illustrations discussed here have dealt with concepts with some currency within scientific usage. This reflects the focus of much philosophical writing about concepts, meaning, and reference. Of course, the communities in which anthropologists apply their craft may be different in important and relevant respects from the communities that are the common philosophical focus. Further, the anthropologist may be concerned with a context of thinking, conceptualization, and action within the relevant community that is importantly different from those predominantly scientific contexts of thinking and conceptualization on which philosophers have so often focused. So, in drawing lessons from the foregoing, we must take care to notice differences and consider what points made above are likely to generalize, and what are not.

We can start with the linguistic division of labor. In societies with a significant division of cognitive labor one may presumably anticipate a corresponding linguistic division of labor. Where some are charged with developing special expertise in some subject, and others are thereby freed from such chores, we will not be surprised to find a relatively high incidence of deferential usage on the part of the less expert. (Of course, even in societies with such differentiation in some contexts — those with scientific and technological experts, for example — one may find comparatively little differentiation in other contexts — that of the common cuisine, for example.) Where there is a division of labor, one's translation should be keyed to the dispositions of the relevant experts. One who wanted to translate or interpret contemporary talk of "molybdenum" or "cyclic AMP" would need to identify who counted as the experts to which most defer in using such terms, and one would need to base one's translation on their usage or dispositions. Similar selectivity in informants typically will only be necessary in the face of parallel divisions of cognitive and linguistic labor and associated patterns of cognitive deference. For example, should there be a priestly or shamanistic cadre to whom others generally defer, interpretation should take its cues from their dispositions.

I noted that our own experts at a given time may not be settled in their take on what is fundamental to a subject or concept. Experts may disagree in what they take to be crucial to satisfying a concept, and yet be disagreeing about a shared subject. Consider the nineteenth century disagreements between evolutionists and non-evolutionists over what constituted a species, for example.[12] It becomes plausible that there have been times at which experts did not individually fully possess the concepts of *species,* did not individually possess a sensitivity to what made for the reference of *species* in the actual world. But, so long as the relevant experts remain engaged in a debate in which their counterparts are understood as disagreeing in fundamental ways about a shared subject, these experts could distributively possess the relevant concept. *We can then consider the possibility that something similar may occur with non-scientific concepts.* One might imagine social groups (or their representative "experts") engaged over the subject of human well-being, for example. They disagree, and their differences may extend to fundamentals — to what would be constitutive of human well-being — and yet they could still take themselves to be disagreeing about a shared subject. They then could be taken as disagreeing about the shared subject, human well-being, and deploying a shared concept — *human well-being.* In such a case, no one "expert", no one set of "experts", no one individual who is party to the debate need be wholly correct regarding the fundamentals about what makes for human well-being. This certainly suggests that the idea of concepts being possessed or held in place only in a socially distributed manner has reasonable application to more than just scientific concepts.

Let me develop further the imagined illustration of engaged debates centering on a concept of *human well-being.* Let me stipulate several points about the deep disagreement involving a shared concept of *human well-being* here envisioned.To give some sense of concreteness to this illustration, suppose that one group might be called "secular materialists" — they insist that human well-being can be measured by one's preference-driven consumption of a healthy slice of the world's gross economic product (perhaps weighted somewhat by the inverse of the relative size of one's contemporary global product).[13] Suppose the contending group might be

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[12]Or consider the way in which quantum mechanics occasioned a reconsideration of what was commonly taken to make for a cause-effect relationship. Those who came to think that there could be irreducibly statistical or probabilistic causal relationships certainly were in disagreement with earlier experts — who typically would have recognized no such possibility. If the more contemporary view is correct, earlier experts must have been uniformly mistaken in some fashion. It is plausible that their mistake is best viewed as a matter of being wrong in fundamental ways about what made for a cause and effect relationship. To say that they were wrong about causation in this way, one must take them as using the concept of *a cause* — the concept we use, albeit with certain misunderstandings. It is plausible that eighteenth century experts neither individually nor (even) jointly fully possessed the concept of *a cause.* To emphasize: to say that they were wrong about causation, one must take them as using the concept. As explained earlier, this seems to depend on their open engagement with an ongoing community inquiry employing a concept, whereof our own community is a later stage, one with an improved grasp of the concept of *a cause.*

[13]The secular materialist is thinking that human well-being is a matter of having at one's disposal a healthy slice of then available commodities or resources — while also allowing that,

termed "stoic ascetics" — they insist that human well-being is a function of the undisturbed state of one's soul, attained by an enlightened lack of attachment to all that is not in one's power (where what is in one's power are understood to be largely internal matters, one's own cognitive life). Now, suppose that when these groups encounter each other, and come to explore their differences, *they would each suppose that the other is fundamentally mistaken about a shared subject.* Thus, the parties to the debate presuppose that the distinct conceptions of the various parties do not constitute different concepts and thus different subjects. This requires that, in actual fact and disposition, the parties to the disagreement do *not* say things like:

"Well, it is true that *ascetic-human-well-being* is compatible with minimal possessions, while *materialist-human-well-being* is not, and this is just a conceptual difference. Those guys aren't talking about what we are."

Instead, recognizing the difference in conception, the parties to the debate typically insist that the one conception and not the other tracks what really makes for human well-being. Thus, whether or not there is indeed a fact of the matter to what makes for human well-being (in the metaphysical sense that realist and anti-realist philosophers would dispute), *the concept* in play, the concept of *human well-being* involves *a presupposition that there is such a fact of the matter.* To the extent that this is so, the concept would seem to function in ways that are analogous to the concepts in certain sciences in one important respect: just as it is a presupposition of the concept of *species* that there is putatively some kinds, species, about which engaged inquirers can fundamentally differ, and be right or wrong, so it is a presupposition of the concept of *human well-being* that there is a important moral feature, human well-being, about which engaged agents can fundamentally differ and be right or wrong. Such presuppositions seem to function as integral to the concept itself (its semantics). Notice that the character of the engaged disagreement itself indicates that *their differences are not to be understood as merely conceptual — it is a part of how the relevant concept works that both parties cannot be right by virtue using different concepts.* Here, differences in conception regarding human well-being do not make for there being different concepts in play. Differences in dispositions (of the sorts just imagined to distinguish the secular materialist and the stoic ascetic) do not make for differences in concept.

It is highly plausible that some ethical disagreements are structured in the ways just suggested. Some ethical concepts work in the way just suggested — and are thus shared despite fundamental disagreements (even at the level of experts, if some are socially treated as experts). Thus, while largely scientific concepts have commonly served as the focus of the philosophical thought discussed in the last few sections, and while many concepts that will interest social scientists may work somewhat differently, we are finding reason to cautiously take some inspiration from certain ideas popular in externalist semantics.

Let me develop further a suggestion regarding the proper translation of the

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as the size of the economic pie grows, one may need a little less (proportionally) of a slice of the whole to have well-being.

concept of *human well-being,* as featured in the illustration above. It is certainly plausible that most people employ a concept of *human well-being* which includes the realist presupposition reflected above — the presupposition that there is some fact of the matter to what makes for human well-being, and that people can differ fundamentally in their understanding of these matters while yet sharing the concept and thus disagreeing about a shared subject. Whether this realist presumption is a part of the semantics of some folk's concept associated with their phrase 'human well-being' is a contingent matter about which one can gather further empirical evidence — it turns on whether, when they encounter apparent disagreements in connection with the application of their phrase, they then are inclined to engage in debates of the sort suggested above. When they are so inclined, both groups associate with their talk of "human well-being" a concept that involves the realist presumption. To make sense of their disagreement, one must translate or interpret them as employing the same concept, while holding markedly different conceptions. The ascetic's talk of "human well-being" translates into the materialist's, and vis-a-versa. When the secular materialist asserts that having a luxurious mode of transportation (or luxurious for one's time) is part of what makes for human well-being, the stoic ascetic would be right in representing this claim, in her terms, as the claim that having a luxurious mode of transportation is part of what makes for human well-being — which the stoic would think to be fundamentally wrong. Here, then, with respect to the working of this moral-evaluative notion, one finds a phenomena that was remarked on in connection with scientific concepts: the sharing of a concept despite deep differences in associated dispositions — and one translates and interprets accordingly. Of course, to understand what the materialist or the ascetic does, one will commonly need to understand their differing conceptions (rather than just their shared concept). To understand their engaged disagreement, however, one must recognize that they share a concept.

Now, suppose that one denies the realist presupposition attributed to the parties to the above disagreement — one denies that there is some one important feature, or one constellation of features, making for human well-being, about which parties to such a debate can be right or wrong. If one then talks of 'human well-being,' one's concept must work differently from that shared by the divergent parties above. One must be using a different concept in talking of "human well-being" than that used by the parties to the debate.

To make matters interesting, suppose that you as an investigator are from a community that has come to use the phrase 'human well-being' *without* a realist presumption. Crudely, when encountering any but the most superficial of differences in evaluation employing the phrase 'human happiness', one does not suppose that differences are differences of mere conception — rather, the differences are taken to constitute differences of concept. It seems that one's community uses the phrase, 'human well-being' to express a concept that is *different from* the concept employed by the parties to the disagreement discussed above. We can draw on the earlier discussions to shed some light on the character of the difference in concepts

that then seem in play.

One who uses a natural kind substance concept such as *water* uses a concept that refers to some stuff about which one and one's community could be deeply mistaken. Central to such a concept is a kind of realist presumption — water is a homogeneous class of stuff with the same composition as the stuff that has served as prominent samples with which one's community has interacted. Should there be no such homogeneous stuff — which of course there is — then *water* would lack a reference. But, two groups of investigators could hold very different understandings of that stuff, very different conceptions, and yet refer to the same stuff and use the same concept. One (perhaps the ancients) might conceive of water as a simple and fundamental substance, the other (being informed about rudimentary contemporary chemistry) might conceive of it as H_2O, and yet share the concept of *water.* This said, the concept of *water* must be a very different concept from a concept that happened to be defined in terms of one such conception — say that of H_2O — and this is so, even for a conception — such as H_2O — that happened to pick out exactly the same stuff that the concept of *water* picks out. For reasons mentioned earlier, *water* and H_2O are different concepts, having the same reference.

Now, suppose that one's evaluation of states of human affairs uses an evaluative concept expressed with the phase 'human well-being', but that one's concept is, in effect, *defined* in terms of something like the materialist conception. As *water* and H_2O are different concepts, so the concept of *human well-being* (shared by the materialist and ascetic above) must be a different concept from the concept that can be *defined* in secular materialist terms. What should one do when confronted by either the above secular materialist or stoic ascetic — given that their shared concept includes (internal to the semantics of that concept) a realist presumption that is (by hypothesis) no part of one's own concept (the concept associated with one's own phrase, 'human well-being')? It would then be misleading to translate their evaluative phrase 'human well-being' (for simplicity I am suppose that they speak a dialect of English) simply using the phrase 'human well-being' in one's own dialect of English — for the concept associated with the phrase in one's dialect is different from the concept associated with the phrase in their dialect. Here, I think, one should engage in a limited bit of reconstructive translation. If one used one's phrase, 'human well-being' as a translational stand-in for their phrase, one would do well to mark the stand-in as deceptive in an important respect. The semantic workings of their concept (which by your lights may have no determinate reference) mandates that they stand ready to get involved in certain engaged disagreements, while your concept (by hypothesis) repudiates such disagreements. The point must surely be registered in a reconstructive moment that notes that your phrase (associated as it is with your concept) is really an abbreviation for a concept for which you may have no ready conceptual parallel — at least not one associated with a short and common phrase within one's community. 'Mangu' apparently cannot be translated as 'witchcraft' — at least not without monograph length qualifications — and it seems that, given the stipulations of my example,

neither the (realist) materialist nor (realist) ascetic talk of "human well-being" can be translated into one's (antirealist) talk of "human well-being" — without significant qualifications. In contrast, their respective talk of "human well-being" may be inter-translatable in a more ready way.

The point of my discussion in these last pages is to illustrate that, although much recent discussion of concepts has focused rather strongly on scientific contexts and scientific communities, some of the ideas encountered there do find application in other contexts. Without the presumption that all language, and all concepts, works just like scientific language and concepts, there is reason to believe that some nonscientific language and concepts work in analogous ways. Thus, there is reason to take some inspiration from the ideas advanced by proponents of externalist semantics.

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PRACTICE THEORY

Joseph Rouse

Anthropology, sociology, and related subfields of history have increasingly taken "practices" as their primary object of study in the last several decades of the twentieth century. Applications of the practice idiom extend from the most mundane aspects of everyday life to highly structured activities in institutional settings. Some of the patterns of performances identified as "practices" are quite localized geographically or historically, while others are of much more general extent. Practices range from ephemeral doings to stable long-term patterns of activity. Attention to practices often requires extensive examination of relevant equipment and material culture, but can also assign constitutive roles to vocabulary and other linguistic forms or performances. The range and scope of activities taken by various theorists to constitute "practices" can be made evident by a few characteristic examples from the practice theory literature. They include spatially dispersed but relatively short-lived activities such as Nasdaq stock market Internet "day trading" [Schatzki, 2002] or academic presentations on the international conference circuit [Rabinow, 1996], but also relatively stable and widespread patterns of social relations such as willfully self-interested bargaining [Taylor, 1985]. Many practices are culturally specific, such as the Kabyle gift-exchanges discussed by Bourdieu [1977] or the secret baptism of money by Colombian peasants described by Taussig [1980]. Yet some practice theorists also refer to activities which take various culturally specific forms, such as eating with specific utensils and preparing food accordingly [Dreyfus, 1991], while others identify long-standing institutionalized activities such as chess ([Haugeland, 1998]; [MacIntyre, 1981]), medicine (MacIntyre), or science. In the latter case, the practice idiom has ranged in scope from references to science generally as a practice [Pickering, 1992] to examining historically specific experimental systems and instruments ([Kohler, 1994]; [Schaffer, 1992]) experiments [Pickering, 1995], disciplinary cultures [Knorr-Cetina, 1999], pedagogical regimes [Warwick, 2003], ways of organizing experimental venues and work groups [Galison, 1996], and styles of theoretical work [Galison, 1998].

The theoretical uses of the concept of practice within social theory and philosophy of the social sciences have been as diverse as the kinds of examples employed. Heidegger's and Wittgenstein's work on understanding and rule-following have been prominent influences upon practice theories, but so has Foucault in each major stage of his work. Prominent sociologists such as Pierre Bourdieu or Anthony Giddens are often cited as practice theorists, while Sherry Ortner's [1984] review article on "Theory in Anthropology Since the Sixties" proposed "practice" as the central theme of anthropological theory in the 1980's, a trend that continues today.

Ortner argued that "the newer practice orientation" in anthropology incorporated a "palpable Marxist influence" which led "the shaping power of culture/structure" to be "viewed rather darkly, as a matter of 'constraint', 'hegemony', and 'symbolic domination'" [1994, 390–91]. Yet conservative theorists such as Michael Oakeshott, Michael Polanyi, or Alasdair MacIntyre have also made central use of the practice idiom, or been retroactively cited as practice theorists. Reference to "scientific practices" has been a central theme of much of the recent literature in science studies as well, not only as a descriptive category, but as a theoretical articulation of a move beyond its earlier characterization as the Sociology of Scientific Knowledge (e.g., [Pickering, 1992]). Ethnomethodological work in sociology, too, is now often presented as attending to everyday practices and agents' understanding of the practices they engage in (see Lynch, this volume). Although Judith Butler [1989; 1991] does not emphasize the term 'practices' in her widely influential work on the performativity of gender, her analysis also has considerable resonance with practice theories. Indeed, in an influential critical study of practice theories, Turner draws their boundaries even more widely, claiming that "a large family of terms [are] used interchangeably with 'practices', among them. . . some of the most widely used terms in philosophy and the humanities such as tradition, tacit knowledge, *Weltanschauung*, paradigm, ideology, framework, and presupposition" [1994, 2].

An especially contentious issue in practice theories has been the place of language within social or cultural practices. Some theorists [Dreyfus, 1979; 1991; Bourdieu, 1977; 1990; Polanyi, 1958] are prominent examples) make central to their discussion of practices those aspects of human activity which they regard as tacit and perhaps even inexpressible in language. Their accounts suggest that the practice idiom is important because it calls attention to important aspects of human life that will likely remain hidden to those social scientists and theorists who give pride of place to language and linguistically articulable thoughts. Yet many people employing the practice idiom go in the opposite direction, identifying "practices" primarily by the vocabulary, linguistically articulable presuppositions, or conceptual relations that participants in the practice share. Still others treat language itself (or "discursive practices") as a paradigmatic application of practice talk. Robert Brandom [1976] and Richard Rorty [1991], for example, claim that the differences between representationalist and social practice approaches mark the most fundamental issue in contemporary philosophy of language.

The diversity of work in social science, social theory, and philosophy that employs the practice idiom (either as a developed theory of social practices, or as an empirical correlate to such a theory) might thus suggest that the term 'practice' has no theoretical coherence. Perhaps the ubiquity of practice talk merely reflects current intellectual fashion with no substantial conceptual significance, or worse, an underlying theoretical confusion assimilating incompatible conceptions of social life under a superficially common term. A different challenge to the felicity of understanding social life in practice-theoretical terms has been proposed by Turner [1994]. He suggested that the broad attractiveness of the practice idiom arises

from the deceptive appearance that it has resolved some fundamental recurrent problems in social theory, in ways that turn out to be superficial or empty:

> The idea of 'practice' and its cognates has this odd kind of promissory utility. They promise that they can be turned into something more precise. But the value of the concepts is destroyed when they are pushed in the direction of meeting their promise. [Turner, 1994, 116]

Assessing these worries about the coherence or substance of the practice concept and its applications within the social sciences and social theory will therefore be a central concern of this essay.

The diversity and the extent of theoretical invocations of practices militates against any attempt to provide a comprehensive catalog of the major contributors to practice theory. The criteria for inclusion would themselves be centrally at issue in any such exposition. Moreover, such an enterprise would be misguided unless it can be shown that practice theory has sufficient conceptual integrity and theoretical coherence to merit consideration as a distinct genre of social theory. I shall therefore address the topic of practice theory in two parts. The first part of the essay will articulate the thematic rationale for practice theoretical approaches. Instead of an exposition of competing theories or theorists, I will address the principal concerns that have motivated theoretical attention to "practices" in philosophy, social theory, and social science. While I shall try to situate the more prominent practice theorists within this thematic survey, the themes themselves and the principal ways they have been taken up will be my primary focus. In the second part of the essay, I turn to some prominent theoretical challenges confronting practice theories, and assess their significance. Contra Turner, I shall argue that the practice idiom remains an important conceptual resource for social theory and philosophy. Turner's and other criticisms nevertheless reveal important inadequacies in many current conceptions of practice theory. Adequately addressing these theoretical challenges will therefore require some significant revisions in many extant conceptions of social practices and their theoretical articulation.

1   WHAT IS "PRACTICE THEORY"?

I highlight six principal considerations that make "practices" a central theme in social theory, social science, or philosophy. These considerations have different importance for various practice theorists, and in some cases, theorists differ substantially in their treatments of the theme. Collectively, however, they express clearly the rationales for theoretical attention to practices.

1.1   *Practices, Rules and Norms*

Perhaps the single most important philosophical background to practice theory is provided jointly by Wittgenstein's work on rule-following, and Heidegger's account of understanding and interpretation. They pose fundamental concerns for

any conception of social life and understanding that emphasizes rules, norms, conventions, or meanings. Such conceptions of the domain of sociology, anthropology, and other human sciences are widespread within the philosophy of the social sciences. The notions that society or culture is the realm of activities and institutions governed or constituted by rules, of meaningful performances rather than merely physical or biological processes, or of actions according to norms rather than (or as well as) causally determined events are ubiquitous. Such conceptions of the social domain trace back to Kant's contrast between behavior according to natural law, and action governed by a *conception* of law, i.e. by a *norm*. Actions governed by norms also involve understanding and responding to the meaning of one's action, and of the situation in which one acts. Indeed, grasping and responding appropriately to meaning is perhaps the exemplary case of normative governance.

For Kant, of course, a norm was simply a rule (or law) one imposes upon oneself. Wittgenstein's and Heidegger's contributions to practice theory stem primarily from their parallel criticisms of this conception of the normativity of human thought and action. Wittgenstein's treatment of this issue stems from his discussion of rule-following in the first part of *Philosophical Investigations*. Wittgenstein's central point is that rules are not self-interpreting. Given only a rule, the possibility always remains open to follow the rule in deviant ways. One might then try to specify how the rule is to be interpreted, but any such interpretation would itself be another rule open to deviant application. Wittgenstein drew a complex conclusion from this concern,

> This was our paradox: no course of action could be determined by a rule, because every course of action can be made out to accord with the rule. The answer was: if everything can be made out to accord with the rule, then it can also be made out to conflict with it. And so there would be neither accord nor conflict here.

> It can be seen that there is a misunderstanding here from the mere fact that in the course of our argument we give one interpretation after another...What this shows is that there is a way of grasping a rule which is *not* an *interpretation*, but which is exhibited in what we call "obeying the rule" and "going against it" in actual cases. [1953, I par. 201]

The challenge, then, is to characterize this way of grasping rules without interpreting them, which is "exhibited in actual cases."

Heidegger makes a closely parallel point that has been comparably influential upon practice theory, in his discussion of understanding and interpretation (*Auslegung*) in *Being and Time*. Heidegger claims that all interpretation (including linguistic assertion) draws upon a more basic understanding or competence that is not explicitly articulated. Indeed, for Heidegger, understanding (as a form of competence) is the more basic notion, and "interpretation" is simply understanding's "own possibility of developing itself...[through] the working-out of possibilities

projected in understanding" [1962, 188-89, H148]. For Heidegger, interpretation is involved whenever one interprets something "as" something, whether one interprets something as a hammer by using it to hammer a nail, or by making explicit assertions about it. In either case, the interpretation is only possible against the background of a prior understanding of the situation. This prior understanding makes three crucial contributions to the intelligibility of the interpretation.[1] In Heidegger's example of hammering, one must already understand the general context of carpentry (the relation between hammers, boards, nails, buildings or furniture, and the various purposes they serve), one must have a sense of how to proceed (hammers must be picked up to be used, held by the handle, swung rather than thrown, hit the nail on the head rather than the shaft and so forth), and one's interpretation is governed by a general sense of what would bring it to fulfillment or completion. Without some prior practical grasp of these considerations, nothing one does with a hammer could amount to hammering with it (indeed, there could *be* no hammers without such understanding of hammering). The outcome of an interpretation, however, then recedes into the understanding which projects possibilities for further interpretation.

Why have these aspects of Wittgenstein's and Heidegger's work been important for practice theory in the philosophy of the social sciences? Wittgenstein's and Heidegger's criticisms can be construed as a regress argument against any regulist conception of social life or normativity. If to act according to norms is to follow a rule, and rule-following can be done correctly or incorrectly, then a vicious regress of rules would render action according to norms impossible. Kripke [1982] notoriously places this skeptical issue front and center in his widely discussed interpretation of Wittgenstein. So construed, Wittgenstein's and Heidegger's challenges to the autonomy of rules or explicitly articulated meanings or norms pose a central concern for the philosophy of the social sciences. The upshot of both criticisms is that there must be a level or dimension of human understanding expressed in what we do that is more fundamental than any explicit interpretation of that understanding. The concept of a "practice" is then widely invoked in social theory to identify the locus of this background understanding or competence that makes it possible to follow rules, obey norms, and articulate and grasp meanings. Practice theorists thereby hope to develop Wittgenstein's enigmatic claim that rules and rule-following draw upon "agreement in forms of life" [PI 241], and Heidegger's more elaborated claim that the most basic articulation of everyday human being comes not from individual self-determining action, but from "what one does" (*das Man*, the "anyone").

The point of introducing "practice" talk here is highlighted by contrast to behaviorist approaches to the human sciences. Behaviorists (psychological behaviorism

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[1]Heidegger has technical terms for these three aspects of the understanding presupposed by any interpretation: *Vorhabe*, *Vorsicht*, *Vorgriff*, collectively referred to as the *Vor-struktur* of interpretation. The standard [1962] translation renders these terms as "fore-having", "foresight", "fore-conception", and "fore-structure", although I think they might be more felicitously rendered in English as pre-possession, preview, preconception, and pre-structuring.

was perhaps exemplary of this theoretical and methodological genre, but behaviorist approaches were also influential throughout the social sciences at mid-century) were suspicious of mental or intentional concepts. They hoped to redirect the human sciences toward the study of human behavior, conceived as publicly observable movements in contrast to internal mental representations or interpretations. The orientation of behaviorism was reductive or eliminativist: behavior was to be described in non-intentional, non-normative terms, such that human social life could be described and explained in terms congenial to a strict empiricist. Charles Taylor characterizes this empiricist/behaviorist orientation as the aspiration to describe human life in terms of "features which can supposedly be identified in abstraction from our understanding or not understanding experiential meaning, [in] brute data identifications" [1985, 28].

Practice theories also encourage attention to publicly accessible performances rather than private mental events or states. Their aim is typically not to avoid intentional or normative locutions, however, but to make them accessible and comprehensible. While attending primarily to "outward" performance rather than "inner" belief or desire, such performances are usually described in what Geertz [1973] characterized as "thick" descriptive terms rather than the extremely thin language demanded by behaviorists. The claim is that human performances and activities are themselves meaningful, rather than having meaning imposed upon or infused within them by animating beliefs, desires, and intentions. Indeed, the stronger suggestion is that rules, norms and concepts get their meaning, and their normative authority and force, from their embodiment in publicly accessible activity. Taylor's account is characteristic of this move:

> The situation we have here is one in which the vocabulary of a given social dimension is grounded in the shape of social practice in this dimension; that is, the vocabulary would not make sense, could not be applied sensibly, where this range of practices did not prevail. And yet this range of practices could not exist without the prevalence of this or some related vocabulary. [1985, 33–34]

I will return below to the question of just *how* rules, norms, meanings, conventions or vocabularies are supposed to be grounded in practices, and how that grounding might make possible the intelligibility and continuity of society or culture.

1.2   Reconciling Social Structure or Culture with Individual Agency

A second theme in practice theories has been to mediate, or perhaps by-pass, perennial discussions of the relative priority of individual agency and social or cultural structures.[2] The issue in these debates has typically been whether the

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[2]For more extensive discussion of these debates, see Zahle, this volume; for elaboration of parallel discussions in anthropology concerning the concept of culture, see Risjord, this volume.

social sciences can and should refer to and achieve knowledge of social wholes (institutions, cultures, social structure, traditions, etc.) that cannot be decomposed into actions by or states of individual agents.[3] The autonomy of anthropology or sociology as distinctively *social* sciences would obviously seem to be enhanced if there are irreducible social or cultural structures that are the proper object of these sciences. Critics of social or cultural wholism have nevertheless raised ontological questions about the existence of social or cultural wholes except as composites of individuals and their actions, and methodological and epistemological questions about how knowledge of such wholes could be grounded in evidence. Wholists have responded in turn that the intelligibility of individual actions often depends upon their social or cultural context. If one simply examined the actions of individuals without reference to supra-individual settings, such familiar activities as voting, exchanging money, performing a ritual, or even speaking a language might not make sense. Individual actions and agents may thus only be identifiable and understandable as components of a larger culture or society.

Practice theories typically resolve these disputes by acknowledging that both sides grasp something important. At one level, practices are composed of individual performances.[4] These performances nevertheless take place, and are only intelligible, against the more or less stable background of other performances. "Practices" thus constitute the background that replaces what earlier wholist theorists would have described as "culture" or "social structure." The relevant social structures and cultural backgrounds are understood dynamically, however, through their continuing reproduction in practice and their transmission to and uptake by new practitioners. While there is nothing more to the practice than its ongoing performative reproduction, these performances cannot be properly characterized or understood apart from their belonging to or participation within a practice sustained over time by the interaction of multiple practitioners and/or performances. Ortner concludes that,

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[3]There have been two very different uses of the term 'holism' in the philosophy of the social sciences. In one sense (discussed by Zahle, this volume), holism is the view that there exist social or culture entities ("wholes") that cannot be fully understood in terms of the actions or states of individual human agents. In another sense of the term, which has been especially prominent in philosophical reflection upon psychological states and linguistic meanings, a property is holistic if one thing cannot have the property unless many other things also have this property. Since there are no useful alternative terms for these two very important concepts, in the remainder of the article I will take advantage of orthographic ambiguities, and refer to the existence of supra-individual entities as "wholism", and to the interdependence of property ascriptions as "holism".

[4]Most practice theorists would identify these performances as the actions of individual agents. Some theorists influenced by Heidegger, however, would emphasize that the "who" performing most basic, everyday human activities is anonymous and undifferentiated, rather than being an already individuated subject or self. Individuation and responsibility only takes place against the background of these anonymous performances. Foucault and many of those he influenced go further in identifying the individual subject as something constituted by rather than underlying and presupposed by actions or performances. Butler [1989] succinctly exemplifies such a theoretical approach: "gender is always a doing, though not a doing by a subject who might be said to preexist the deed. ... There is no gender identity behind the expressions of gender; that identity is performatively constituted by the very "expressions" that are said to be its results". [25]

> The modern versions of practice theory appear unique in accepting all
> three sides of the...triangle: that society is a system, that the system
> is powerfully constraining, and yet that the system can be made and
> unmade through human action and interaction. [1984, 159]

This emphasis upon the dynamics of social structures and their governance
or constraint of individual actions gives a strongly historical dimension to any
practice-theoretical approach to sociology or anthropology. Such dynamics also
allow for conceiving a "cultural" background that is not monolithic or uncontested,
which has been a very important consideration in recent anthropological work.
Anthropologists had long worked with a conception of culture that treated cultures
as unified and systematic. Kluckhohn and Kroeber's formulation typifies such a
conception:

> Culture consists of patterns, explicit and implicit, of and for behav-
> ior acquired and transmitted by symbols, constituting the distinctive
> achievement of human groups, including their embodiments in arti-
> facts; the essential core of culture consists of traditional (i.e., histori-
> cally derived and selected) ideas and especially their attached values;
> culture systems may, on the one hand, be considered as products of ac-
> tion, on the other as conditioning elements of further action. [Kroeber
> and Kluckhohn, 1963, 181]

Instead of positing such a unified conception of culture, practice theories rec-
ognize the co-existence of alternative practices within the same cultural milieu,
differing conceptions of or perspectives on the same practices, and ongoing con-
testation and struggle over the maintenance and reproduction of cultural norms.
Moreover, practice theories provide additional resources for understanding cross-
cultural interaction brought about through migration, political domination, or
trade relations. Instead of treating cultural interaction as a matter of translation
between whole cultural systems, practice theorists can recognize more localized
practices of partial interpretation and exchange that can be somewhat isolated
from other practices and meanings that function within each of the interacting
fields of cultural practice.[5] The acknowledgement of cultural dissonance within
practice theory also allows practice theorists to recognize the differential uses and
meanings of cross-cultural interaction within intracultural politics [Traweek, 1996].

While practice theorists generally share a conception of social or cultural struc-
tures as existing only through their continuing reproduction in practices, they
differ extensively over the degree of stability that practices can sustain. Bourdieu,
for example, claims that,

> The conditionings associated with a particular class of conditions of ex-
> istence produce *habitus*, systems of durable, transposable dispositions,

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[5]For an interesting discussion of such partial interactions, understood as "local coordination"
rather than systematic translation and understanding, see [Galison, 1996, ch. 9].

structured structures predisposed to function as structuring structures.
... [1990, 53]

Bourdieu thus conceives of *habitus* as having a degree of stability not so different
from that posited in more traditional anthropological conceptions of culture. At
the other extreme, one might compare Steve Fuller's [1993, xv] characterization of
the basic conditions of knowledge transmission:

> Knowledge exists only through its embodiment in linguistic and other
> social practices, [which] exist only by being reproduced from context
> to context [through] the continual adaptation of knowledge to social
> circumstances [with] few systemic checks for mutual coherence...Given
> these basic truths about the nature of knowledge transmission, ...it
> is highly unlikely that anything as purportedly uniform as a mind-
> set, a worldview, or even a proposition could persist through repeated
> transmissions in time and space.

There is of course good reason to think that different social practices might vary
in their stability over time, such that the extent to which social practices sustain
a relatively stable background for individual action would be a strictly empirical
question, admitting of no useful general philosophical treatment apart from char-
acterizing some of the considerations that might generate continuity or change.

Much more fundamental differences arise concerning *how* patterns of social prac-
tice supposedly govern, influence, or constitute the actions of individual practition-
ers. This is perhaps the central issue for any practical-theoretical conception of
social life. If practices are temporally extended patterns of activity by multiple
agents (perhaps encompassing more than one generation of practitioners), then
the question of how this pattern is sustained, transmitted, and imposed upon sub-
sequent performances has to be a primary theoretical concern. Turner captures
the problem well:

> We often cannot understand what other people mean other than by
> translation, ... [and] often cannot understand what the behavior, ges-
> ture and doings of other people mean other than by consciously invent-
> ing and then selecting on the basis of observation a hypothesis that
> explains this behavior. But we know that the people we are attempt-
> ing to understand did not themselves acquire their capacity to speak
> a language through formal teaching or books, or come to understand
> one another's gestures and performances by consciously constructing
> and testing hypotheses. So there must be some way to acquire [these]
> capacities. The puzzle is how they are acquired. [1994, 46]

Turner [1994] argues forcefully that this puzzle has not and probably cannot be
solved in ways that would vindicate the aspirations of practice theories.

There are fundamentally two strategies for resolving Turner's puzzle so as to un-
derstand how the practices that supposedly provide a social/cultural background

governing individual performances are transmitted between practitioners and sustained over time. Taylor [1995] characterizes these two strategies as different ways of reading Wittgenstein's claim that "'obeying a rule' is a practice" [1953, I, par. 202]:

> There are two broad schools of interpretation of Wittgenstein, . . . two ways of understanding the phenomenon of the unarticulated background [to rule-following]. The first would interpret . . . the connections that form our background [as] just de facto links, not susceptible of any justification. For instance, they are imposed by our society; we are conditioned to make them. . . . The second interpretation takes the background as really incorporating understanding; that is, a grasp on things which although quite unarticulated may allow us to formulate reasons and explanations when challenged. [Taylor, 1995, 167–68]

Taylor cites Kripke's [1982] influential book on Wittgenstein as a clear example of the first strategy, but there is a long tradition of understanding socialization into shared practices as a matter of sheer imitation, training, and sanctions, which transmit and enforce the continuity of practices by straightforwardly causal means. Bourdieu perhaps most prominently exemplifies this strategy among practice theorists in the social sciences. For example, he claims that,

> The objective homogenizing of group or class *habitus* that results from homogeneity of conditions of existence is what enables practices to be objectively harmonized without any calculation or conscious reference to a norm and mutually adjusted in the absence of any direct interaction or . . . explicit coordination. [Bourdieu, 1990, 58–59]

Much of Foucault's *Discipline and Punish* [1977] also emphasizes the role of training in creating a conforming subject, for example, but in this respect he merely follows Nietzsche [1967, 61]:

> Man could never do without blood, torture, and sacrifices when he felt the need to create a memory for himself; the most dreadful sacrifices and pledges, the most repulsive mutilations, the cruelest rites of all the religious cults — all this has its origin in the instinct that realized that pain is the most powerful aid to mnemonics.

This strategy for understanding the transmission and maintenance of practices and norms is sufficiently familiar that Brandom [1994] could allow a caricatured example to stand in for it: "a prelinguistic community could express its practical grasp of a norm of conduct by beating with sticks any of its members who are perceived as transgressing that norm" [1994, 34].

This first strategy offers the advantage that, if it worked, it would make the normative and meaningful aspects of human behavior more readily intelligible by

solving several problems at once. Both the problem of understanding the character and functioning of irreducibly wholistic social or cultural phenomena, and the problem of understanding the authority and force of norms (which may, of course, just be a special case of the former problem) have long been found philosophically troubling. If practice theorists could provide a clear causal basis (in the form of relatively non-mysterious processes such as imitation, training, and sanctioning) for the institution and maintenance of social or cultural patterns exercising normative authority over individual performances, this would seem to constitute genuine philosophical progress. Adherents of the second strategy suspect that it cannot be done, and that rationality and understanding permeate social and cultural practices. For these latter practice theorists, the aim of practice theory is not to reduce social wholes to individual performances or norms to non-normative causal interaction, but simply to articulate insightfully and in detail how human understanding is inculcated and developed through social interaction.

Taylor himself distinguished these two strategies precisely in order to argue for the second approach, in which the transmission and uptake of practices always involves human understanding:

> We have to think of man as a self-interpreting animal. He is necessarily so, for there is no such thing as the structure of meanings for him independently of his interpretation of them; for one is woven into the other. ... Already to be a living agent is to experience one's situation in terms of certain meanings; and this in a sense can be thought of as a sort of proto-'interpretation'. [Taylor, 1985, 26–27]

How is this a conception of the transmission of patterns of practice? Taylor's point is that practitioners must learn a practice from the performances of others (presumably including their responses to correct and incorrect performances by oneself and others). Such learning is not merely a matter of imitating the movements of others or being trained or disciplined into correct performance by straightforwardly causal means, but instead requires appropriate uptake, which involves some *understanding* of the performance to which one responds. The capacity for such "proto-interpretive" uptake is presumably acquired gradually, as one's responses to earlier performances are assessed in light of a more extensive background of experience, including one's interpretation of others' responses to one's own previous performances ("our aim is to replace [a] confused, incomplete, partially erroneous self-interpretation by a correct one, and in doing this we look not only to the self-interpretation but to the stream of behavior in which it is set" [Taylor, 1985, 26]).

Note well that Taylor describes such a grasp of one's situation and possible responses in terms of meanings implicit in practices as nevertheless only "a sort of '*proto*'-interpretation". His qualification is intended to take account of the Wittgensteinian and Heideggerian criticism of regulism concerning norms. If our interpretive responses were themselves explicit articulations of the meaning of a performance, or resulted from a rule for generating new performances of the

practice, it would violate that fundamental insight behind practice theories. So the ability to learn how to participate in a practice must involve a grasp of other performances as meaningful without needing to (or perhaps even being able to) spell out explicitly what one has grasped. Just how one could possibly have a form of know-how that is more than causal product and less than explicitly articulated cognition will be a central theme of the next section.

Before turning to that point, however, I want to consider the possibility of combining Taylor's two Wittgensteinian strategies. Such combinations ought to evoke initial suspicion, because of the temptation to equivocate on the notion of a practice. If one were to use Taylor's first strategy when talking about how practices are transmitted between individuals, and his second strategy to characterize how the norms implicit in these practices affect subsequent performances, the result would seem superficially powerful. The straightforwardly causal mechanisms of transmission would render the resulting socially or culturally "wholistic" patterns unmysterious, while their richly meaningful content and normative force would enable them to have far-reaching effects upon individual performances and responses to them. The suggestion that the widespread appeal of practice theory turns on just such equivocations is integral to Turner's conclusion that this appeal is spurious.

There are nevertheless ways of combining the two approaches that need not depend upon conceptual sleight-of-hand. On such an account, "thin" forms of interaction and transmission would be necessary but not sufficient contributions to the transmission and maintenance of social practices. Language learning offers an especially clear illustration of how such conceptions would work. One could not learn to speak a natural language without the capacity to differentiate linguistic signs (phonemes, letters, gestures or whatever serves as the relevant tokens), and the ability and disposition to reproduce them by imitation. Babies babbling and imitating the sounds made by others are not yet language speakers, however. Language is holistic,[6] in the sense that a speaker cannot have the ability to understand and produce one sentence unless she can understand and produce many of them, in appropriately interconnected ways. So having acquired the causally-generated ability to imitate meaningful utterances, our proto-speaker must then somehow be able to pick up on their semantic significance. The realization of this capacity would undoubtedly require appropriate responses from others (additional utterances to imitate and respond to, but also appropriate corrections of and constructive responses to one's own performances). Speakers characteristically respond to language learners by treating them as if they had a capacity they manifestly do not yet have, by responding to their imitative utterances as if they were already meaningful performances. Yet such efforts to initiate others into the practice would not work unless these cues prompted (rather than merely causally provoking) the right kinds of response from the learner. Training and proto-interpretive or expressive uptake are both necessary, so as to produce not merely de facto conformity to social norms, but a self-policing conformism [Haugeland, 1982].

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[6]See note 3 above for the distinction I am drawing between the terms 'wholism' and 'holism'.

In fact, most practice theorists who explicitly address the issue (such as [Dreyfus and Dreyfus, 1986; Foucault, (1977) 1978; Schatzki, 1996; Brandom, 1994]) advocate such a hybrid combination of Taylor's two strategies. There are really only two grounds for defending the second strategy by itself. Phenomenologically-influenced practice theorists (Taylor himself is a good example) argue that there is no distinct component of merely-causal transmission of practices; even the infant language-learner is imbued with a richly affective sense of her surroundings and her own response to it as meaningful, however inarticulately. Practice theorists influenced by Davidson [1984; 1986] or Sellars [1963], on the other hand, may treat what the latter calls the space of causes and the space of reasons as parallel, non-intersecting domains of understanding, such that a theory of social practice could only avail itself of conceptual resources internal to the space of reasons.[7]

I shall return to the difference between merely causally-induced behavior and spontaneously produced performances of a practice in the second part of my discussion below, when I assess Turner's criticisms of practice theories. The next section, however, does discuss an aspect of practice theory that has often played a pivotal role in the effort to understand how social practices could transmit wholistic patterns of culture or society to new individual practitioners in ways that could constructively shape or govern their performances.

1.3   *Bodily Skills and Disciplines*

A third important theme in practice theory has been the central role of human bodies and bodily comportment. Emphasis within practice theory upon understanding human agency and social interaction as bodily performance has countered intellectualist conceptions of culture and social life, although the charge of intellectualism comes from many directions. Ortner [1984], for example, detects a strong Marxist-materialist background within practice-theoretical criticisms of a perceived tendency toward an idealist conception of culture as systems of symbols or meanings. Polanyi [1958], by contrast, mobilizes a conception of scientific understanding as bodily skill and "conviviality", in order to counter the Marxist-inspired aspiration to a socially-responsible administration of science prominently espoused by J. D. Bernal.

Undoubtedly, an important rationale for attending to bodily comportment is precisely the aspiration to reconcile the causal and normative dimensions of social life, or the simultaneously socially constrained/enabled and individually spontaneous character of human agency. The human body, as both causally affected and effective object in the natural world, and unified capacity for self-directed movement and expression, seems promising as a site for understanding how these apparently exclusive conceptual registers can be accommodated together. Prac-

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[7]These attempts to block any theoretical crossover between causal interaction and rational justification are more-naturalistically-respectable descendants of Kant's sharp distinction between the phenomenal realm of causally determined objects, and the noumenal realm of (possibly) free, self-determining rational beings.

tice theorists thus understand human bodies as both the locus of agency, affective response and cultural expression, and the target of power and normalization. The challenge, of course, is to characterize human bodily interaction with other bodies and a shared environment in ways that actually resolve these dual conceptions. The danger is that appeals to the role of the body in social life merely name the coincidence of the causal and normative conceptual registers, in ways that obscure their lack of reconciliation.

Taylor's two strategies for understanding Wittgenstein have evident counterparts in practice-theoretical conceptions of human embodiment. Some practice theorists characterize bodily dispositions or habits as the locus of continuity in social practices: a practice can be sustained over time because it is inculcated in the ongoing dispositions or habits of individual agents. For example, Bourdieu explicates his influential conception of the *habitus* by claiming that "the dispositions durably inculcated by the possibilities and impossibilities, freedoms and necessities, opportunities and prohibitions inscribed in the objective conditions...generate dispositions objectively compatible with these conditions and in a sense pre-adapted to their demands" [1990, 54]. Such conceptions exemplify the notion that causally instituted, de facto patterns of behavior provide the background that makes possible rule-following and other complex normative activity. Their appeals to imitation, repetition and imprinting, training, and sanctions make the human body the crucial intermediary in the transmission, acquisition, and reproduction of social practices. This first strategy always provides the temptation to an equivocation, however, in which one's resolutely causal account of the acquisition of habits or dispositions slips into an account allowing for much more richly expressive and flexible exercise of these austerely-acquired patterns of behavior. Bourdieu, for example, went on to characterize the habitus as both "the product of a particular class of objective regularities" and also as a form of "spontaneity without consciousness or will" [1990, 55, 56].

More commonly, however, practice theorists locate a continuous background to the discontinuous performances of a practice in bodily skills, and even bodily intentionality, rather than in mere dispositions or habits. Practice-theorists' discussions of skills seek an alternative to two apparently exhaustive ways of characterizing perception and action. On the one hand, there are the objectively-describable, causally-induced movements and internal processes of bodies as natural objects. On the other hand, there are actions in which the body is a more or less transparent medium for consciously reflective action. As cases that do not fit within these alternatives, for example, Polanyi cites an expert pianist's touch and an ordinary bicyclist's ability to maintain balance amongst a variety of countervailing forces. Here, he claims, "rules of art can be useful, but do not determine the practice of an art; they are maxims, which can serve as a guide to an art only if they can be integrated into the practical knowledge of the art [and] cannot replace this knowledge" [1958, 50]. The body becomes the locus of such "practical knowledge", which is neither merely causal conditioning nor consciously articulable rational action.

While such appeals to practical skill are common among practice theorists, Hu-

bert Dreyfus [Dreyfus, 1979; 1984; 1991; Dreyfus and Dreyfus, 1986] has developed an elaborated characterization of skills, drawing extensively upon previous work by Merleau-Ponty [1962], Heidegger [1962] and Todes [2001]. There are four crucial components to his account. First is the practical unification of one's command of one's own body, an implicit "I can" that is the bodily-intentional analogue to the Kantian "I think" that tacitly accompanies all mental representations. Unlike objects, whose motions can be decomposed into the separate movements of their parts, the entire body works together as a unity in skillful movement. Even when an action is focused in one bodily member, such as the arm or hand, such performance takes place against the background of a balanced, poised, directed bodily set that enables that effective focus. Second, bodily performances are intentionally directed toward objects, but without intentional intermediaries (such as meanings or spatial representations). One consequence of this conception is that there is no sharp distinction between perception and action, or bodily receptivity and spontaneity, for all bodily skills involve the coordination of bodily movement with a receptive responsiveness to one's surroundings. In order to grasp a teacup with my hand, I do not need to locate hand and cup perceptually in a three-dimensional space and then coordinate their intersection in practice. Rather, I direct my arm toward the cup itself, and responsively conform my hand to its contours, its delicacy, and its heft. The need to proceed in an explicitly representationalist way that human agents do not share turned out to be an insuperable obstacle to guiding effective robotic action by traditionally-conceived artificial intelligence [Dreyfus, 1979]. Dreyfus originally expressed his conception of skillful bodily practice as a phenomenological critique of early artificial intelligence, although he later pointed to the rise of parallel-processing, connectionist work as empirical vindication of these phenomenological insights.

This second point, the lack of intentional mediation to bodily intentionality, becomes especially important for understanding social interaction, since one can pick up on and respond to the expressive movements of others without having to infer their intentions or articulate their meaning. This immediacy of bodily interactions transforms Turner's challenge concerning the transmission of practices. Implicit in the concept of "transmission" is the notion that a performance is present and complete in one embodied agent, and then needs to be imparted to another agent in an equally self-contained form. But Dreyfus and others argue that bodily movement is not like that. The body is not merely interactive with its surroundings, but "intimately" involved with it, so as to efface any sharp boundary between them.[8] When one's skillful responsiveness is involved with the bodily performances of others, we get not the transmission of a skill from one agent to another, but the "dialogical" shaping of action, such that it is "effected by an integrated, nonindividual agent" [Taylor, 1991, 310]. At the most basic levels of bodily performance, human agency is realized through participation in practices

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[8]I owe this distinction between an "interaction" between clearly bounded components of a situation, and an "intimate" entanglement that cannot be usefully disentangled, to [Haugeland, 1999, ch. 9].

that are "ours" before they can be "mine".

Dreyfus's third point, the flexibility of bodily skills, contrasts skillful movement to ingrained habits or other causally induced repetitions. Skills do not merely repeat the same movements, or the same connections between environmental cue and bodily response. They instead permit a flexible responsiveness to changing circumstances. Instead of repeating the same sequence of muscular contractions, skilled performances manifest a common embodied sense, a directedness toward a goal through varying means. Having learned to spike a volleyball, I do not do the same thing again and again, but am instead capable of doing something slightly different each time, in response to slightly different circumstances. A bodily orientation toward a task, which requires varied performance under varying circumstances, is what we acquire in learning a skill.

Dreyfus does not deny, however, that there can be an element of explicit rule-following or repetitive movement in the acquisition of skills. His final point is that explicit rule-following and merely habitual motions are characteristic of novice rather than expert performance. When first learning a skill, we "go through the motions" in awkward, but explicitly specified terms. As these movements become more familiar, however, we can pick up on the pattern, in ways that leave the rule behind (indeed, often violate it). The early, halting and relatively ineffective initial movements are replaced by a different way of engaging the world with one's body. Earlier, I referred to Dreyfus as one who combined Taylor's two Wittgensteinian strategies, but he does so in a distinctive way. Causally-induced or rule-guided movements are an important part of the process of learning a practice, but only as precursors to a more effective mastery of a task which leaves behind all vestiges of its initial acquisition.

Practice theorists' emphasis upon bodily agency, intentionality, expressiveness, and affective response might initially seem to rest uneasily with the role of social constraint in practice theories. Yet practice theories do crucially insist that individual actions are shaped by social practices and the norms they embody, and often recognize the body as the primary target of social normalization and the exercise of power (e.g., [Foucault, 1977]). Can the spontaneous, expressive body, and the docile, normalized body inhabit the same organism? Perhaps surprisingly, this combination is often conceived not just as the compatible co-existence of opposing vectors of body-world relations, but as mutually reinforcing conceptions. Foucault, for example, identified the domain of *power* relations specifically in opposition to the merely causal imposition of force, and insisted that it was appropriate to speak of power as "including an important element: freedom. Power is exercised only over free subjects, and only insofar as they are free. By this we mean individual or collective subjects who are faced with a field of possibilities in which several ways of behaving, several reactions and diverse comportments may be realized" [Foucault, 1982, 221]. Foucault is hardly unique in this respect, however. A philosophical tradition going back to Kant and Hegel emphasizes a fundamental connection between freedom and normative constraint; social practices institute the very meanings, possibilities, and goods in terms of which human beings can

understand themselves and act for reasons. In Brandom's succinct formulation: "The self-cultivation of an individual consists in the exercise and expansion of expressive freedom by subjecting oneself to the novel discipline of a set of social practices" [1979, 195]. The distinctive contribution of practice theories in this respect is to locate both discipline and expressive freedom in coordinated bodily engagement with the world.

1.4 Language and Tacit Knowledge

Practice theorists' emphasis upon bodily skills or dispositions co-exists uneasily with the integral role of language in social life. Virtually every practice theorist treats this as an important theme, but they take it in some apparently discordant directions. Many theorists argue that practices have a crucial "tacit" dimension, a level of competence or performance prior to, and perhaps even inaccessible to verbal articulation. Practice theories are replete with reference to what can be shown but not said, or competently enacted only when freed from verbal mediation. Yet other practice theorists identify practices precisely by linguistically-articulated characteristics, such as shared presuppositions, conceptual frameworks, vocabularies, or "languages".[9] For these theorists, what unites the disparate performances of a practice is their linguistically-expressible background, which amounts to the practitioners' shared but unarticulated understanding of their performances. The conceptions of what a "practice" is thus range from understanding practices as pre-linguistic and perhaps inarticulable, to accounts of social life as thoroughly linguistically constituted. Still a third perspective on this issue arises in several influential strains of practice theory, which take language itself as an exemplary social practice. These conceptions of discursive practice variously draw upon the work of philosophers as diverse as J. L. Austin, Foucault, W. v. O. Quine, Jacques Derrida, Taylor, or Brandom.

Despite this apparent diversity in treatments of the role of language, all of these conceptions stem from different senses of the claim that social agents' understanding of their actions and interactions with others cannot be understood solely in terms of explicitly articulated and accepted propositions or rules. To this extent, the question of the place of language or discursive practice within practice theory is continuous with the influence of Wittgenstein's and Heidegger's criticisms of normative regulism, and also with the widespread emphasis upon bodily comportment. The extraordinary range of differences in their conceptions of language as part of social practices express different conceptions of the "tacit" dimension of social life.

Perhaps the most widespread version of this point emphasizes the shared "presuppositions" of some community or culture (with many other terms such as "tradition", "paradigm", "commitments", "ideology", "theory", or "research program"

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[9]For a more complete discussion of conceptual schemes in the social sciences, see Henderson, this volume.

used to express a similar point).[10] Turner thus concluded that,

> Together with such concepts as ideology, structures of knowledge, *Weltan-schauungen* and a host of other similar usages, the idea that there is something cognitive or quasi-cognitive that is 'behind' or prior to that which is explicit and publicly uttered that is implicit and unuttered became the common currency of sociologists of knowledge, historians of ideas, political theorists, anthropologists, and others. [1994, 29]

These themes became especially prominent through their emergence in the philosophy of natural science from the late 1950's through the 1970's. Against the prevailing logical empiricist claim that the norms of scientific reasoning could be expressed as purely formal, logical principles that any rational human being should endorse, Kuhn [1970], Toulmin [1962], Feyerabend [1962], Polanyi [1958] and Hanson [1958] and others argued that substantive commitments shared by scientific communities played an ineliminable role in actual scientific reasoning. Many practice theorists concluded that if even the natural sciences, an apparent exemplar of rationality, rely upon prior unarticulated commitments, then surely other areas of human activity do likewise.

Shared presuppositions play different roles in various conceptions of practices, however. Often their role was conceived as justificatory. Queries or criticisms of practitioners' performances would be met with enthymematic arguments whose validity depended upon the unarticulated presuppositions, whereas these presuppositions themselves were not given further justification even when articulated and questioned. Such conceptions of the role of presuppositions frequently invoked Wittgenstein's remark that at some point in seeking justifications for what I do, "I reach bedrock and my spade is turned" [1953, I, par. 217]. For these theorists, the crucial presuppositions of a practice were shared commitments that functioned as justificatory bedrock. The sense in which such presuppositions were "tacit" was that a social practice could and typically did proceed coherently in the absence of any explicit articulation of or agreement about these basic presuppositions. Practitioners responded to performances by others by acting in ways consistent with an acceptance of similar underlying beliefs, but without needing to express them, let alone justify them. Those who questioned these presuppositions were supposedly more often ignored or ostracized than answered.

A different conception of shared presuppositions often arose in practice theories more influenced by Heidegger, Gadamer, Dilthey, and the hermeneutical tradition.[11] Here presuppositions are invoked primarily in understanding how agents' participation in a practice makes sense (to the agent herself as much as to an interpreter). Taylor offers a clear example to illustrate this conception of practical presuppositions:

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[10]For further discussion of issues raised by conceptions of practices as constituted by shared presuppositions, see Jarvie, this volume.

[11]For further discussion of this tradition in the philosophy of the social sciences, see Outhwaite, this volume.

> The vision of society as a large-scale enterprise of production in which widely different functions are integrated into interdependence...is not just a set of ideas in people's heads, but is an important aspect of the reality which we live in modern society. And at the same time, these ideas are embedded in this matrix in that they are constitutive of it; that is, we would not be able to live in this type of society unless we were imbued with these ideas or some others which could call forth the discipline and voluntary coordination needed to operate this kind of economy. [Taylor, 1985, 46]

Here these ideas are tacit in the sense that they are so "obvious" to everyone embedded in such a social practice that they do not need to be said; indeed, many people may have difficulty recognizing the possibility of serious alternatives. Normally, they do not serve to justify actions so much as simply to render them intelligible. Nevertheless, they can be articulated, whether by social theorists aiming to understand what people do, by dissenters from the practices that incorporate this tacit vision of society, or by travelers who arrive with different preconceptions. Moreover, once these presuppositions have been brought to explicit attention, their role can shift toward justification: for example, a participant in these practices who has become more attentive to her constitutive commitments may now respond to dissenters by noting how much of what she values would have to be abandoned to institute an alternative matrix of social life. In contrast to those inspired by Wittgenstein's image of reaching justificatory bedrock, hermeneuticists claim that the process of interpreting social practices never ends. Anyone engaging in such interpretation, however, brings to it further unarticulated presuppositions, whose articulation would invoke still further background, and so on.

This sense of shared presuppositions as grounding the intelligibility of social practices sometimes carries over to a stronger sense in which they might be "tacit": their implicit acceptance might be necessary conditions for understanding the practice at all. Kuhn, for example, at some points talks about scientists who presuppose different research paradigms as having radically "incommensurable" conceptions such that they actually "work in a different world". The result is that in defending their points of view, they end up "talking through one another", failing to grasp adequately the meaning of one another's claims, either by literally misunderstanding them, or at least by failing to grasp what it would mean to engage in the practice from within such a conceptualization of the world [Kuhn, 1970, 103, 118, 132]. Such an account of tacit understanding might seem to make social science impossible, by making the sense of radically different social practices inaccessible to interpreters not already participants in them. Anthropology in particular might seem challenged by such a conception of cultural difference as involving constitutive presuppositions of social life. But Kuhn himself insisted that such radical incommensurability of social practices only prevented understanding other practices simply by translation into one's own familiar terms. The alternative route to cross-cultural understanding, one long integral to the self-conception of ethnographic practice, has been to immerse oneself in an alternative way of life as

a participant or participant-observer:

> To translate a theory or worldview into one's own language is not to
> make it one's own. For that one must go native, discover that one
> is thinking and working in, not simply translating out of, a language
> that was previously foreign. That transition is not, however, one that
> an individual may make or refrain from making by deliberation and
> choice... [Instead] he finds he has slipped into the new language with-
> out a decision having been made. [Kuhn, 1970, 204]

A further shift is often involved as one moves toward stronger senses in which
the presuppositions that constitute a practice are tacit rather than fully articu-
lated. In these stronger claims, the constitutive presuppositions of a practice are
often identified with something akin to a (natural) language rather than to specific
statements expressible within that language. The sense in which these presuppo-
sitions are tacit thus involves their constitutive role in shaping the very language
(and social context) in which any explicit articulation takes place. Wittgenstein
has also been highly influential on this theme as well, with frequent reference to
this passage from *Philosophical Investigations*:

> It is what human beings *say* that is true and false; and they agree in
> the *language* they use. That is not agreement in opinions but in form
> of life. [1953, I, par. 241]

Conceptions of practices as constituted by tacit presuppositions have been sub-
ject to a variety of telling criticisms. Those practice theories that interpret the pre-
suppositions of a practice as constituting justificatory bedrock have been claimed
to lead to an untenable or undesirable epistemological relativism.[12] Those theo-
ries that instead take different practices to presuppose mutually incomprehensible
or incommunicable ways of understanding the world or experience have also been
widely criticized. The more mundane critical responses have appealed to de facto
successes in interpreting apparently divergent social or cultural practices (com-
mitted defenders of conceptual incommensurability may, of course, question the
adequacy of such supposed successes). Davidson [1984, ch. 13] challenged ac-
counts of conceptual incommensurability more fundamentally, arguing that they
are committed to an incoherent distinction between a conceptual scheme and its
empirical or objective content.

Yet another line of criticism of interpretive appeals to tacit presuppositions has
been integral to Turner's attack on practice theory. His objection is to the iden-
tification of shared presuppositions as the basis for treating various performances
as instances of the same practice. The difficulty comes from the supposedly tacit
character of the presuppositions. First, there is a problem of underdetermination.
There are various ways to assign implicit premises to agents' performances so as
to justify them or make their meaning intelligible. Yet since the presuppositions

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[12]For more extensive discussion of this issue, see Jarvie, this volume.

are supposedly tacit, there is no evidence other than the performances themselves for choosing among alternative construals of the underlying presuppositions. The problem of underdetermination then points toward what Turner takes to be a deeper issue. Why should we think that there is any common basis at all underlying the diverse performances that an interpreter takes to be instances of the same practice? Turner concludes that the only legitimate standard for assigning tacit presuppositions to the supposed instances of a practice would be if there were a basis for demonstrating their "psychological reality" in individual cases. Otherwise, practice theory could not satisfy

> the need to connect the stuff of thought to the world of cause and substance. The predictive use of. . . the 'psychological' concept of presupposition and its variants depends on the idea that there is some substance to it, something with more continuity than the words or acts which exhibit the practice or presuppositions. . . . Unless we can proceed as if a practice were real, a cause that persisted, we would have no basis for using our past understandings or interpretations to warrant future interpretations. [Turner, 1994, 37–38].

Yet the interpretive character of practice-theoretical appeals to shared presuppositions provides no basis for connecting overt performances to underlying causal processes within individual psychology.

All of these conceptions of practices as constituted by shared presuppositions are what one might call "linguistic" conceptions of practices. Whether what practitioners tacitly share is a commitment to specific assertions within a language, or something more akin to the language itself, the notion of 'presupposition' suggests some form of semantic content. Those practice theorists who emphasize the bodily basis of practices, however, often emphasize a very different relation between language and social practice. If the crucial components of a practice are bodily skills, dispositions, habits, or other performances, then the description of the practice does not have the same kind of seemingly constitutive relation to the practice itself that is suggested by an identification of practices by their presuppositions. Marcel Mauss's [1979] discussion of distinctively French and American styles of walking provides a relatively early and widely discussed example of a non-linguistic practice. One might well describe this difference, as Mauss himself attempted, but there is normally no semantic content to how someone walks. Anthropologists especially have often been attentive to the kinesthetic character of cultural practices. Geertz's [1973, ch. 15] classic essay "Deep Play", for example, is replete with discussions of culturally exemplary ways of running, squatting, stroking the feathers of a fighting cock, and avoiding bodily acknowledgement of others, and this in an essay which then explicitly identifies such kinesthetic performances as akin to texts to be "read". Bourdieu, Dreyfus, Taylor, Polanyi and other practice theorists also emphasize a level of meaning and understanding which, if not utterly inaccessible to language, is nevertheless much more a matter of practical performance and perceptual recognition. The skillful know-how under-

lying social practices supposedly bypasses any verbal expression, even (or perhaps especially) in the process of its acquisition or transmission, which requires leaving rules behind in order to achieve a distinctively bodily capacity. Thus Dreyfus claims that, "in acquiring a skill ...there comes a moment when we finally can perform automatically, ...[having] picked up the muscular gestalt which gives our behavior a new flexibility and smoothness" [1979, 248–49]. On these conceptions of social practices, then, substantial aspects of social life and social understanding are fundamentally non-linguistic.

The question of whether and how we should understand social practices as linguistic or non-linguistic is further complicated by the conception of language itself in practice-theoretical terms. The ability to speak and understand language, after all, is very much a form of practical, bodily know-how. The difference between the halting, uneven speech of a language learner and the smooth, rapid flow of a fluent speaker (and the comparable difference in their perceptual skill in discriminating the words spoken by others) is an especially telling example of Dreyfus's distinction between expert skill and the incompetence of explicitly rule-guided action. The difficulty in following through with a conception of language-learning as the acquisition of a bodily skill is the apparent opposition between the supposedly tacit or inarticulate character of bodily skills, and the semantic content that is expressed through language use. Most philosophers who have acknowledged the importance of bodily skill in language use have tended to employ stratigraphic metaphors to incorporate both aspects of language. The practical and perceptual aspects of language use are taken to comprise one "level" of linguistic competence, while a grasp of semantics and pragmatics are regarded as another level, which is accessible to us in a different way.[13] The difficulty with these metaphors is that the supposedly different levels of linguistic understanding and competence are realized in exactly the same performances. There is no way to exercise semantic competence without also exercising the practical/perceptual bodily skills of a language speaker, for the performances of each are exactly the same performances. I will return to the question of how to think about these aspects of linguistic or discursive practice in the second part of the essay.

Perhaps because of this difficulty of integrating the practical-perceptual and the semantic aspects of language, most attempts to understand language use in terms of practice theory have considered linguistic or discursive practice solely at the level of pragmatics or semantics. The pragmatic aspects of language came to philosophical prominence through what is commonly called the theory of speech acts. J. L. Austin [1962] noted that many linguistic performances are actions performed through the use of words. Promising, commanding, christening, questioning, marrying, doubting, ruling out of order, sentencing a prisoner, proposing, suggesting, and a host of other cases exemplify actions that are typically performed by uttering appropriate words in felicitous circumstances. The circumstances matter, because in many cases, the successful performance of the act depends upon

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[13]For a good example of an explicit appeal to the metaphor of practical and semantic skill constituting different "levels" of linguistic competence, see [Dreyfus, 2002, 313–322].

them. I cannot marry two people by pronouncing them married, unless I have the appropriate institutional standing, and they are present before me having fulfilled additional institutional requirements. I similarly cannot rule a question out of order, christen a ship, or command you to do something without the appropriate standing in the right setting.

Much more intricate conceptions of the pragmatics of language have been developed within sociology in the form of conversational analysis and ethnomethodology.[14] These methodologically conceived programs have focused upon the kinds of social work done within everyday linguistic practice. They share with Austin's account of speech acts (and related work by [Grice, 1988] and [Searle, 1969]), however, a severing of their analysis of the pragmatics of language use from the determination of its semantics. These analyses of discursive practice take for granted that the meanings of the words and sentences used in conversation, or in specific speech acts, are determined by means other than the pragmatics of their use in context. Searle and Grice, for example, look to the psychological states of speakers (their beliefs, desires, intentions, and so forth) to determine the meanings of their words, and only on that basis examine the pragmatic work done by uttering those words in specific social circumstances. These practical-theoretical accounts are thus of limited scope; they are accounts of how linguistic meanings and structures instituted by other means are used as part of a social practice of conversation or to accomplish specific kinds of socially situated performance.

A more ambitiously practice-theoretical conception of language emerges from the work of W. v. O. Quine [1960] and Donald Davidson [1984; 1986], although it is not often expressed in those terms.[15] They propose to account for language use and understanding in terms of what they call "radical translation" or "radical interpretation". The practice of radical interpretation is taken to be a model of language use more generally. Ostensibly, it only seems to concern how to interpret the utterances and other behavior of someone else in terms of a language that I take myself already to understand. Such interpretation proceeds on the assumption that the speaker's performances are governed by norms of rationality.[16] The meaning of her utterances are determined by what makes the best systematic rational sense of them under their various circumstances of utterance. The presumption is that the speaker utters mostly truths and behaves mostly rationally.

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[14]For further discussion of ethnomethodology and its treatment of conversational language, see Lynch, this volume.

[15]Brandom [1976] and Rorty [1991] do explicitly include Quine and Davidson among those philosophers of language who conceive language as a "social practice" rather than a representation.

[16]This formulation in terms of rationality is Davidson's rather than Quine's. Quine had hoped to use the conception of language use as modeled by radical translation to facilitate a behaviorist reduction of semantics. Davidson eschewed any such reductionist project, taking norms of rationality as irreducible and constitutive of language and thought. Their views thus diverge in intent. I nevertheless include Quine as at least a precursor to a practice-theoretic conception of language, because his conceptions of radical translation and the principle of charity were crucial precursors to Davidsonian radical interpretation and Brandom's [1994] model of "discursive scorekeeping" as exemplary practice-theoretic models of language.

Only on this presumption can one plausibly use one body of evidence (the whole of the speaker's utterances and behavior in specific circumstances) to solve for two variables, their meaning and their truth value. Of course, one's interpretation is constantly changing in subtle ways as new evidence accumulates, such that radical interpretation is an ongoing practice. This conception of the interpretation of other speakers expands into a thoroughgoing practice-theoretic conception of language as soon as one recognizes that, for Quine or Davidson, to speak a language is implicitly to interpret one's own performances as rational in this way. In Quine's famous formulation, "radical translation begins at home" [1969, 46]. Thus, for Quine or Davidson, their semantic theories are attempts to express in an articulated theoretical model the capacities that are implicit in the performances of competent speakers of a natural language.

Davidson had employed a traditional representationalist structure (derived from [Tarski, 1944]) as a conceptual instrument for a practice-theoretic account of language, in which he modeled the interpretation (and self-interpretation) of speakers via a systematic representation of the language being spoken. The extent to which this was a model of linguistic practice rather than of the structure of a language became clearer in his influential [1986] "A Nice Derangement of Epitaphs", which claimed that linguistic ability involved "no learnable common core of consistent behavior, no shared grammar or rules, no portable interpreting machine set to grind out the meaning of an arbitrary utterance. . . . [T]here is no such thing as a language, . . . a clearly defined structure which language-users acquire and then apply to cases" [Davidson, 1986, 445–46]. Instead, there is only the activity of interpretation itself (of which speaking is also implicitly an example, as self-interpretation), which always outruns any systematic structure acquired or presupposed in advance.

Robert Brandom [1994] develops an even more thoroughgoing model of language use along broadly Davidsonian lines, but now explicitly presented as a practice-theoretic conception. Where Davidson modeled discursive practice as implicitly involving an interpretation of the idiolect of a speaker,[17] Brandom modeled discursive practice itself as "deontic scorekeeping" in which speakers keep track of the commitments undertaken and the entitlements accrued by fellow participants in the practice. Each subsequent performance calls for a revision of that participant's discursive score, her overall balance of commitments and entitlements. Brandom then shows how to account for logical and semantic concepts in terms of their expressive role in discursive practice. The account culminates in the effort to show how the representational dimensions of language use, including their accountability to speakers' causal interaction with objects through perception and action, can be understood in terms of norms implicit in discursive practice.

A somewhat different way of thinking about language as discursive practice arises in Foucault's work. Foucault's initial discussions of discourse and discur-

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[17]Strictly speaking, Davidsonian radical interpretation could just as well be applied to a collection or community of speakers, or alternatively to a fragment of the discursive performance of a single speaker.

sive practice resembled those accounts of practices that take them to presuppose a shared commitment to a language. Foucault [1972] was primarily interested in what he called "serious" speech, the effort to make authoritative knowledge claims. He then argued that the specific statements that circulate within a discourse only function as knowledge (*connaissance*) because they belong to a systematically interconnected "discursive formation" that specifies which statements are even candidates for serious consideration as truths and which other statements are relevant to their assessment. Moreover, in the human sciences, the more fundamental knowledge (*savoir*) articulated by the structure of these discursive formations incorporates the objects of knowledge as well as what is said about them: in these domains, he argued, the very objects of knowledge were constituted within discursive practice. In his later work, Foucault [1977; 1978] expanded this conception to give central place to seemingly non-discursive elements of these constitutive practices. Forms of bodily discipline, training, normalization (including practices of examination and confession) worked in concert with these discursive patterns to constitute new forms of knowledge and power that function together. These themes have been developed further in Butler's [1989] influential account of the discursive-performative constitution of gender, and her subsequent efforts [1993] to show how the body itself is "materialized" through such discursive performativity. Working within this theoretical orientation, Barad [forthcoming] argues that Butler's account does not adequately account for the materiality of discourse and embodiment, but Barad then develops an alternative conception of the performative character of "material-discursive practice" that proposes to remedy this deficiency.

These efforts to understand the linguistic or discursive dimensions of social practice, and to integrate them with a conception of material or bodily practice, have been among the most contested and conceptually difficult aspects of practice theory. I return to these issues in part 2, where I assess the challenges confronting practice-theoretical conceptions of social theory and social life.

1.5 *Social Science and Social Life*

Those practice theories that emphasize a tacit, inarticulate dimension to social practice give especially clear impetus to another theme. How should one conceive the relation between the presuppositions, norms, or skills implicit in social practice, and the effort to articulate this background explicitly within social science or social theory? Many practice theorists have been centrally concerned to theorize the relation between social inquiry and social life. At one extreme on this issue, consider once again Bourdieu, who sharply contrasts the stances of disinterested social scientists and habituated social actors:

> Science has a time which is not that of practice. For the analyst, time disappears. ... Only for someone who withdraws from the game completely, who totally breaks the spell, the *illusio*, renouncing all the stakes, ... can the temporal succession [of practice] be seen as a pure

> discontinuity and the world appear in the absurdity of a future-less,
> and therefore sense-less, present. [Bourdieu, 1990, 81, 82]

For Bourdieu, the social scientist's aspiration to a disinterested objectivity marks a sharp break between a practice-participant's understanding of what she is doing, which is deeply embedded in the bodily dispositions and action-orientation of a *habitus*, and the social scientist's articulated, tense-less understanding that is detached from any stakes in the practice itself.

Several other practice theorists also sharply distinguish the aspiration to scientific understanding from an understanding embedded in social practice, but to a very different end. MacIntyre, Polanyi, and Dreyfus each argues, on somewhat different grounds, that a practice-theoretic understanding of social life shows why a genuinely scientific understanding of social practices is fundamentally unattainable. For MacIntyre [1981], the mark of a genuine social science would be the articulation of social scientific laws, and the predictive power they would confer. Without such predictive capacity, the managerial or policy-making aspirations of social science would be baseless. Yet MacIntyre argues that social practices are indeed unpredictable, for multiple reasons: their interactive, "game-theoretic" character, their openness to constitutive conceptual innovation (which is unpredictable by the analyst in the sense that to predict a conceptual change in a sufficiently fine-grained way would be to bring it about already), the first-person predictive opacity of future decisions, and the pure contingency of some causal determinants of social practice. Polanyi [1958] argues against the social scientific analysis and administrative direction of practices on different grounds. Practices that draw upon skilled performances are not properly predictable or manageable, because the guidance and direction of a practice requires the skilled judgment of the practitioner rather than the rule-based analysis available to a social analyst. Dreyfus [1984] both extends Polanyi's argument through his more extensive analysis of skills, and interestingly applies it to the linguistic and conceptual dimension of social life. He claims that the concepts employed within and partially constitutive of social practices are structured differently than the concepts employed within any social scientific "theory". Using an example of gift-giving practices derived from Bourdieu [1977], he argues that a social scientist's analysis of gift exchange must always diverge from the participant's grasp of, for example, what differentiates "gifts" from trades, because the participant possesses a flexible responsiveness to novel situations that cannot be captured in a predictively successful social scientific theory.

The differences between Bourdieu and MacIntyre, Polanyi or Dreyfus concerning the possibility of social science highlight their more basic agreement about the essential character of genuinely scientific analysis in the social sciences. All insist that science must be objective, disinterested, predictive, and employing concepts whose proper use is determined intratheoretically. Without that agreement, their differences over whether a scientific analysis of social practice, so conceived, is attainable would be but a quaint curiosity. Not surprisingly, most other practice theorists reject this conception of the aspirations and norms of social science. One

alternative approach to the relation between social analysis and social practice is developed in various ways by hermeneutical practice theories (e.g., Taylor or most anthropological conceptions of practices), ethnomethodologists, and Foucauldian genealogists. These theorists take their own social-theoretical accounts of practices and their meaning or significance to be continuous with the "self-interpreting" character of social practices.[18] Precisely because the theorist's interpretation is itself situated within her own field of significant action, her account will never reach completion or closure, but it is not thereby rendered pointless. The point of social theory is itself situated within the field of ongoing activity to which it contributes.

Another prominent response to this issue has been to understand scientific inquiry as itself a practice, understandable in the same way as any other social practice. Not surprisingly, this possibility has made the practice idiom especially attractive to social studies of science. If both science and social life more generally are best conceived as "practices", that would give clear impetus to the aspiration to a social science of science.[19] Yet this conception of social science as a kind of meta-practice has also raised serious and far-reaching questions about the epistemic, political, and rhetorical aims of social scientific inquiry, and its representation of other agents and practices. Within anthropology, these problems were centrally posed in Clifford and Marcus [1986] and remain live issues throughout the discipline; within science studies, they have been widely discussed under the heading of "reflexivity" or "diffraction".[20] Perhaps the most striking character of these debates has been the deep disagreements over the locus of the challenge to social scientific practice. Is the difficulty epistemic (and thus continuous with, or perhaps radicalizing, familiar debates about social and cultural relativism), moral and political (the not-always-intended alliance between the quest for authoritative knowledge and the influence of hegemonic political power, and/or the role of authoritative scientific representation in preempting or silencing the self-presentation of social actors), or rhetorical (a quest for new forms of writing and representation that undermine or supplement the implicit authorial authority of the writer or theorist)? Both rhetorically and politically, these arguments have often suggested that the authorial or cultural self-understanding of the inquirer is as much or more at issue in the practice of social science as are the practices of the ostensible "ob-

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[18]These theorists differ, of course, in their conception of how social practices are "self-interpreting", including whether 'interpretation' is the appropriate concept (ethnomethodologists emphasize conceptions of practical knowledge and judgment that do not treat participants in practices as "judgmental dopes" [Garfinkel ,1967]; Foucault [1970; 1977; 1978] seeks to conceive his own genealogical "history of the present" as disclosing and partially resisting the networks of power/knowledge within which it is situated, without invoking variations on an "analytic of finitude" that conceives human agents as both transcendental subjects and empirically determined objects, so he would surely have taken 'interpretation' to be problematic on these grounds).

[19]For extensive discussion of the practice idiom within science studies, see [Pickering, 1992]; [Rouse, 1996b; 1999]; [Rabinow, 1996], and [Barad, forthcoming].

[20]Prominent discussions of reflexivity within science studies include [Woolgar, 1988]; [Ashmore, 1989]; [Pickering, 1992]. The claim that 'diffraction' is a more appropriate light metaphor than 'reflection' is developed by [Haraway, 1997], and [Barad, forthcoming].

jects" of social scientific inquiry (see, for example, [Marcus and Fischer, 1986]; [Rosaldo, 1989]; [Traweek, 1992]).

1.6 *Practices and the Autonomy of the Social*

My final expository theme is the frequent appeal to a conception of practices as the proper domain of the social sciences, in order to secure their disciplinary or conceptual autonomy. The most common challenges to conceptions of sociology and anthropology as distinctively "social" sciences have come from psychology (especially conceptions of instrumental or computational rationality in cognition and action), neoclassical economics, and evolutionary biology. These challenges are discussed extensively in other contributions to this volume, so I will only briefly highlight the distinctive contributions of practice theory to these debates.[21] The principal features of social practices that might make them immune to reductive treatment in psychological, economic, or biological terms have already been presented in the preceding sections. The historical and cultural particularity of practices, and the ways in which the meaning of individual performances of a practice depend upon their particular context are perhaps the most frequent grounds for appeal to practice theory in defense of an autonomously *social* science. Practice theory would thereby resist any reduction of social context to the thoughts and actions of individual agents by showing how to understand the latter as dependent upon the constitution of meanings that are irreducibly social, without thereby being ontologically mysterious or epistemically inaccessible. The emphasis upon a level of bodily disposition, discipline, or skill that cannot be made fully explicit as rules or conscious intentions has also been prominently employed to challenge the encroachment of instrumental or computational conceptions of rationality upon the social constitution, comprehension, and deployment of meaning.

   Practice theory may also go beyond merely preserving an autonomous domain of social science, by challenging the conceptual or disciplinary autonomy of psychology, neoclassical economics or biology in turn. Within economics itself, the imperial aspirations of neoclassical models of individual economic behavior have already been qualified by widespread recognition of the ineliminable importance of specific institutional contexts in mediating economic behavior, in ways that resonate with practice theory in sociology and anthropology (for example, see [Rutherford, 1996]). Practice theory may offer a more radical challenge to any psychological reduction of social practices, however. Dreyfus long ago noted that the domain of distinctively psychological theorizing occupies a curiously intermediary position between biology and higher-level descriptions of socially-situated action:

   The brain is clearly a physical object which uses physical processes
   to transform energy from the physical world. But if psychology is to

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[21]For further discussion, see Shweder, this volume; Roth, this volume; Pizzorno, this volume; Zahle, this volume; and Haines, this volume.

> differ from biology, the psychologist must be able to describe some level
> of functioning other than the physical-chemical reactions in the brain.
> [Dreyfus, 1979, 163]

For most of the efforts to understand social life in psychological terms, this distinctively psychological "level" of functioning is characterized in the terms of so-called "folk psychology", the attribution of beliefs, desires, hopes, intentions, and other propositionally contentful states to individual agents, as part of a psychological explanation of what they do. There are at least two distinct kinds of practice-theoretical challenge to this strategy of psychological reduction of social life. Those practice theorists that emphasize the role of bodily skills (especially [Dreyfus, 1979]; [Dreyfus and Dreyfus, 1986]; [Haugeland, 1998]) deny that there need be any semantically contentful psychological intermediaries between the description of bodily action at the biological level, and its description in terms of socially and culturally situated practices. These practice theories thus suggest that ordinary perception and action often has no appropriate description at the intermediary "psychological" level, but is appropriately and perspicuously described and explained in practice-theoretical terms.

A more radical and far-reaching practice-theoretical challenge to folk-psychological conceptions arises within some theories of discursive practice. The objection is that the supposedly characteristic psychological categories of belief, desire, intention, perception, and other "propositional attitudes" in fact do not refer to *psychological* states at all, but instead characterize "normative statuses" that are constituted within distinctively *social* practices. Brandom [1994] offers the most extensively developed version of this line of argument. He points out that the propositional attitude concepts are ambiguous. When they refer to intentional contents that speakers or agents explicitly endorse (or would endorse upon reflection), they might plausibly be mistaken for psychological states that might somehow be physically realized in people's minds or bodies. But these concepts are also appropriately used to characterize commitments that other agents attribute to someone to make best rational sense of her actions. He then proposes a unitary conception of these two kinds of ascriptions, as normative statuses taken on through participation in public, discursive practices. The apparent difference between two kinds of belief or desire would then simply mark two ways of acquiring the normative status of a semantically contentful commitment (or entitlement) within a social practice, by first-person avowal and third-person ascription. Brandom's model of discursive practices would thus obviate any intermediary cognitive-psychological "level" between neurophysiology and social practice, understanding the ascription of individual beliefs and desires as part of a complex, socially-articulated discursive practice. Rouse [2002] then expands the scope of Brandom's argument by extending it to encompass perception and action as well as belief, desire or intention, thereby integrating Brandom's challenge to the autonomy of psychological explanation with that posed by practice theorists who emphasize bodily skills.

2   CONCEPTUAL PROBLEMS IN PRACTICE THEORY

The six themes I highlighted in the first part of the paper provide some unity to the various projects in sociology, anthropology, social theory, and the philosophy of social science that have been characterized as contributions to practice theory. My discussion showed the considerable diversity and controversy that persists amidst this thematic unity, because various practice theorists develop these themes in different, and sometimes opposing directions. In this section, I shall address a different kind of controversy concerning practice theory. The issues that concern me now are not simply points about which practice theorists disagree, but issues that may pose conceptual difficulties for practice theories collectively. There are three such points that I shall address. The first issue is whether the appeal to practices can actually resolve the problems about justification and normativity that were highlighted by Wittgenstein and Heidegger. The second concerns the conception of meaning and its explicit articulation that underlies the distinction sometimes invoked in practice theories between what is (or can be) explicitly formulated in rules or language, and the tacit, perhaps inarticulable background to such formulations. The final issue is the significance of conceiving practices as "social" practices, that is, as characteristic forms of human interaction which can largely be abstracted from their material embodiment and environment.

2.1   *Two Concepts of Practice in Response to Normative Regulism*

We have seen that practice theories are motivated in substantial part by Wittgenstein's and Heidegger's criticisms of normative regulism, which identified the understanding of norms or meanings with grasping and following rules (see section 1.1 above). The question I raise here is whether and how practice theories can successfully account for such understanding in ways that avoid the incoherence of regulist conceptions. Regulism about meanings and norms was incoherent, because rules are themselves meaningful and normative. If understanding a rule and following it correctly requires understanding and following yet another rule that interprets the first rule, then we will never arrive at an account of meaning and normativity. The most common conception of how practice theories overcome this problem is by suggesting that the regress of rules comes to an end in a regularity *exhibited* by what practitioners do, rather than in a rule *followed* by them. In accord with Brandom, I call this alternative a "regularist" conception of practices and the norms that govern them.[22]

   The inspiration for regularist practice theories frequently stems either from Heidegger's insistence that an anonymous conformity to what "one" does *(das Man)* is an essential structure of human existence, or Wittgenstein's remark (PI 217) that, "If I have exhausted the justifications [for following the rule in the

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[22]Brandom [1994] introduces the terms 'regulist' and 'regularist' for conceptions of normativity in terms of rule-following and regularity-exhibiting, respectively. His own principal criticisms of regulist and regularist conceptions are on pp. 20-26 and 26-29 respectively.

way I do], I have reached bedrock, and my spade is turned. Then I am inclined to say, 'This is simply what I do"'. The result is to conceive a practice as an exhibited regularity that underlies and undergirds action according to explicit norms or rules. Whether Wittgenstein's or Heidegger's own accounts amount to a regularist conception of practices is a more controversial question; in my view, neither endorses a regularist conception of practices, and indeed, they both develop significant criticisms of regularism, but this is not the place to defend my interpretation of their work.

Turner's [1994] criticisms of practice theories are directed exclusively against regularist conceptions of practices (indeed, he does not acknowledge any alternative to such conceptions). His exposition of social practice theories instead highlights the difference between conceiving the regularities that are constitutive of a practice as semantically contentful presuppositions, or as prior and perhaps even inaccessible to semantic articulation. In the former case, he argues that practice theories cannot account for the psychological reality of the attributed presuppositions; in the latter case, he claims that the causal efficacy of the underlying behavioral regularities cannot be explicated; moreover, in neither case can practice theorists account for the transmissable identity of the regularities that they posit to explain the normativity of social life. Given these difficulties, Turner concludes that there is no adequate evidential basis for the claim that there are regularities of performance behind the manifestly diverse phenomena of social life.

Turner is right to acknowledge a difference between semantically articulated presuppositions and shared patterns of behavior, and also correct to criticize both ways of conceiving practices in terms of underlying regularities. His objections to the psychological reality, causal efficacy, and transmissibility of these regularities nevertheless do not quite get to the heart of a more fundamental difficulty confronting *any* regularist conception of norms or meanings. Regularist conceptions of norms run up against what Brandom [1994] called the gerrymandering problem: a finite set of performances exhibits indefinitely many regularities. One can in principle always identify various performances as instances of the same practice in multiple ways, with no grounds to identify the relevant "practice" (or its presuppositions) with any one of them. These alternative conceptions of the underlying regularity would of course provide differing verdicts as to whether subsequent performances were in accord with prior practice, but the resulting conception would remain underdetermined even by the additional evidence, since the gerrymandering problem recurs. Regularist appeals to exhibited rules thus *cannot* resolve the difficulties confronting regulist conceptions of normativity as rule-following.

There is, however, an alternative conception of practices and normativity that does not reduce them to rules or regularities.[23] On this conception, a practice is not a regularity underlying its constituent performances, but a pattern of interaction among them that expresses their mutual normative accountability. On this "normative" conception of practices, a performance belongs to a practice if

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[23]For a more extensive treatment of this conception of practices, see [Rouse, 1999], and [Rouse, 2002], especially chapters 6-9.

it is appropriate to hold it accountable as a correct or incorrect performance of that practice. Such holding to account is itself integral to the practice, and can likewise be done correctly or incorrectly. If incorrectly, then it would appropriately be accountable in turn, by responding to it as would be appropriate to a mistaken holding-accountable. And so forth. Such a conception of practices, as constituted by the mutual accountability of their constituent performances, can be retroactively identified in many familiar practice theorists. For example, Brandom once suggested that "we can envisage a situation in which *every* social practice of [a] community has as its generating response a performance which must be in accord with another social practice" [1979, 189–90], and must ultimately be accountable to an "essentially perspectival", "token-reflexive" conception of objectivity [1994, ch. 9]. MacIntyre's conception of a tradition also exemplifies a normative conception: "What constitutes a tradition is a conflict of interpretations of that tradition, a conflict which itself has a history susceptible of rival interpretations." [1980, 62]. Further examples include Davidson's [1986] denial that discursive practice depends upon a shared language, and my rereading [Rouse, 1999] of Turner's own reinterpretation of Mauss on French and American ways of walking. Wittgenstein's suggested invocation, "This is what we do" can also be appropriated within a normative conception of practices if given the inflection with which a parent tells a child, "We don't hit other children, do we?"; such an utterance does not describe a regularity, but instead holds a prior performance accountable to a norm.

Turner is not alone in failing to recognize even the possibility of a normative conception of practices. Such a conception is difficult to recognize *as* a conception of practices, because it amounts to something like a Galilean or Copernican revolution in philosophical understanding of normativity. Philosophers have long been suspicious of normativity, regarding it as acceptable only when reducible to or otherwise explicable by what is non-normative. Typically, normativity has been characterized in terms of the presence of a special kind of entity (such as values, rules, regularities, commitments, or preferences), or in terms of another modality (such as rational, transcendental, or social necessity). Note well that a regularist conception of practices itself exemplifies the familiar strategy of explicating normativity by reducing it to something non-normative, in this case the exhibiting of a regularity. A normative conception of practices instead makes normativity irreducible. Such irreducibility does not make normativity inexplicable, however.

There are at least three crucial aspects to the explication of a normative conception of practices. First, the bounds of a practice are identified by the ways in which its constitutive performances bear upon one another, rather than by any regularities of behavior or meaning that they encompass. One performance expresses a response to another, for example, by correcting it, rewarding or punishing its performer, drawing inferences from it, translating it, imitating it (perhaps under different circumstances), circumventing its effects, and so on. Not surprisingly, such conceptions have most commonly arisen in accounts of discursive practice. Latour and Woolgar (1986, ch. 2), for example, treat statements within a scientific practice as implicitly "modalizing" other statements, whether by explicitly

referring to and qualifying them ("S claims that p", "S has shown that p", "it is widely acknowledged that p", "despite some recent ill-founded claims to the contrary, $\sim p$", and so forth), or by implicitly referring to them, perhaps by taking them for granted or ignoring them.[24] Brandom's [1994] model of discursive practice as "deontic scorekeeping" offers a much more general conception of an interactive field of performances, mediated by each participant's implicit tracking of the commitments and entitlements accrued by the various participants, such that each subsequent performance affects the significance of others by changing the score. Foucault's conception of power, as "a mode of action which does not act directly and immediately upon others, [but] instead acts upon their actions" [1982, 220], does expand such conceptions beyond the explicitly discursive realm, however. Wartenberg [1990] offers a useful gloss upon this conception, by explicating how the action of one action upon another is mediated by what he calls a "social alignment":

> A situated power relationship between [the performances of] two social agents is thus constituted by the presence of peripheral social agents in the form of a *social alignment.* A field of social agents can constitute an alignment in regard to [the performances of] a social agent if and only if, first of all, their actions in regard to that agent are coordinated...comprehensive[ly] enough that the social agent facing the alignment encounters that alignment as having control over certain things that she might either need or desire...The concept of a social alignment thus provides a way of understanding the "field" that constitutes a situated power relationship *as* a power relationship.[25] [Wartenberg, 1990, 150]

The result is a conception of practices whose performances are integrated within the practice not by a shared semantic content or behavioral similarity, but as a complex network of mutual interaction.

Such networks of mutually interactive performances are not yet normative, however, and hence not yet identifiable as practices. The second crucial feature of practices, normatively conceived, is that these patterns of interaction must constitute something at issue and at stake in their outcome. MacIntyre provides a useful illustration of this point: "If I am a Jew, I have to recognize that the tradition of Judaism is partly constituted by a continuous argument over what it means to be a Jew" [1980, 62]. What it is to be a Jew is at issue in the practices of Judaism

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[24]Latour and Woolgar's account of such modalities, and Latour's [1986] later expansion of the conception, do not adequately articulate a normative conception of scientific practice in my view, but they do exemplify the conception of practice-constitutive performances as mutually responsive to one another.

[25]Wartenberg himself talks about alignments of social agents rather than of their performances. I have interpolated the Foucauldian notion that power relations are between actions rather than agents. Elsewhere [Rouse, 1996b, ch. 7], I have argued that his characterization solely in terms of social relations between agents also inappropriately omits the role of agents' physical environment and the things, processes, and interactions it contains.

in all their historical complexity; what is at stake in those practices is the difference it would make to resolve that issue one way rather than another. But that difference is not already settled; working it out is what these practices continue to be "about". The issues and stakes constitutive of practices thus indicate the temporality of practices and their normative accountability: practices point ahead of themselves toward something essentially contestable. For a performance to be accountable to norms is not merely for it to interact with other performances, but to do so in a way that can be understood to be for the sake of something at stake in the interaction and its consequences.

Most philosophical conceptions of normativity presume that there must be some determinate norms that already govern the performances accountable to them, and thus that already settle what is at stake in the practices to which those performances belong. Such conceptions might allow for epistemic uncertainty about these norms on the part of the practitioners, but not metaphysical indeterminacy. Normative practice theories, however, take the issues and stakes in practices to be indeterminate (or perspectively variant), and this amounts to a third crucial feature of their conception of practices. Samuel Wheeler strikingly presents such indeterminacy in the case of the semantic and epistemic norms at stake in discursive practice:

> If truth is a matter of norms, of what "we" say and when we say it, and there is a struggle about what is to be said, truth is loose. We should not think that somehow the truth is already there, waiting to be discovered. "Is true" is like "is a turning point", "is the winning run", or "is a decisive play." Such concepts can only be applied retrospectively. [1990, 132][26]

Brandom characterizes the commitment to the objectivity of conceptual norms (which he takes to be constitutive of the normativity and semantic contentfulness of discursive practices) as essentially perspectival rather than as indeterminate, but he is making a similar point:

> Each perspective is at most *locally* privileged in that it incorporates a structural distinction between objectively correct applications of concepts and applications that are merely subjectively taken to be correct. But none of these perspectives is privileged *in advance* over any other. ...Sorting out who *should* be counted as correct, whose claims and applications of concepts should be treated as authoritative, is a messy retail business. ...[T]here is no bird's-eye view above the fray of competing claims from which those that deserve to prevail can be identified ...[1994, 600, 601, my emphases]

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[26]Strictly speaking, the concepts *can* be applied prospectively, and are so applied whenever someone makes a truth claim. The correctness of their application can only be settled retrospectively, however.

Foucault likewise rejects any "sovereign" standpoint "above the fray" from which competing political or epistemic claims can be definitively assessed, colorfully expressed by the claim that "in political thought and analysis, we still have not cut off the head of the king" [1978, 88–89].[27] For such views, the normativity of practices is expressed not by any regularity among their performances, or by any already determinate norm to which they are accountable, but instead in the mutual accountability of their constitutive performances to issues and stakes whose definitive resolution is always prospective. Normativity is an interactive orientation toward a future encompassing present circumstances within its past.

This rejection of even the possibility of a sovereign standpoint that could definitively resolve perspectival differences or overcome the metaphysical indeterminacy of what is at issue and at stake in social practices thereby also commits normative practice theories to the continuity of social theory and social life. On such a conception, the performances that contribute to a practice at least implicitly already express an interpretation of what is at issue and at stake in the practice. Moreover, any effort to stand outside of an ongoing practice and definitively identify the norms that govern its performances is instead incorporated within the practice, as one more contribution to shaping what the practice will become. As Arthur Fine nicely summarized this point in the case of scientific practice,

> If science is a performance, then it is one where the audience and crew play as well. Directions for interpretation are also part of the act. If there are questions and conjectures about the meaning of this or that, or its purpose, then there is room for those in the production too. The script, moreover, is never finished, and no past dialogue can fix future action. Such a performance... picks out is own interpretations, locally, as it goes along. [1986, 148]

Such a conception of normativity is especially suitable for naturalists, since it deliberately eschews any determination of norms from a standpoint outside of nature and history, yet it is also non-reductive. The causal nexus within which an action is situated does not determine its normative significance, but it does substantially affect it. Indeed, within normative conceptions of social practice, the concept of power takes on a central role precisely in order to express the relations between causes and norms. 'Power' does not denote a substantive capacity within the world (it is distinct from force or violence, for example); instead, it expresses how one action affects the situation in which other actions occur, so as to reconfigure what is at issue and at stake for the relevant actors.[28]

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[27]Foucault's willingness to extend his criticism of the role of sovereignty within political theory to a comparable criticism of "epistemic sovereignty" is only implicit in his account of the mutually constitutive relations of power/knowledge. Rouse [1996a] develops an explicit criticism of the aspiration to a standpoint of epistemic sovereignty.

[28]Rouse [2002] explicitly defends both this conception of power as expressive rather than denotative, and its contribution to a naturalistic conception of normativity. Similar conceptions of how actions affect the normative significance of other actions can be readily identified elsewhere,

2.2 *Language, Presuppositions, and Discursive Practices*

In section 1.4, we saw that the apparent diversity of practice-theoretical treatments of language marked different interpretations of a widely shared commitment that social agents' understanding of their actions and interactions with others cannot be understood solely in terms of explicitly articulated or articulable propositions or rules. This commitment in turn results from practice theorists' acceptance of Wittgenstein's and Heidegger's criticisms of normative regulism. John Haugeland usefully expresses this common stance toward language as a rejection of what he calls "the first dogma of rationalism", the fundamentally positivist claim that "reality is 'exhausted' by the facts — that is, by the true propositions" [forthcoming, 1]. Something seems importantly right about the practice-theoretical criticism of this rationalist dogma. After all, we do many things without ability or need to say them, and our understanding of what we say depends upon many non-linguistic capacities. Moreover, further articulating such matters verbally does not leave the original skills and activities unchanged. Yet difficulties also confront the attempt to characterize some aspects of our skills and dispositions as essentially tacit or inarticulable. The problem is not just one of having to *say* the allegedly unsayable; it is also unclear how to specify in advance the limits of language or linguistic expressibility.

Underlying these difficulties, I think, is a widespread confusion in many such discussions concerning what it is to make something explicit. Defenders of inarticulable knowhow seem to conceive a contrast to explicit articulation as a kind of complete verbal counterpart to what is described; Haugeland's formulation captures it well with the notion that some portion of reality might be "exhaustively" described. His positivist "first dogma of rationalism" would then be the claim that the exhaustibly describable portion of reality is its proper subset. What worries me, however, is the more basic presumption that *whatever* portion of reality is explicitly described is thereby somehow "exhausted". On such conceptions of what it is to make explicit, an assertion represents something, and whatever portion of reality it represents, it represents completely. Moreover, to understand a proposition would then be to grasp its representational content completely. The world would then divide neatly into those portions that are representable and understandable in this way, and those that are not.

The problem with this conception is that it seems to me a hopelessly untenable account of linguistic description or conceptual articulation. The history of early 20th Century philosophy of language can be written as a story of failed attempts to realize such a conception of linguistic expression as something fully present to

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however. Most obviously, Foucault's [1977; 1978] account of power/knowledge embodies such a conception. Yet the central argument of MacIntyre [1981] also implicitly treats actions as causally reshaping the normative significance of subsequent actions. His claim is that the conjoined effect of conceptual innovations of modern moral theorists and the emergence of managerial and therapeutic practices has been to change what is at issue and at stake in moral life today; the subtitle of his concluding chapter, "Nietzsche *or* Aristotle, Trotsky *and* St. Benedict", was intended as a capsule expression of his conception of this reconfiguration of those issues and stakes.

the mind, whether in the form of Fregean (or Husserlian) senses, Tractarian pictures (whose representational content can be said, but whose pictorial form can merely be shown), Carnapian formal structures, and so forth. Quine's [1960] and Davidson's [1984] criticisms of the analytic/synthetic and scheme/content distinctions are prominent markers for the failure of any such autonomous conception of linguistic expression. Davidson concludes that such criticisms "erase the boundary between knowing a language and knowing our way around the world generally" [1986, 445–46]. In doing so, they erase any boundary between what can be said in language, and what cannot, not because everything is expressible, but because what it is to express something in language (and to understand what is expressed) is integral to a more extensive practical competence. Indeed, those practice theorists who infer from Heidegger's and Wittgenstein's criticisms of regulism the claim that some parts of reality are essentially inarticulable betray an incomplete understanding of the consequences of those criticisms. Only by exempting language itself from the criticism of regulism, and thereby banishing linguistic meaning from the world into an extraworldly realm of Fregean senses, Husserlian transcendental consciousness, or Carnapian logical form can one preserve a boundary between the expressible and the essentially tacit.

Recognizing how thoroughgoing the criticism of regulism must be thus strongly supports those approaches that incorporate the understanding and use of language within practice theory. To use and respond to words and sentences as semantically significant is to engage in discursive practice. There is a rich and diverse philosophical literature along these lines, from speech act theory, to Davidson or Brandom, to Foucault, which I have already discussed in section 1.4 above. We can now, however, draw one final and telling conclusion about such conceptions. To talk about discursive practice in this way is not to draw a boundary between discursive and non-discursive practice. Language is itself a social practice that integrally involves a rich practical and perceptual engagement with our surroundings. Indeed, language use itself involves complex bodily skills. But the discursive and the non-discursive are inseparable, not only because discursive practice involves much more than just word use, but also because the much more finely-grained articulation that language makes possible transforms everything else we do. Instead of treating language as an autonomous domain of representation, the best practice theories consider language a pervasive and irreducible aspect of human ways of life. Rather than talking about "language" as a distinctive kind of entity, skill, or structure, such theories emphasize the semantically articulated normativity of all human activities and institutions.

2.3   The Social and the Biological

I will address the third and final conceptual problem within practice theory more briefly, largely because a comparably thorough discussion would take me too far afield. Most practice theories primarily concern *social* practice, that is, the situated doings of human agents as interactive with those of other human agents. Vir-

tually everyone acknowledges that social agents are "also" natural entities causally interactive with their material surroundings, and perhaps more strongly, that social practices depend upon physical and biological capacities of human beings and their environment. Having acknowledged such interaction, however, most theorists then treat social interaction as more or less autonomous from its physical and biological capacities and circumstances. Talk of a more or less autonomous world of meaning, rationality, normativity, or social practice, realized "in" the natural world but conceptually distinct from it, has become the philosophically respectable way to sustain an analogue to Kant's dualism between the phenomenal world governed by natural laws, and the noumenal world of actions according to a rational conception of law.

I nevertheless think it is a mistake to distinguish the social world from its natural environment in this way, such that practice theory would make the social world the domain of autonomously social sciences. Moreover, this mistake is one that practice theory is especially well equipped to overcome. It is not sufficient to acknowledge that the social and natural worlds "interact". Adopting another distinction from Haugeland [1999, ch. 9], I take the important alternative to conceiving social and natural "worlds" as interacting to be recognizing their "intimate" interconnectedness. Haugeland introduced this distinction between "interaction" and "intimacy" to reject not only any clear boundary between mind and body, but also between body and world, concluding that "human intelligence abides in the meaningful, which...extends to the entire human world" [1999, 237]. I would add that the human world and the supposedly inhuman world of nature are *also* too entangled to allow clear and useful boundaries between them.

Practice theories provide multiple reasons to insist upon the intimacy of natural and human worlds. One reason for such insistence is continuous with Haugeland's challenge to the autonomy of mind and body: social practices are embodied, and the bodily skills through which they are realized are intimately responsive to the affordances and resistances of their surroundings. A second consideration arises from the integral role of equipment and "material culture" more generally in human practices. The recurrent difficulty of clearly distinguishing socially instituted norms of correct performance from instrumental norms of success and failure calls for a conception of "practice" that cuts across any boundary between normative social interaction and its causal-environmental nexus. Similar difficulties arise at a macro level in the intertwining of environmental and social or political history. Yet a third reason to recognize the intimate entanglement of nature and social practice arises from the semantic externalism needed for an adequate conception of discursive practice. We cannot understand the normativity of language simply in terms of intralinguistic relations, and/or the pragmatic interrelations between speakers. Language use is intimately connected to the circumstances in which utterances are made. That point parallels my claim in section 2.2 that there can be no interesting boundary between discursive and non-discursive practice.

Language and discursive practice invite further reflection upon what is at stake in the difference between appeals to the interaction or intimacy of nature and

culture. When divisions are made between nature and the human, social world, discursive practice is almost invariably placed on "our" side of the distinction (whereas human anatomy and physiology are mostly conceded to nature). The attainment (both in human evolution and in individual ontogeny) of a physical capacity for speech and hearing (including, presumably, the relevant patterns of neural development) perhaps belongs within biology, but initiation into extant human languages and the cultural patterns they embody is reserved for social and cultural study. Yet such divisions clearly will not do. Theorists of development and evolution nowadays recognize that development is not a self-contained process within an organism, but instead involves characteristic patterns of interaction with its environment; moreover, such developmental patterns are integral to evolution. In this theoretical context, it would be difficult not to acknowledge that the pervasive presence of human speech and written symbols are among the most pervasive and highly influential features of the environments in which human biological development normally occurs. Indeed, the continuing reproduction of natural languages is perhaps the most striking example of what biologists call "niche construction", the ways in which organisms make the relevant environmental circumstances shaping their own development and evolution.[29]

Breaking down the boundaries between the social and the biological may nevertheless seem to resuscitate the specter of biological determinism, or at least of the biological subsumption of social inquiry. Yet such worries depend upon a narrowly reductive conception of biology, which would identify the biological domain with changes in gene frequencies, molecular cell biology, and organismic physiology. Ironically, it is precisely the spectacular successes of molecular genomics that have most extensively challenged (I am inclined to say "demolished") any such narrow conception of the biological domain.[30] I therefore conclude by suggesting that practice theory conceived more adequately in this respect does indeed preserve the integrity of the social sciences, not as a bulwark against reductive appropriation by biological interlopers, however, but instead as an ineliminably rich aspect of a more adequate human biology.

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[29]On "niche construction", see [Laland *et al.*, 2001]. For more general discussion of the new perspectives in evolutionary and developmental biology reflected in this paragraph, see [Oyama, Griffiths and Gray, 2001].

[30][Dupre, 2004] provides a good introduction to the role of molecular genomics in undermining reductive, gene-centered conceptions of biology.

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NATURALISM WITHOUT FEARS

Paul Roth

> Reflection on the method of science has become increasingly thinner
> since Kant. If there's any upshot of that part of modern philosophy,
> it's that the scientists didn't have a secret. There isn't something there
> that's either effable or ineffable. To understand how they do what they
> do is pretty much like understanding how any other bunch of skilled
> craftsmen do what they do. Kuhn's reduction of philosophy of science
> to sociology doesn't point to an *ineffable* secret of success; it leaves us
> without the notion of the secret of success. (Richard Rorty[1])

A concern to understand why the sciences succeed where they do and as well
as they do has typically prompted the *philosophical* study of the sciences. What
makes (or was thought to make) the study philosophical involves the level of gen-
erality at which the presumed secrets of scientific success lay. Philosophical natu-
ralists, however, study science not because they imagine that the sciences possess
insight other putative sources of knowledge do not. Rather, naturalists hypothe-
size, no forms of inquiry apart from those which the sciences provide hold any
comparable promise of being successful guides to acquiring any knowledge worthy
of the name. Philosophers become naturalists once convinced that the explanation
of scientific success does not lie in some set of factors which themselves cannot be
accessed, studied, and explained by these sciences.

Since the standards of science themselves fall within the purview of what the
sciences examine, philosophical naturalism locates all putatively distinctive philo-
sophical (e.g., normative) issues as continuous with and part of what the sciences
study. The sciences in turn have no further justification for their ways of pro-
ceeding other than what account they provide of their sources and methods.[2]

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[1] Richard Rorty, "Reply to Dreyfus and Taylor", *Review of Metaphysics*, 34, September 1980, 55.

[2] What counts as a "recognized science' proves to be historically contested and contingent. But that creates no special problem for naturalism as conceived and elaborated in this essay. Since the various sciences critique and monitor their own normative commitments, one result has to be that the disciplines of which the term 'science' may be properly predicated will alter as theoretical and related justificatory commitments do. As I indicate below, one must view the suggestion that the sciences can only be descriptive and not prescriptive as disingenuous, since it presupposes a notion of science to which no one, once asked to make fully explicit what this notion implies, actually subscribes.

Such is Quine's "mutual containment" of epistemology within empirical psychology and empirical psychology within epistemology.[3] Insofar as science can provide an account of how it came to be,[4] it functions as an epistemology. Insofar as epistemology invokes no standards or procedures alien to scientific inquiry, it resides within science. Moreover, naturalism bases this refusal to honor any appeals to extra-scientific justification for the sciences on studies of the history and philosophy of science, albeit recognizing full well that standards change, and not always for reasons current science can explain. That what goes by the title of 'science' shifts need not trouble a naturalist just so long as what the title includes proves the best guide to success in explaining experience.[5]

Philosophy as a naturalist conceives of it shares with more conventional philosophical approaches a concern to conduct a type of meta-level examination of particular sciences. That is, a philosopher *qua* naturalist examines, systematizes, and generally seeks to make explicit the rules by which the first order endeavor proceeds, including those circumstances under which the rules of inquiry themselves might be modified. But a key difference between naturalists and others in formulating and articulating such matters arises from naturalism's commitment to the view that in doing this, philosophy has no special methods or resources other than those which belong to the sciences collectively examined. Normative recommendations regarding, e.g., justification, can only draw from studies of scientific practice.[6] Moreover, each metascience can in its turn be made an object of study

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[3]One can only regard as deeply if unintentionally ironic those who attribute the rise in interest in naturalism to Quine's landmark essay, "Epistemology Naturalized", (in *Ontological Relativity and Other Essays*, New York: Columbia University Press, 1969) inasmuch as the main morals urged there by Quine are almost universally rejected, including those who profess to be naturalists. For a discussion of this and a defense of Quine's position along lines outlined here, see my "The Epistemology of 'Epistemology Naturalized'", *Dialectica*, 53: 87-109, 1999.

[4]The phrase 'came to be' means to capture both how a theory evolves from some earlier stage, and how it comes to be accepted as correct.

[5]I will not trouble here to try to delineate exactly how to distinguish what separate naturalism and pragmatism. My colleague Ellen Suckiel pointed out to me that, with respect to science, pragmatists tend to be naturalists, and vice versa. However, the two might also diverge; her apposite example involved religious belief. A pragmatist could well find a justification for religious belief; a naturalist would be less likely to do so, barring some at present unknown scientific advantage to, e.g., appeals to intelligent design. Quine suggests that a naturalist, but not necessarily a pragmatist, could take an interest in questions regarding the unity of science. A pragmatist would not have any clear reason to trouble about this question, but a naturalist could find reason to pursue questions of unity (methodological or ontological) as questions within science. See Quine's remarks on this point in, "Naturalism, or Living Within One's Means", *Dialectica*, 49: 251–61, 1995. This suggests that one distinguishing feature would be that naturalists use scientific standards (however broadly the term 'science' might be understood) as their most general framework for determining the relevance of and the means for answering *all* questions, while pragmatists do not endorse scientific standards as those holding final or most general relevance.

[6]I will comment more on this below. However, *qua* naturalist, norms must be tied to the practices of the sciences, broadly understood. Norms, of course, may be thought to have other sources — divine texts, revelation, or the *a priori* (among many others). But for purposes of this essay, the pronouncements of seers with regard to such realms will be kept separate from and not considered under the rubric of naturalism. The suggestion that scientific activity somehow proscribes or precludes consideration of ends just is obviously false. For the doing of science

and analysis, so there exists no final resting place, no *summa scientia*.

The term 'nature' here connotes the world as our sciences collectively picture it. Naturalism situates the study of humans, in all their aspects, as of a piece with those methods and theories used to investigate other objects in nature.[7] This naturalizing approach was considered less plausible when what counted as science seemed inadequate to the task of fully accounting for creatures like us, enculturated beings capable, *inter alia*, of creating both systems of meaning as well as complex theories of the world. The understanding-explanation divide receives its basic motivation from the thought that explanation requires laws — causal or at least correlational regularities—while social life is marked by localities of reasoning and meaning which do not generalize cross-culturally over time in the requisite ways (for a genuine scientific explanation).[8] Naturalism so construed denies that human beings *qua* knowledge producing creatures constitute a *sui generis* phenomenon, studiable only by methods uniquely suited to and tailored for conceptualizing creatures.[9]

Thus, the demise of positivism as a philosophy of science, and so by implication as a philosophy of social science, does not preordain the rise of naturalism to philosophical prominence in its stead. For the demise of positivism can just as easily be read as the vindication of interpretivism construed as a form of meaning realism. In this respect, naturalists stand accused of ignoring just those aspects

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requires acceptance of the norms of inquiry. Surely some argument is owed if the position of the opponents of naturalism rests on the assertion that reflection on these norms constitutes, *ipso facto*, a non-scientific activity. Debates about theory choice, proof, and evidence occur within science, concern normative issues, and belong to the ongoing discussion of the nature and practice of science. Those who maintain that the sciences provide only "descriptions" owe a characterization of science legitimating this view.

[7]To cite but two examples which manifest a concern for this linkage, and its overwhelming importance for an understanding of what naturalism may be, see David Thomas's *Naturalism and Social Science* (Cambridge UP, 1979) and Harold Kincaid's *Philosophical Foundations of the Social Sciences* (Cambridge University Press, 1996). Although both claim to defend an approach to the social sciences informed by philosophical naturalism, each devotes almost no space to explicating the notion of naturalism. Rather, the efforts lie in providing an account of science, for both reasonably seem to fear, without a well-delineated notion of science, there exists nothing that marks off naturalism from any other approach. But this ties naturalism too closely to the vicissitudes of efforts to delineate what to count as science. Better, I suggest, to cease to worry about a positive characterization of science and indicate what sort of explanatory moves naturalism excludes however science comes to be defined or constituted.

[8]For a survey of some history of this divide, and reasons for now considering it passé, see my "Beyond Understanding", in *The Blackwell Guide to the Philosophy of the Social Sciences*, ed. Stephen P. Turner and Paul A. Roth, Blackwell Publishing Ltd., 2003, 311–33. My argument for the current irrelevance of the explanation-understanding divide turns on the claim that inasmuch as disciplines such as history which countenance reasons as causes no longer need be excluded from what counts as science, the rationale for the distinction disappears. The dualism no longer serves any purpose, e.g., the need to account for people as creatures who act for reasons.

[9]Indeed, as Ian Hacking argues, a particular case for slighting the explanation-understanding divide from a Foucauldian perspective would insists that people both create and come to inhabit categories which allow for their manipulation, medication, and modeling of their behavior. See in particular his, "The looping effects of human kinds", in *Causal Cognition*, ed. D. Sperber, D. Premack, and A.J. Premack, Oxford: Clarendon Press, 1995, 351–383.

of the social which undid the positivists' efforts to provide general templates for explanation and demarcation criteria for determining which disciplines offered legitimate modes of scientific understanding. Brian Fay put the issue succinctly some time back.

> many philosophers of the social sciences who have rejected naturalism have not done so because they saw the natural sciences through positivist lenses. (Think of Schutz, Winch, Taylor, von Wright, Gadamer, Habermas, MacIntyre, Harré and Secord, Lévi-Strauss, and Putnam, to name just a few anti-naturalists: none of them are positivists.) Instead, they have rejected naturalism because there is not enough in the natural sciences that is helpful in dealing with the essentially historical, culturally defined, meaningful, mental, and rational character of human phenomena.[10]

However, Fay's characterization presumes that 'science' must mean just and only 'natural science', i.e., inquiry which much exclude the 'meaningful' for some reason. But unless history and related disciplines have been denied membership in the club of science for some now unspecified reason, no *a priori* argument excludes the investigation of meaningful behavior from the realm of what can count as science. As argued below, naturalism need make no distinction between sciences hard and soft, or even demarcate timelessly what science is. What science is is something which naturalists study.

In this respect, naturalism can best be delineated by contrasting it with what it presently *excludes* for purposes of explanation (e.g., supernaturalism — views that the natural world requires for its explanation something not found among its objects and the processes governing their interaction (God, the synthetic *a priori*, etc.), or foundationalism — views that the world must be explained by certainties, including certainties about the nature of ordinary experience, not explicable in turn by the sciences themselves). The lack of demarcation criteria proves to be a strength, not a weakness of the position. For it relieves the naturalist of the futile attempt to specify, in advance of what experience reveals, what must be, must remain, or cannot become a science. Any demand for a prior specification of normative framework proves to be no more than a demand that a naturalist not be a naturalist. But why accept that?

Yet the liberation of naturalism from any need to specify what science, timelessly imagined, is brings with it a threat to the doctrine as well. For naturalism has enjoyed increased philosophical favor just at the historical point where the prospects of a purely philosophical delineation of science appear highly unlikely. Understanding this seemingly ironic outcome proves critical, or so I shall maintain, to appreciating what naturalism offers and why so few who profess to be naturalists practice in accord with it.

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[10]Brian Fay, review of *Naturalism as a Philosophy of Social Science*, in *Philosophy of the Social Sciences*, 14: 542, 1984.

By way of approaching and unpacking the vexed relation between philosophical naturalism and the notion of science, I begin with the question of how philosophical naturalism can be distinguished from more conventional or traditional philosophical approaches to the sciences. Survey the usual suspects collected and arraigned in a philosophical lineup for purposes of providing extra-scientific explanation — analytic or necessary truths, norms, self-evident truths, etc.[11] A point to note involves the fact that there exists no physics of the right or the good, the logic of (Goodmanian) projection remains a riddle, and consequently, widespread philosophical practice notwithstanding, there exists no received account of how the notions on offer must interact as elements of explanation.[12] Authors defending antinaturalist approaches appear much more confident in particular alleged certainties than in any theory by which to account for them. In this regard, the issue which separates naturalists and those who would oppose them concerns less, e.g., the naturalization of norms than the question of how to specify just what supposedly needs incorporation into a theory of the world, i.e., a "naturalizing" of this or that.[13] One cannot say that this or that — norms or whatnot — can (or cannot) be naturalized until given some reasonable specification of what supposedly needs "naturalizing."

In what follows, I sketch a working notion of philosophical naturalism and offer some justification for it. This includes a brief historical characterization of naturalism, including some (unapologetically whiggish) historical speculations on how it came to be. I then turn to questions of how this serves to distinguish naturalism in the social sciences from two possible anti-naturalist alternatives, a formalist account (of which the best known variant is positivism) and a meaning realist account (some forms of which I term below 'interpretivism'). A review the reasons thought to support one or the other of these anti-naturalist accounts will remind

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[11] A clear and philosophically unapologetic approach to thinking about epistemological issues which is fundamentally at odds with the approach advocated in this essay can be found in Richard Feldman, *Epistemology*, Upper Saddle River, NJ: Prentice-Hall, 2003.

[12] See, in this regard, essays in *Rethinking Intuition*, ed. Michael R. DePaul and William Ramsey, New York: Rowman & Littlefield, 1998.

[13] For example, Kim's widely cited essay relies on just the distinction that whatever science is (and Kim provides no such characterization), it can countenance only causal explanations. He is not alone in this. Jaegwon Kim, "What is 'Naturalized Epistemology'?", in *Philosophical Perspectives*, ed. J. Tomberlin, Atascadero, CA: Ridgeview, 1988. Kim imagines that whatever norms are, some "special" justification for them must be forthcoming, though no argument hints at a reason for giving norms a non-natural status, or even to say in what forms the imagined norms exercise their magic. For a development of this criticism, see Roth, "The Epistemology of 'Epistemology Naturalized'", op. cit. Relatedly, Barry Stroud asserts that naturalism remain caught in a "basic dilemma" because it cannot deny that there exist "psychological facts" regarding beliefs, intentions, and the like, on the one hand, but on the hand, Stroud finds it unclear how these "psychological facts" readily fit into the "restricted conception" (47) of the world to which naturalists subscribe. Many problems suggest themselves here, not the least of which concerns how, on any theory whatsoever, one knows what to count as an "adequate" account of "psychological facts". It is not impossible that Stroud is right. But one would first need to know exactly what things he finds missing before rushing to conclude that some view or other does or does not account for them. Barry Stroud, "The Charms of Naturalism", *Proceedings and Addresses of the American Philosophical Association*, 70: 43–55, November 1996.

readers of why these positions were found wanting. Finally, I sketch some varieties of naturalism on the current social scientific scene and rehearse what they imply for the philosophy and the practice of social science.

Nothing in this essay will serve as direct argument to convince someone not now inclined to naturalism to adopt it. Among other reasons for this, some will doubtless judge my negative conclusions about the feasibility or potential fruitfulness of pursing more traditional philosophical methods and agendas at best premature. *De gustibus non est disputandum.* My approach also implies that while many might claim to be naturalists, few philosophers actually qualify as such in practice.

1   WHAT NATURALISM IS

A working characterization of naturalism needs to be formulated. Despite the trumpeted "naturalists return",[14] the very pervasiveness of the term on the current philosophical scene gives rise to fears that the term has become too polyvocal to be useful. Indeed, its pervasive use only lends credence to the suspicion that the term may be vacuous.

Worries about vacuity tie in part, I suspect, to an absence of a canonical account of philosophical naturalism. Historically, the term connotes more to a loose school than to a specific doctrine.[15]   A more substantive and genuine but much

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[14]Philip Kitcher, "The Naturalists Return", *The Philosophical Review*, 101: 53–114, January 1992. Kitcher identifies anti-naturalism with the animus of Frege and those who followed him towards any appeal to psychological or contingent scientific factors. Frege, focused on mathematics and structures presumed to be universal and shared, could envision no role for the empirical in this account. However, as challenges mounted to attempts to stipulate a principled divide between what requires appeals to experience for verification and beliefs that can be held true come what may, Fregean reasons for precluding the relevance of naturalism appeared less compelling. Michael Friedman's case for a return to anti-naturalism reverts to Fregean themes, but at yet higher levels of mathematical abstraction; naturalism cannot be what philosophy should become, because pure mathematics not only "floats free" of the tribunal of experience, but actually serves as a constitutive condition for constituting any such tribunal. See, for example, Michael Friedman, "Philosophical Naturalism", *Proceedings and Addresses of the American Philosophical Association*, 71: 7–21, 1997, especially p. 14. However, it does seem to be a consequence of Friedman's position that all philosophy comes to are principles at such a level of abstraction, and the only ones that appear to fill that bill belong to extremely abstract and abstruse areas of mathematics. Moreover, a key move in this essay by Friedman in criticism of Quine and defense of Carnap, viz., that Quine's contention in "Two Dogmas" that the dogmas are, at root, identical, is to claim that this rests on Quine's holism. He asserts this without argument, and in this he is wrong. Reductionism would provide just another species analytic statements, inasmuch as the 'reduced' term and the reducing ones are fully equivalent. Holism enters Quine's discussion as a plausible explanation for why it proves so hard to specify which statements are analytic. Holism is *not* given as an argument against analyticity.

[15]In his Presidential Address to the APA a half century ago, Ernest Nagel articulates an general framework for conceiving of naturalism to which the present essay chimes. Ernest Nagel, "Naturalism Reconsidered", *Proceedings and Addresses of the American Philosophical Association*, 28: 5–17, 1954-55. Nagel remarks that he uses the term "partly because of its historical associations, and partly because it is a reminder that the doctrines for which it is a name are neither new nor untried". (7) He goes on to add, as I also emphasize, the "if naturalism is true, irreducible variety and logical contingency are fundamental traits of the world we actually

less acknowledged (or at least discussed) worry here arises from the fact that any characterization of naturalism proves unilluminating because it requires a specification of the notion of science invoked when stipulating the doctrine. But, post-positivism, what principled characterization of the notion of science can be provided?

In short, the dividing line between what naturalists embrace and what they exclude seemed clearer when thinkers had confidence that the "real" sciences and their related methods could be formally demarcated from the proposals of pretenders. Insuperable appearing challenges to demarcation stem both from the failure to discern historically any necessary differentia between the true sciences and mere pretenders, on the one hand, and, on the other hand, work by e.g., feminists and sociologists of science which challenges any proposed separation between scientific theorizing and social convention.[16] Failing this ability to demarcate, what does naturalism then connote? In this regard, some attention must be paid to how the notion of science has itself evolved post-positivism in order to appreciate what one endorses if one declares for naturalism.

How then to say what science is? This question underlines the concern that naturalism fails to mark out any special ontological or methodological realm because no philosophically principled lines can be drawn between scientific approaches and others, and so no ontological line between the objects of science and others. Erstwhile naturalists might well fear that their doctrine proves empty either because the social has so expanded as to include "the realm of science" within its ambit of explanation, and not vice versa. If the sciences form only a motley, so much the worse for any doctrine which seemingly relies by definition on the sort of sharp delineation of science which current accounts fail to provide.

Ironically, this very lack of a philosophically principled demarcation of science from other forms of inquiry does *not* mark the passing of or threaten vacuity

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inhabit". (10)

[16]Examples here are legion. Representative would be, e.g., Helen Longino's work and work in the sociology of science by thinkers such as Barry Barnes, David Bloor, and Steve Woolgar. An important difference in this group concerns the fact that most alternatives to traditional philosophical accounts of scientific rationality reject what they see as 'traditional' about such views or what they view as excessively 'philosophical' about such views. In both cases, critics emphasize the implausibility of excluding social and cultural factors from any explanation why one theory prevails over another. These critics remain well within the tradition insofar as they hold that broadened account of influencing factors suffices to explain theory preference among scientists. For discussions of how the traditional view lives on in the work of its erstwhile critics, see my "Feminism and Naturalism: If Asked for Theories, Just Say 'No'" in *Feminist Interpretations of W.V. Quine*, ed. L. Hankinson Nelson and J. Nelson, University Park, PA: The Pennsylvania State University Press, 269–305, 2003, and "Will the Real Scientists Please Stand Up? Dead Ends and Live Issues in the Explanation of Scientific Knowledge", *Studies in History and Philosophy of Science*, 27: 813–838, 1996. The exception here would be Woolgar's work, which recognizes clearly the irony involved in sociological claims to provide a scientific explanation of scientific practice which relies on a "debunking" of this practice as anchored in norms of rationality. An adumbrated discussion of these issues can be found in Steve Woolgar, *Science: the very idea*, London: Tavistock, 1988. See especially references therein, especially to Woolgar's own earlier work. For a review of this debate, see John Zammito, *A Nice Derangement Epistems* (Chicago: University of Chicago Press, 2004).

to naturalism but rather has made possible its resurgence and re-established its relevance. For the wresting free of the concept of science from formalist shackles to which it had become, though much of the 20th century, bound makes possible a "naturalists return" by allowing the notion of science to range over the variety of ways humans systematically explore and account for the world as they find it — from physics to history. Liberalizing what to count as science in this way removes the need for individious distinctions between the natural and the social sciences. Relatedly, as I detail below, criticisms of the notion of "objective meaning" and the consequences of appreciating the implications of the indeterminacy of translation dissipate fears that a naturalistic social science cannot avail itself of the notion of meaningful behavior. Granted, the account offered in this essay leaves open to change what exactly to count as science. But better to acknowledge that this notion appears fated to remain contested than to pretend to more determinate knowledge than, in fact, we do or can expect to possess.[17]

Naturalism, I have claimed, can be best clarified by contrasting it with possible alternatives. For purposes of exposition, Russell's theory of descriptions may be taken as paradigmatic of a non-naturalistic theory, although I would also echo Frank Ramsey famous *mot* and take it also as a paradigm of (non-naturalistic) philosophy. In this regard, Russell's analysis of "The present King of France is bald" or "George IV wished to know whether Scott was the author of *Waverley*" proved paradigmatic in two distinct senses. One is as problem-solving model. It greatly simplified the assumptions needed to analyze some standard "hard cases".

Yet, and more importantly for present purposes, Russell's analysis belongs to a philosophical theory of meaning of which the theory of descriptions served as but a part. Meaning here becomes a function of a language possessing a particular logical structure, a structure proper analysis reveals. This theory of meaning itself belongs to *no* natural science, but rather presupposes problematic philosophical views regarding "knowledge by acquaintance" and "knowledge by description". The Russellian paradigm reminds us of how a wrong-headed philosophical theory may lurk just below the surface of elegant and seemingly metaphysically pristine formal analyses.[18]

My aim here is not to offer any general criteria for designating theories as philosophical, but only to note some features that can make them ones. Failure to solve conceptual puzzles represents a difficulty, not disconfirmation. In addition, as is notoriously the case in the various formulations of the principle of verifiability, the demarcation criteria kept coming out wrong. Various schemas invariably ex-

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[17]Those who worry that this approach opens the door to creationism, alchemy, and other questionable claimants to the title of science do so needlessly. By emphasizing more the pragmatic outcomes of theories, one can make a stronger argument than those offered by formalist criteria for favoring one theory over another. Scientific methods can be mimicked; research outcomes are there or not for the world to see.

[18]Another important instance of how a logical analysis may overlie a contentious philosophical theory is the verifiability theory of meaning. It merits the title of a "philosophical theory" just because it too purported to explain, without the aid of science, why science works well when it works well.

cluded from the realm of meaningful statements in science some sentences even positivists wanted to keep. But the sundry shortcomings only whetted their philosophical appetite, inasmuch as the theories were held for reasons experiments could not touch, e.g., assumptions about the possible sources of human knowledge, the deep structure of natural language, and the requirements of cognitive significance.

It is important to remember what separates the approach of a Russell and especially a Carnap from those of their empiricist and rationalist predecessors. It is their positive proposal to actually reconstruct the link between existing scientific theories and their empirical base. Somewhat ironically, what we owe to the decades of intensive work especially by Carnap is a deep appreciation of how resistant scientific theorizing is to this specification of its inferential relation to evidence.

This explicitly constructive aspect of the logical positivist project comes finally to define what empiricism is in the 20th century.[19] Rational reconstruction would have established the objectivity and rationality of scientific knowledge to anyone's satisfaction. "Rational reconstruction", Carnap says, attempts "for the first time, the actual formulation of a conceptual system of the indicated sort", (ibid.) i.e., our system of knowledge. Reconstruction would be proof positive of long-standing empiricist claims regarding what the "deconstruction" of empirical knowledge *must* yield. The hallmark of the positivist philosophical theories of knowledge is just this reconstructive claim, of tracing a logical path from "protocol sentences" to those of theory.

This defining characteristic of logical positivist epistemological theories is doubly philosophical. On the one hand, it is not tested via experiment; reconstruction is just an exercise in logical imagination. On the other hand, reconstruction provides the justificatory basis, in the best understood sense of that term — a formal logical derivation — of theoretical claims. Logical reconstruction is then a paradigm *philosophical* claim, a "first philosophy" that is prior to scientific knowledge.

In the above scouted sense of "philosophical theorizing", naturalism is not a philosophical theory of knowledge. Some, to be sure, have tried to make it so. Naturalism asserts a normative and methodological continuity between epistemological and scientific inquiry. That is, the techniques endemic to the former are only a subset of the historically received and contingently held norms and methods of the latter.

What counts as a scientific method for naturalists is not itself limited to or defined by one particular science, or driven by a prior philosophical characterization of such. For Quine, as for American naturalists historically, the methods of science include the full panoply of procedures employed in fact-driven research programs in any area of inquiry. As John Herman Randall puts it:

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[19] "I had realized, on the one hand, the fundamental importance of mathematics for the formation of a system of knowledge and, on the other hand, its purely logical, formal character to which it owes its independence from the contingencies of the real world. These insights formed the basis of my book...This orientation is sometimes called "logical empiricism" (or "logical positivism"), in order to indicate the two components". Rudolf Carnap, *The Logical Structure of the World*, Berkeley, CA: University of California Press, vi, 1967.

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> The "new" or "contemporary" naturalism . . . stands in fundamental opposition not only to all forms of supernaturalism, but also to all types of reductionist thinking which up to this generation often arrogated to itself the adjective "naturalistic",. . . [Naturalists agree] that the richness and variety of natural phenomena and human experience cannot be explained away and "reduced" to something else. The world is not really "nothing but" something other than it appears to be; it is what it is, in all its manifold variety, with all its distinctive kinds of activity.[20]

Quinean naturalism, in particular, demands no strict demarcation criteria of what to count as science or scientific.[21] Nor are there any philosophical *cum* ontological requirements regarding the necessary building blocks of knowledge. Epistemology is "contained in" empirical psychology only in the sense, and to the extent, that ontogeny recapitulates phylogeny.

In a like manner, those who perceive naturalism as handicapped by some form of the is/ought distinction likewise mistake what a naturalist's conception of science involves. Why assume that an account of scientific method excludes or precludes evaluations of standards of scientific justification any more than working within a system of logic precludes or excludes consideration of rules for the system? Systems of science and systems of logic alike are constructions whose operant standards are chosen in light of certain ends and purposes — namely, those of their makers and users.

Science and logic are conceived from the outset as systems that stand in a dynamic relation to their rules, rules which are in turn chosen for and adjusted to certain ends. Reflexive adjustment of means and ends is just part of what it is to have and maintain such a system.

Quine's naturalism extends to rules of logic. In a 1936 essay which prefigures many of his key philosophical themes, "Truth by Convention", Quine disparages an important attempt to cash out the view of these truths as analytic by appeal to the notion of mathematical or logical convention. There is a fundamental difference, he argues, between rules codified in light of practices, and practices that follow clear rules. Only in the latter case are conventions explanatory. That is, conformity to a convention explains behavior when the convention is specified in advance of the behavior. But when the convention itself is only formulatable *subsequent* to a particular practice, the convention then does not explain what one observes.

> When we first agree to understand 'Cambridge' as referring to Cambridge in England, failing a suffix to the contrary, and then discourse accordingly, the role of linguistic convention is intelligible; but when a convention is incapable of being communicated until after its adoption,

[20]John Herman Randall, "The Nature of Naturalism", in *Naturalism and the Human Spirit*, ed. by Y. K. Krikorian, New York: Columbia University Press, 1944, 361.
[21]W.V. Quine, "Naturalism; Or, Living Within One's Means", *Dialectica*, 49: 251–61, 1995, 252.

its role is not so clear. In dropping the attributes of deliberateness and explicitness from the notion of linguistic convention we risk depriving the latter of any explanatory force and reducing it to an idle label. We may wonder what one adds to the bare statement that the truths of logic and mathematics are a priori, or to the still barer behavioristic statement that they are firmly accepted, when he characterizes them as true by convention in such a sense.[22]

Where the convention is beholden to some prior practice, citing it adds nothing to "bare behavioristic statement" for purposes of explanation.

Science and logic are then constructed systems and their makers choose the norms constraining them in light of their purposes. In light of this fact, why grant any measure of credence to claims that working with and within such system precludes assessing, emending, or amending the previously chosen norms?

Over 50 years ago, Abraham Edel mounted a defense of naturalism in ethics germane to this discussion of naturalism as a source of normative insight for the sciences. He there nicely articulates just why naturalism is reflexive regarding its normative commitments. In the quote that follows, imaginatively replace each use of 'ethics' or cognate terms with the appropriate form of the term 'science.'

> The whole articulation of a morality within a society under given conditions, the problems of change and adjustment within it, require constant valuational activity. We find our commitments as what we are committed to in the specific lines of choice and directions of striving in which we are engaged. Even the major permanent ends we may thus elicit on analysis . . . do not become the objects of isolated independent selection. Their evaluation rests on the whole network of choices and the kind and quality of life to which they commit us.
>
> . . . Mr. Murphy seems to me to pose the question almost as if an ethical theory must somehow equip a hypothetical man who holds no values to choose between conflicting values. If he means to eliminate all reference to an existent value-pattern of the self as already settling the moral problem, then he poses an impossible task. The question "What values should I choose if I had no values?" is meaningful only if it asks what other who had values would recommend for a person in my position. All justification is in a matrix of existent values. Scientific method is applicable to values in so far as it provides a way of identifying one's existent values, testing them, and refining or revising them in choice.[23]

We have ends important to us, and we have systems which, we hope, will abet us in achieving those ends. If the ends seem to require rules we find overly restrictive,

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[22]W.V. Quine, *Ways of Paradox*, 2nd Ed., rev., Cambridge, MA: Harvard U.P., 1976, 99.

[23]Abraham Edel, "In Naturalism Arbitrary?", *Journal of Philosophy*, 43: 141–52, 1946, 146-7. Again: "A naturalistic ethics recognizes frankly the primacy of human striving or goal-seeking as the matrix of its inquiry. In every moral choice there is a reference to some values which act as standards of judgment." 144.

we can alter or drop the goal; if a rule does not function well relative to the end in view, we can change the rule. This is as true as science as for ethics.[24]

Questions of what ends we ought to choose, in abstraction from lived experience and human history, are meaningless. For such questions cannot apply to us, or anyone known to us. Barring a satisfactory account of just how norms of justification are somehow summoned from realms beyond time and history, there is then no good reason to believe that a naturalistic perspective impedes epistemology's normative aims.

So-called naturalist positions that promise more by way of normative edification than does Quine, Philip Kitcher or Alvin Goldman come to mind here, invariably turn out to fail to justify such normative claims naturalistically or, I should add, to justify them at all. As Miriam Solomon quite properly notes with regard to Kitcher's pseudo-naturalism — a position she dubs "Legend Naturalism" – his "naturalism does no work — no data or theories from psychology or sociology shape the epistemic account — the naturalism is just window-dressing for a previously and independently developed account of scientific rationality".[25] Much the same is true, I have elsewhere argued, regarding Goldman.

In this light I propose to examine two common but in fact incompatible criticisms of naturalism. The first insists that the characterization proves too vague to be of any use.[26] Van Fraassen, for example, remarks that "To identify what naturalism is . . . I have found nigh-impossible".[27] Second maintains that naturalism proves too narrow or restrictive; its link to scientific methods supposedly precludes naturalism from fulfilling any of the normative roles to which philosophy aspires. This alleged problem may be understood as just a version of the Humean is/ought: science describes, norms prescribe, hence the latter cannot be derived from the former. Since naturalism limits itself what science provides, it cannot

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[24]In this respect, I believe, Quine would echo Goodman's "justification of deduction". "Principles of deductive inference are justified by their conformity with accepted deductive practices. Their validity depends upon accordance with the particular deductive inference we actually make and sanction. If a rule yields unacceptable inferences, we drop it as invalid. Justification of general rules thus derives from judgments rejecting or accepting particular deductive inferences . . . A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend. This process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either". Nelson Goodman, "The New Riddle of Induction", in *Fact, Fiction, and Forecast*, 3rd Ed., Indianapolis, IN: Hackett, 1979, 63-4.

[25]Miriam Solomon, "Legend Naturalism and Scientific Progress", *Studies in History and Philosophy of Science*, 26: 205–18, 1995, 207. Solomon thinks this is the case for Quine as well, and here I disagree.

[26]Alexander Paseau, although content to use the term 'naturalism' in his title, quickly alerts his readers that "in general, the term 'naturalism' is overworked in contemporary philosophy. Vague orientation aside, most philosophical naturalisms have little in comment with one another". Alexander Paseau, "Naturalism in Mathematics and the Authority of Philosophy", *British Journal for the Philosophy of Science*, 56: 377–96, 2005, 377 fn.1. See also Bas van Fraassen "Against Naturalized Epistemology", in *On Quine*, ed. P. Leonardi and M. Santambrogio, NY: Cambridge U.P., 1995.

[27]Bas van Fraassen, "Science, Materialism, and False Consciousness", in *Warrant in Contemporary Epistemology*, ed. J. Kvanvig, Latham, MD: Rowman & Littlefield, 1996, 172.

derive from this a way to philosophically prescribe.

But just how vague is the notion of naturalism? No more vague, I suggest, than our ability to catalog the methods of science. Naturalism, moreover, does not yoke what counts as science to some philosophical characterization. It is ironic, then, to find philosophers such as van Fraassen making continued references to "science", as if they knew exactly what that means, and yet complaining all the while about the vagueness of naturalism. So long as that proves workable, naturalism is as well. I have elsewhere urge a liberal Quinean line in how to construe the notion of science, but that case need not be rehearsed here.

The second, flowing from naturalism's embrace of science and its rejection of claims to other, non-scientific forms of knowledge, invokes the hoary descriptive-prescriptive distinction. The objection here imagines that sciences, however conceived, can only describe the world. Philosophers, intent on a prescriptive (or, as the current favored term of art has it, the 'normative') investigation — not what is the case, but what (ethically, epistemically) ought to be the case — regard scientific investigations so conceived as incapable of providing philosophic insight, so conceived. While science may tell us what is the case, only philosophy, in its various guises, can pronounce on what (rationally) ought to be the case.

Yet why this repeated injunction of the descriptive-prescriptive distinction, as if someone somewhere had established a clear demarcation of these notions and, in addition, adduced arguments that 'real' science partakes of just one and never the other? Given the relentless blurring of the distinction between theory and observation, itself a species of the descriptive-prescriptive dualism, I profess extreme skepticism that any good case could be made at this point in time either for this distinction in general, or for an account of science which has 'real' science doing one and not the other.[28]

Moreover, the two criticisms — the charge of vagueness, on the one hand, and the insistence on the descriptive-prescriptive distinction within science, on the other hand — appear incompatible. For insofar as what happens to be science can be specified sufficiently sharply so that only descriptive accounts (under some suitably acceptable notion of 'descriptive') qualify, then naturalism cannot be belabored as vague, since its vagueness could flow only from the account of science to which it links. Yet if the going account of science cannot support the charge that 'real' science must foreswear partaking in prescriptive notions, then the alleged hostility of naturalism to the normative cannot be sustained. For embracing scientific methods would not exclude "by definition" examinations of normative considerations. Put another way, consideration of the methods of science includes, I assume, those standards with which scientists work. Limiting examination to the announced results — the products, not the processes — represents an arbitrary limit not backed by any argument. Yet only by such an arbitrary limit do the norms of science themselves not count as a product of scientific inquiry and so open to scientific explanation and scrutiny.

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[28]In this regard, Richard Rudner's brief piece "The Scientist qua Scientist Makes Value Judgments" remains worth reading, *Philosophy of Science*, 20: 1–6, 1953.

It would be a mistake to construe the cut between naturalism and its philosophical Other as rooted in a metaphysics of objects or primary processes. For that would be to make an assumption a naturalist does not, viz., that one can in principle draw some line between what counts or does not as science. Naturalists need not (and, on my view, ought not) be in the business of prescribing in advance what can or cannot be part of the ontology or causal order. Rather, the critical point turns out to be one of a type of *explanatory unity*. Naturalists seek only explanations which fall within the causal and ontological orders as the sciences, broadly construed and contingently constituted, would have them.

In this regard, to claim that there exists, e.g., a normative realm (in logic, in ethics, or wherever) over and above the world science examines simply fails to add to our knowledge, unless one has some special notion of knowledge on offer. Were there to be had some non–stipulative knowledge of these other supposed realms of being, then all would be well. But absent some "physics of the normative", no one knows what ones know and how ones know it for these other realms of supposed knowledge.[29] It is not enough, that is, to stipulate what one takes to be true a priori; one would need to know what makes the *a priori* what it is. For present purposes, explanatory resources available to us now must be sought in the sciences, broadly conceived.

In sum, then, naturalism is not a philosophical theory in the previously specified pejorative sense of that term. It is empirical through and through, from its conception of logic to its conception of methods to what even to count as science. Naturalism so conceived is untainted by prior philosophical commitments to reduction or to a hierarchy of sciences. No area of belief stands aloof from alteration or emendation in light of experience. Even the preference for naturalism itself is evidence driven. Should some approaches other than those the sciences offer prove more efficacious in furthering our goals, the commitment to naturalism itself would then be jettisoned. There is no more vagueness to the notion of what naturalism is than there is to what the methods of the sciences themselves are. There is no more an obstacle to examining, emending, or excluding norms within a naturalistic approach than there is in any self-critical scientific approach. Which is to say, there is none at all.

2   NATURALISM AND SOCIAL SCIENCE

With respect to the social sciences, does naturalism represent just one more guise for rampant scientism or unrepentant reductionism? Scientism assumes that the definition of science limits it to the natural sciences; reductionism assumes that within the natural sciences all acceptable theoretical notions ultimately must find a place in terms provided by physics (or whatever the current candidate happens

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[29]Consider, in this regard, John Mackie's complaint that the "ethically real', should such exist, would constitute an ontologically strange realm. I find no advance in knowledge here by invoking notions of necessity where some corresponding explanation of why such necessities need obtain proves absent.

to be for the genuine *Über*-science). These conceptions of scientism and reductionism, in turn, generate two lines of resistance to naturalism in the social sciences. One line maintains that naturalism fails in the social sciences because given what science is, sciences of the social simply cannot reach the standards of explanation required for "real" science. Here the criticism assumes that what sciences are can be specified relative to formal criteria, and that given these criteria, the sciences of the social just do not meet the mark.[30]

A second line of resistance in fact grants to formalist conceptions of science the substance of their claim about the natural sciences, but maintain that the object of social science lies elsewhere. Proponents of a special status for the "human sciences" emphasize the centrality of the meaningfulness of actions — the intentionality of some of our behavior. In this regard, meaning is taken to be a non-naturalistic phenomenon — meaning is not a property of behavior as, e.g., color is of an object. Interpretive social science may, in this regard, appear to be a contrast to naturalistic social science, insofar as the former emphasizes the centrality of meaning, and meaning in turn is taken to be a non-naturalistic notion because tied to an intentional vocabulary which has no naturalistic, i.e., scientific, analogue.[31]

A closely related issue, touching on both formalist and interpretive preconceptions regarding the necessary features demarcating their areas of study from others, involves claims about standards of rationality. In its first incarnation, the debate pits formalist conceptions of scientific rationality against interpretivist claims re-

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[30]For a text which takes basically this view, see Alexander Rosenberg, *Philosophy of Social Science*, Boulder, CO: Westview, 1988, especially Ch. 1. Rosenberg, in Ch. 7, raises key moral issues that might arise in "committing social science", e.g., experiments on human subjects. However, to insist that this then establishes the prescriptive/descriptive divide in social science simply begs the question against naturalism in ethics. A very different path to a related conclusion — that the social sciences must satisfy certain formal constraints in order to count as science — can be found in Daniel Little, *Varieties of Social Explanation*, Boulder, CO: Westview, 1991. But whereas for Rosenberg the key formal element concerns the need for laws or law-like statements for explanation, Little emphasizes a particular analysis of causation as the necessary formal condition. Interestingly, they reach opposed conclusions on the question of whether the social sciences satisfy their respective formal desiderata.

[31]This view has origins in the very creation of the idea of a social science, e.g., the work of Max Weber. Insofar as positivism enshrines a certain formalist conception of science, interpretivists mount their opposition to positivism not because they challenge how positivists characterize natural science, but because they claim that this characterization excludes an essential element needed for social science —reference to meaning understood as a non-natural notion. For good historical surveys of the debate over the status and character of the human sciences vis-à-vis the natural sciences which takes for granted both the legitimacy of a formalist conception of science and the non-natural status of the notion of meaning, see K-O Apel, *Understanding and Explanation*, Cambridge, MA: MIT Press, 1984, especially Chs. 1-4. While I am deeply sympathetic to the issues Apel raises in his Appendix, "Is the Controversy between Explanation and Understanding Obsolete?", my own take on the nature and consequences of this obsolescence remain quite different from the analysis Apel offers. Other worthwhile accounts include J. Habermas, *On the logic of the Social Sciences*, Cambridge, MA: MIT Press, 1988, and G.H. von Wright, *Explanation and Understanding*, Ithaca, NY: Cornell U.P., 1971. An excellent overview of the full sweep of this general debate remains R. Bernstein, *The Restructuring of Social and Political Theory*, Philadelphia: The University of Pennsylvania Press, 1978.

garding what to count as rational or "logical" standards regarding the justification of beliefs.[32] The next incarnation of the *Rationalitätstreit* pits sociologists of science against philosophers of science with regard to who better explains theory change in science — sociologists by appealing to factors exogenous to scientific method formally conceived, or philosophers by appealing to some formal canon of scientific rationality.[33] Interestingly enough, the first incarnation turns out to be simply an artifact of the now untenable root beliefs held by the competing accounts — the existence of a single logic of science, on one side, the "idea of the social" as a conceptual reality subscribed to by the other. Once rationality itself becomes "naturalized", then the disputes between philosopher sand sociologists of science become tractable, at least in the sense of having common ground on which to settle the issues.[34]

I focus below on whether the lines of resistance to naturalism emanating from formalist or interpretivist preconceptions about the "human sciences" remain plausible. None of these issues, I claim — the efforts to provide a formalist demarcation of science and non-science, the conception of meaning as a non-natural yet objective phenomenon, the dispute about competing standards of rationality — constitutes a viable objection or obstacle to philosophical naturalism in (or out) of the social sciences.

In the context of philosophy of social science, the two contrasting positions to naturalism are standardly taken to be positivism and interpretivism. The former contrast turns on a view that what science is can be determined by logical form, and the question of form is not itself a matter of investigation by one or another science. Nor is this form a historically contingent matter, except in the philosophically irrelevant sense that it was over time that the proper form was discovered. 'Science,' on this view, presents no moving target; our challenge would simply be to discover what the proper form is.[35]

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[32]Canonical collections here include *Rationality*, ed. B. Wilson, Oxford: Blackwell, 1970, which examines the dispute as it arose within primarily the ambit of analytic philosophy (pre- and post-Wittgensteinian, on one reading of Wittgenstein). Peter Winch's essay, "Understanding a Primitive Society" (and included in the Wilson anthology) sparked this debate and remains a primary focus. I would include in this context the Popper-centered dispute which provides the focus for *The Positivist Dispute in German Sociology*, T. Adorno *et al.*, New York: Harper Torchbooks, 1976. Although the issues differ, the dispute remains centered on how to determine what counts as rational standards for justification. In addition, two collections which examine the rationality dispute in broader context are A. Giddens, *Positivism and Sociology*, London: Heinemann, 1974, and *Understanding and social inquiry*, ed. F. Dallmayr and T. McCarthy, Notre Dame, Ind. : University of Notre Dame Press, 1977.

[33]A generally good overview of this debate can be found in *Rationality and Relativism*, ed. M. Hollis and S. Lukes, Cambridge, MA: MIT Press, 1982. The essay by Barry Barnes and David Bloor therein provides a helpful, if typically polemical, overview of how the sociologists configure the debate here.

[34]See "Will the Real Scientists Please Stand Up?", op. cit..

[35]For classic expositions of the philosophy of social science in this mode, see Otto Neurath, *Foundations of the Social Sciences*, Chicago: The University of Chicago Press, 1944, or Richard S. Rudner, *Philosophy of Social Science*, Englewood Cliffs, NJ: Prentice-Hall, 1966. Many of the early anthologies on this subject, e.g. May Brodbeck's *Readings in the Philosophy of the Social Sciences*, reflect in their division of topics — laws, explanation, ontology — the assumption that

Much of what was outlined above with regard to criticisms of philosophical theories applies directly to positivism. But the failure of positivism as a philosophical theory needs to focus here on the particular criticisms internal to that theory, particular those scouted by Carl Hempel in his classic essay, "Problems and Changes in the Empiricist Criterion of Cognitive Significance" and Carnap's essay "Empiricism Semantics, and Ontology". In both these cases, one witnesses positivism ultimately going 'holistic", by which I mean that the question of what makes a statements verifiable (as Hempel stresses) or what makes a theory rationally preferable (as Carnap discusses) turns away from any simple consideration of the relation of statements and evidence and towards more global considerations regarding how theories constitute mutually supporting sets of statements not individually evaluable or how theory choice can be primarily motivated by pragmatic questions. There are no ultimate frameworks by which to decide a "best" theory.

It helps here to emphasize how these conclusions in support of a holist view of theories and pragmatic forces guiding theory choice emanate from writers such as Hempel and Carnap. For if one only discusses, e.g., Kuhn on incommensurability and revolutions or Quine on underdetermination or the analytic/synthetic distinction, the mistaken impression arises that all might be well with the sentence-by-sentence view were some one objection answered. But the Kuhnian and Quinean criticisms prove to be *symptomatic* of the same fundamental problem, viz., that a certain view of how language relates to the world never cashed out as promised. Analysis of the logical structure of the world was to make good on the longstanding empiricist promise that a chain of justified inference led or could be reconstructed from the evidence available to us *qua* embodied and reasoning beings to the highest reaches of the sciences which constitute part of what humans know. But not only was no such determinate chain of inference revealed, but also close inspection turn up compelling reasons to reconceive the entire theory-evidence/word-world relationship. Quine and Hempel offer no novel criticisms of notions fundamental to positivism. Each, rather, rehearses and details generally known failures within that program. The difference between Quine and Hempel in this regard lies not in their cataloging of fundamental shortfalls in the program, but in their imagined philosophical futures. Hempel enjoins his readers to press forward with the original program. Quine offers a specific counter-suggestion to the going dogmas on how to think about our beliefs and the evidence for them. Neither claims to have shown "in principle" why positivism fails, but only to have indicated the massive problems such a program faces.

Positivism was abandoned, and ironically the "reconsideration of logical positivism" stresses not verificationism but its (allegedly under- or unappreciated) ties to neo-Kantian projects. But whatever the interest or legitimacy in reading at least Carnap in this way, the reading serves only to underline that no serious

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philosophy of social science is just a spin-off of the philosophy of science. But this very division of topics supposes precisely what no longer can be assumed to be the case, viz., that something distinctive and general with regard to each of these topics marks out what a science is as opposed to another type of study.

effort appears on the current philosophical front to resuscitate the sort of theory-evidence relation imagined by the verifiability criterion of meaning. This, in turn, underwrote the hope that a purely formal or structural analysis of the science could be provided, an analysis which would provide as a direct result a demarcation criterion for the sciences. Absent this structural demarcation, then, the type of anti-naturalism represented by a neo-Kantian reading of the Carnapian project is not one which concerns me. For it remains unclear how this would intersect with worries about the "human sciences" which flowed from the early construal of the work.

Those interpretivists I have labeled "meaning realists" too often endorse formalist notions of science, and content themselves with denying the relevance of this notion of science to the study of the human. Enthusiasts who promote a special status for the human sciences concern themselves rather with the "special" sense in which such studies lay claim to knowledge and objectivity which distinguish the human sciences from the standards staked out by the friends of demarcation.[36]

The critical point to appreciate, the primary reason making possible the naturalists return, concerns the double failure — but both formalists and meaning realists — to make good on their respective claims to locate fixed points outside of science (however understood) by which to demarcate or delineate their respective objects of study (science and the world of nature, on the one hand, and shared meaning and rules on the other hand). That is, theorists of the social have proven no more adept than the philosophers of science they seek to displace in their efforts to specify the objects – rules and whatnot — which supposedly constitute the world-making stuff of "the social".[37] What "observation sentences" were to strict verificationists, "objective meanings" are to meaning realists, i.e., one pillar on which to base their claims to objectivity. The related pillar in each case constitutes the structural fixed points of analysis — analytic or logical truths for formalists, notions of rules or transcendental bases of meaning for meaning realists. In this respect, the friends of demarcation and the defenders of the special status of the *Geisteswissenschaften* alike were undone by the "holist turn" challenging alleged distinctions between truths certified by non-natural factors — the *a priori*, the eidetic, etc. — and those which people at a moment can find no reason to question.[38]

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[36]For an account of the relation which embraces the very distinctions my approach denies, see Joseph Margolis, "Knowledge in the Humanities and Social Sciences", in *Handbook of Epistemology*, ed. I. Niiniluoto, M. Sintonen, and J. Wolenski Dordrecht: Kluwer Academic, 2004, 607-646.

[37]See my "Mistakes", *Synthese*, 136: 389–408, September 2003. I return to this point in the final section of this.

[38]Richard Bernstein emphasizes a version of just this point as well. Phenoomenologists proved no better than positivists at disclosing the determinate structures of the social world, and for a related reason. For just as the positivist notion of rationality could not account for the persistence of seemingly "irrational" behavior, phenomenologists could not on their side separate causal determinants from those dependent on an individual's understanding. See Bernstein, op. cit., 156-67. Although I do not argue here for the view, I maintain that just as the positivist program required the analytic-synthetic distinction in order to forge a working theory of verification,

Neither of these dual and parallel failings — the inability to identity the uniquely "world stuff' to serve as determinate evidence for theories of nature and correspondingly fixed logical points or some special "social stuff' (meanings, practices) and fixed "mental" points (rules, transcendental grounds of meaning) on which to base accounts of the social world — threatens naturalism. For the very blurring of the concept of science allow investigators of the world around us to move past futile debates regarding demarcation — science versus non-science, natural versus social — and on to substantive debates regarding what difference proposed different theories make for purposes of explanation and social engineering.[39]

3 NATURALIZED EPISTEMOLOGY AND SOCIAL SCIENCE

In this final section, I wish to briefly explore two versions of naturalized epistemology which appear to hold special relevance for the philosophy of social science. One takes to heart the Darwinian paradigm and an evolutionary model. Major exponents of this view include, Donald Campbell, Alex Rosenberg, and Michael Bradie. The other major naturalistic approach stems from work by Barry Barnes, which he terms "natural rationality". The person who has pushed the critical edge of this view the hardest and the furthest is not Barnes himself, but Stephen Turner. I explore these views in turn.

Michael Bradie distinguishes evolutionary epistemology from other flavors of naturalized epistemology in the following way:

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phenomenologists needed a similar distinction between what is objective and constitutive of meaning and what is not in order to make the phenomenology of the social into a suitably scientific enterprise. But phenomenology fared no better with the notion of objective meaning than did positivists with the notion of analyticity, and with similar results. Without this principled distinction in hand, notions only hold fast because people choose to do so, and no mechanism exists, in any case, describing what it is to which people do hold fast when they favor certain beliefs over others.

[39]In this regard, what remains of the search for the "unity of science" consists primarily of what Charles Morris long ago presciently termed the "pragmatics" of science. See in particular Morris's essay "Scientific Empiricism" in the *Encyclopedia and Unified Science*, ed., O. Neurath, N. Bohr, J. Dewey, B. Russell, R. Carnap, and C.W. Morris, Chicago: The University of Chicago Press, 1938, 63–75. This is, in turn, Volume I and Number 1 of the *International Encyclopedia of Unified Science* (a series to which *The Structure of Scientific Revolutions* also belongs). Morris's remarks on 72–75 prove significant and prophetic, especially in the current scene where the notions of the social and the scientific are often treated as contrastive and inimical. In particular, Morris states, "Further, the confirmation of every proposition always involves some instrument, whether this be simply the scientist himself or in addition such instruments as those involved in experimentation — and methodologically there is no important distinction between the two cases. In this (theoretically the most important) sense, all empirical science involves experimentation, and experimentation is an activity, a practice... [S]cience is part of the practice of the community in which it is an institution, ministering — however indirectly — to the needs of the community and being affected — and very directly — in its development by the community of social institutions of which it is a part. It is clear that any adequate account of science must take account of these psychological, methodological, and sociological aspects of scientific practice." (ibid., 72) Compare with Dewey's essay in this volume, and note the contrast with the essays therein by Russell and Carnap.

> Human beings, as the products of evolutionary development, are nat-
> ural beings. Their capacities for knowledge and belief are also the
> products of a natural evolutionary development. As such, there is
> some reason to suspect that knowing, as a natural activity, could and
> should be treated and analyzed along lines compatible with its status,
> i.e., by the methods of natural science. On this view, there is no sharp
> division of labor between science and epistemology...Such approaches,
> in general, are called naturalistic epistemologies, whether they are di-
> rectly motivated by evolutionary considerations or not. Those which
> are directly motivated by evolutionary considerations and which ar-
> gue that the growth of knowledge follows the pattern of evolution in
> biology are called "evolutionary epistemologies".[40]

Bradie distinguishes between those who conceive of evolutionary epistemology in terms of mechanisms and those who imagine it in terms of competitor or successive scientific theories. The former comports well, he notes, with the contemporary understanding of biological theory. "There is a sense in which some version of the [evolutionary view of human epistemological/cognitive mechanisms] must be true if our current understanding of evolutionary processes is anywhere near correct. What remains to be seen is what useful insights, if any, will be forthcoming about the evolution of the cognitive mechanisms of organisms".[41] Bradie's characterization of evolutionary epistemology as focused on mechanisms might appear to raise again the question of whether naturalized epistemology somehow precludes inquiry into normative issues. He rightly rejects this implication. "If one construes knowledge along Quinean lines as a holistic product of norms and experience, then just as our knowledge *claims* are conjectural and subject to revision so the norms we employ to validate them can be construed as conjectural and subject to revision as well".[42]

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[40]Michael Bradie, "Naturalism and Evolutionary Epistemologies", in *Handbook of Epistemology*, ed. I. Niniluoto, M. Sintonen, and J. Wolenski Dordrecht: Kluwer Academic, 2004, 735–46, 735. This article contains a helpful and current bibliography of work in this area. The *locus classicus* for providing a formulation of an "evolutionary epistemology" in the philosophy of social science is Donald T. Campbell's "Evolutionary Epistemology" in *The Philosophy of Karl Popper*, ed. Paul A. Schilpp, LaSalle, IL: Open Court, 1974, 413–63. Worth notion here is Popper's uncharacteristically enthusiastic response to Campbell's essay, ibid., 1059–65. Campbell is guarded about the extension of this evolutionary model to a kind of social Darwinism view of theories in science, but he does not rule out a role for an evolutionary account even here. See, e.g., Donald T. Campbell, "Science Policy from a Naturalistic Sociological Epistemology", PSA: *Proceedings of the Biennial Meeting of the Philosophy of Science Association (1984)* 1984: 14–29. General discussions of the significance of Campbell's work on evolutionary epistemology can be found in *selection theory and social construction: the evolutionary naturalistic epistemology of Donald T. Campbell*, ed. Cecilia Heyes and David L. Hull, Albany, NY: SUNY Press, 2001. An anthology emphasizing more the Popperian roots of this view is *Evolutionary epistemology, rationality, and the sociology of knowledge*, ed. G. Radnitzky and W.W. Bartley III, La Salle, Illinois: Open Court, 1987.

[41]ibid., 739.

[42]ibid., 742. For a detailed defense of the view, see Paul A. Roth, "The Epistemology of 'Epistemology Naturalized' ", op. cit.

One of the most informed and trenchant commentators of efforts to apply a Darwinian model to issues in the philosophy of science or social science is Alex Rosenberg. However, his own writing appears to move through at least three distinct stages. Each separately is worth noting, and collectively they chart a type of evolution not just of a single thinker, but of a species of thinking about how biological models might (or might not) yield more general epistemological insight.

His 1980 book, *Sociolobiology and the Preemption of Social Science*,[43] marks what I take to be the first stage in the evolution of Rosenberg's thoughts on this topic. In this work, Rosenberg states forcefully a case for sociobiology as representing the "best bet" (perhaps the only bet) which would allow social scientists to fulfill an ambition to be scientists of the social. The key here, and where change occurs under the pressure of Rosenberg's own thought, is that at this point in time, Rosenberg accepts as given that biology counts as a science if anything does. However, that view changes.

In particular, his view on the nature of biological science alters in two distinct ways, each of interest in its own right. The first alteration primarily concerns what Rosenberg now views as a misplaced enthusiasm for the applicability of a Darwinian model to inquiry generally. While he does not rule out in principle, so to speak, that such a model could be applied, he provides by far the best available critiques in the literature of why proposals actually on the table fail to deliver as promised.[44]

But he also raises questions here regarding the status of biology as a science. Rosenberg's conception of science connects to a certain view of what passes muster as a law, and he professes skepticism with regard to the existence or even the possibility of such laws in biology. Thus a naturalist invested in the Darwinian model faces a double failing: on the one hand, a failure of the Darwinian model to does what the naturalist promises in epistemology, and, on the other hand, the failure of biology, *contra* what a work such a that discussed in the previous paragraph assumes, to be a science in the full-blooded sense of that term.[45]

However, in recent work Rosenberg brings together these seemingly conflicting themes — biology looms as the best model for social science, but no one can make this model apply in a fruitful way and biology does not seem to be a true science anyway — in an unanticipated but suggestive way.[46] For, Rosenberg argues, what three decades of work in biology reveals is how much more like the social sciences

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[43](Johns Hopkins University Press, 1980.)

[44]The key essays with respect to this facet of Rosenberg's thought are to be found in his excellent collection, *Darwininism in Philosophy, Social Science and Policy*, Cambridge: Cambridge University Press, 2000. Particularly notable are the first two essays in this collection, "A Field Guide to Recent Species of Naturalism" and "Naturalistic Epistemology for Eliminative Materialists".

[45]See his essay, "Limits to Biological Knowledge", in *Darwinism ...*, op. cit. This rehearses issues more fully argued in his *Instrumental biology, or, The disunity of science*, Chicago: The University of Chicago Press, 1994.

[46]The essay in question is his "Lessons from Biology for Philosophy of the Human Sciences", *Philosophy of the Social Sciences*, 35: 3–19, March 2005, hereafter just referred to in the text as "Lessons".

than the natural sciences biology is. But he maintains, still in all, social science would do best to move closer to the example biology provides.

> In what follow I will not be able to give more than a brief summary of what we have learned about the nature of biological science in the past three decades. My aim will rather be to show how it applies to a budget of problems in the philosophy of social science. I start with a simple argument that all the social and behavioral sciences need to be viewed as biological ones. Then I will try to show that doing so leaves most of them pretty much as they were ex ante. In effect my project is one of giving the right reasons (and displacing bad arguments) for viewing the social and behavioral sciences as pretty much on the right track, or at least as doing as well as can be expected in the business of explaining and predicting human affairs. ("Lessons", 4)

So, while remaining in the spirit of his work from 25 years earlier — the only plausible model for social science can be found in biological science, Rosenberg surprisingly concludes that what has transpired in the interim has reinforced this view by revealing a deep connection between biology and history, and so the filiation of biological science lies, in fact, in a social science. As he remarks, "Biology is an *almost* completely historical science." ("Lessons", 4)

As one might suspect, the key to Rosenberg's benign appraisal of the current state of affairs in the social sciences lies in the "almost", if not biology. On the one hand, evolution produces functionally related kinds; but functionally determined kinds make it unlikely that the adaptive traits will be explicable by appeal to perfectly general or universal laws as opposed to local, ecological factors. (See "Lessons", 5-6). But, on the other hand, the locality at work in determining kindness is offset, Rosenberg maintains, but the fact that evolutionary theory provides an account of mechanism which counts as a law even by the standard Rosenberg sets: "There is one law or set of laws that is distinctive of biology: the principle or principles of natural selection, which describe the way in which adaptations come about in a purely mechanistic world bereft of causally efficacious purpose, goals, or ends." ("Lessons", 6-7) So while historical (local) contingencies set the problems that organism must overcome or perish, natural selection provides the mechanism which determines how the story plays out.

This brings together the explanatory burden to which the biological or the human scientist must answer. "The task of the biologist and the human scientist is to identify the design problems faced by creatures so that they can individuate the adaptive traits, explain what they were selected for, reveal the mechanism by which they solve the design problem, and then if possible and interesting [sic] explain and predict the particular occasions on which these solutions are deployed." ("Lessons", 10) Rosenberg boldly speculates that this rubric will prove broad enough to cover what presently appears not as a common thread but rather as a gap between how so-called interpretative social science proceeds and the biological and natural sciences.

> Interpretative human science, hermeneutics, qualitative social science, symbolic interaction, these are all names for an approach to human behavior as unavoidable as adaptationalism is in biology. And the reason is simple. Humans are biological creatures and interpretation just is adaptationalism. The only difference between the subject matters of interpretative social science, the mathematical modeling social sciences, and the historical/comparative social sciences is the rate at which selection operates to overtake the generalizations these disciplines could or seek to articulate. ("Lessons", 15)

Rosenberg's account here remains at one and the same time the boldest and also the most specific proposal for adapting the biological model to the social sciences. Reduction now proceeds in terms of the search for a mechanism, and the mechanism in turn is that which the natural selection provides. "For each of the real patterns — transitory or persistent — uncovered in the human sciences, there must be a set of underlying mechanisms put in place by natural selection". ("Lessons", 19) It provides as specific a research program as has been proposed in this area. Whether the proposal finds takers, and its results, remains to be seen.

The other version of a naturalized epistemology which holds clear promise for social science turns on a proposal first articulated by Barry Barnes in a landmark piece published in the mid-1970s.[47] I shall refer to it as the "natural rationality" view (hereafter NR). In the work of the strong programme in the sociology of knowledge which Barnes helped found and with which he has so long been identified, this view has been encapsulated in what is termed the "symmetry principle", i.e., rational and irrational belief acceptance both require explanation. What makes "good" reasons good, that is, cannot not be assumed to be transculturally transparent.

But, I suggest, at least two different strains of NR apart from that propounded by sociologists of science such as Barnes can be identified. One I term the Weberization of the sociology of science, a view put forward in many works by Steve Fuller.[48] The other involves Stephen Turner's efforts to naturalize talk of rationality and normativity. Turner, in the spirit if not the letter of Rosenberg, lays

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[47] See Barry Barnes, "Natural Rationality: A Neglected Concept in the Social Sciences", *Philosophy of the Social Sciences*, 6: 115–126, June 1976. He returns to this theme and updates it slightly in his "How Not to Do the Sociology of Knowledge", *Annals of Scholarship*, 8: 321–36, 1991. But the core of the position does not alter from that articulated in the earlier essay.

[48] I will not here discuss Fuller's work, or at least that aspect of it relevant to a naturalized epistemology and its applications to a philosophy or sociology of science. But Fuller, it should be noted, has been a relentless critic of how sociologists of science, and many in the science studies field, have unflinchingly adopted a "descriptive only" approach to the study of science. Regarding a more general discussion of norms, in terms of how science ought to proceed, and particularly how science policy ought to be fashioned, he finds the science studies literature to be not just quiet, but quietistic. Unlike Woolgar, who emphasizes the irony of this approach, Fuller straightforwardly castigates those in science studies for this quietism. A good example of his work in this vein remains *Philosophy of Science and its Discontents*, 2nd Edition, New York: Guilford, 1993.

emphasis on understanding the mechanism of the transmission of the social.[49] While not concerned to emulate accounts which follow natural selection, his efforts have a not dissimilar effect, viz., eliminating from social science talk of anything irreducibly social as a causal factor.

A succinct statement of NR is the following: "the fact that we ourselves accept... knowledge as valid does not mean that its emergence, acceptance, and persistence are not empirical phenomena. Acts of validation and assertions of validity are themselves empirical phenomena, and as such are available for sociological investigation".[50] Perhaps one could quibble here with the use of 'sociological' to modify investigation, but the quibble would only concern the fact that the adjective might mistakenly imply a limiting kind of inquiry into the nature of the phenomenon in question. For what represents the core of NR resides in the claim that inferential practices constitute a type of empirical phenomenon, to be studied and understood along with other empirical phenomena, that is to say, naturalistically.

Insofar as inferential practices fall under the heading of empirical phenomena, all must be regarded as contingent. This might strike some as an endorsement of a form of relativism, but that would be mistaken. The spirit here, rather, is best exemplified by Quine's recurrent use of Neurath's metaphor of rebuilding the ship while afloat on it. One's belief structure must constantly be repaired and modified while in use. The status or epistemic place assigned certain practices only reflects a fact about the practices endorsed by particular groups at particular times.

Barnes's favored example of a work which explores 'natural rationality' in a manner he approves turns out, somewhat surprisingly, to be the work of a philosopher of science, Mary Hesse.[51] What Barnes likes is the use by Hesse of a social science to help understand how successful learning, and so the development of science itself, is possible.

> [Hesse attempts] to elucidate the natural proclivities which make learning of any kind possible—including the learning of conventions. She strives to identify the preconditions which enable us to find things intelligible at all. This is why her work must be praised and defended as a valuable essay in speculative psychology. Its subject is man as thinker rather than the logic of the natural sciences; its achievement

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[49]Of particular note here is his important book, *The Social Theory of Practices*, Chicago: The University of Chicago Press, 1994. See also his more recent collection of essays, *Brains/Practices/Relativism: Social Theory after Cognitive Science*, op. cit.

[50]Barry Barnes, "How Not To Do the Sociology of Knowledge", op. cit., 321.

[51]In this regard, much of the work of Ian Hacking reflects an examination of certain aspects of "natural rationality". See in particular his "Making Up People", in *Reconstructing Individualism*, ed. T. Heller, M. Sosna, and D. Wellbery, Stanford: Stanford University Press, 1986, "World Making by Kind Making: Child Abuse for example", in *How Classification Works*, ed. Mary Douglas and David Hull, Edinburgh: Edinburgh U.P., 1992, and "The Looping Effects of Human Kinds", op. cit. For a well-taken caution regarding the notion of "social construction", see Stephen Turner's essay, "The Limits of Social Constructionism", in Turner's *Brains/Practices/Relativism: Social Theory after Cognitive Science*, Chicago: The University of Chicago Press, 2002.

may or may not be epistemology; it is certainly a theory of natural rationality.[52]

There is here, as Barnes acknowledges in later work, more than a slight echo of themes from philosophers such as Nelson Goodman and W.V. Quine. The use of psychology to understand how humans might "bootstrap" themselves into more sophisticated forms of thought defines the naturalization of rationality.

Stephen Turner's work extends and deepens the account of natural rationality by challenging proposed explanations of reasoning which neglect to account for how the relevant norms and other "shared stuff" read into the heads of members of a society gets to be there. At the core of Turner's critique of many contemporary varieties of social theory is the "transmission argument": either provide an account of what is transmitted and how, or foreswear the use of a "shared something" as explanatory of observed uniformities in behavior. The core of the argument stresses that what can be observed by way of inculcating uniformities of behavior cannot account for what social theorists characteristically claim is shared, and so appeals to "sharing" turn out to be explanatorily idle — to add nothing to the noting of behavioral conformity. "There is in general no way to make a distinction between 'having habits that enable public proficiency' and 'possessing some shared thing of the basis of which proficiency is possible'."[53] The problem is worse than Turner's statement suggests inasmuch as there exists no accounts of the norms, rules etc. which any one individual follows, much less a going account of what sharers share "in the head". Notions such as practices and norms stand in need of clarification and explication and as such can make have no positive contributions when employed in the *explanans*.

The implications of this view for any theory of natural rationality prove profound. For it forces debates about the nature of the social to deliver on mechanisms which must themselves be found "in the open" and influencing individuals in particular ways. At this point, as Turner argues elsewhere, any meaningful distinction between talk of "social construction" and "ordinary" history collapses. For purposes of explaining the social, only history remains.[54]

"Naturalizing" the social has the consequence, both Rosenberg and Turner suggest, of eliminating the social as itself an explanatory notion. Given the paucity of results in social science, one may well wonder what would be lost in this case. Moreover, in each case, the naturalizing move has the advantage of explaining why results in the social sciences have been so meager and hard to come by. Neither rules out the discovery of or a role for generalization. Indeed, Rosenberg insists on such a role. But both indicate why such generalizations will be few, transitory,

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[52]Barnes, "Natural Rationality", op. cit., 121.

[53]Turner, *The Social Theory of Practices*, op. cit., 111. For an expansion and defense of Turner's points here, see my essay "Mistakes", op. cit., and "Why There is Nothing Rather than Something: Quine on Behaviorism, Meaning, and Indeterminacy", *Philosophy, Psychology, and Psychologism: Critical and Historical Readings on the Psychological Turn in Philosophy*, ed. D. Jacquette, Kluwer Academic 2003, 263-287.

[54]Turner, *Brains. . .*, op. cit., 119.

and difficult in any case to come by.

Bertrand Russell wrote that "every advance in knowledge robs philosophy of some problems which formerly it had, and... it will follow that a number of problems which had belonged to philosophy will have ceased to belong to philosophy and will belong to science".[55] The dismal image of philosophy Russell offers here pictures the discipline that can prosper and thrives only by lurking in those shadows when the sun of systematic scientific inquiry has yet to shine. Philosophy so conceived cannot survive coeval with science. (Russell, in fact, goes on to compare anti-scientific philosophers to those who continue to migrate to avoid the encroachments of civilization.)

Naturalism does not name the "better half" of a new dualism, one encompassing and superseding all others. To the contrary, as argued above, naturalists need not even insist that anything special marks science from all the rest. It can rest with discovering (and modifying) the conception of science as inquiry proceeds. Naturalists scoff at those who imagine that disciplinary boundaries carve the world at its joints and that department names name an intellectual essence. Thus, naturalism does not define itself by oppositions, but by placing philosophy within and as part of those disciplines which seek to make the best possible overall systematic sense of ourselves and the world.

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[55]Bertrand Russell, "What There is", [1918], reprinted in *Classics of Analytic Philosophy*, ed. R. Ammerman, Indianapolis, IN: Hackett, 1990, 34.

WE, HEIRS OF ENLIGHTENMENT: CRITICAL THEORY, DEMOCRACY, AND SOCIAL SCIENCE

James Bohman

1 INTRODUCTION

Critical Theory has had a complex relation to the Enlightenment. On the one hand, it is clearly its continuation, as when Horkheimer takes as a constitutive aim of a critical theory to liberate human beings from all circumstances that enslave them. The aim of Enlightenment criticism is freedom, in which human powers and capacities are no longer put in the service of "idols" or constrained by "self-imposed tutelage" but can be brought to bear upon the comprehensive goal of human emancipation. These images of immaturity and progress have been fraught with historical dangers. For this reason, many have rightly pointed out that Enlightenment can itself be a new source of domination. Horkheimer and Adorno's *Dialectic of Enlightenment* goes farther, showing the self-destructive tendencies of Enlightenment, tendencies toward the domination of nature, both inner nature and that of others. Thus, Critical Theory has been reflexive and self-critical in endorsing human emancipation, deeply aware of the paradoxes of freedom and domination and their unresolved tensions, ones that cannot be resolved once and for all in some definitive theory, but rather must be rendered productive in practice.

My goal here is to come to terms with the Enlightenment as the horizon of critical social science. I want to argue that Critical Theory, especially in the form of critical social inquiry, can understand its Enlightenment commitments not simply in terms of the progress of capabilities, but also in terms of freedom. It is just this dialectic between human freedom and powers that helps us to rethink the critical and political aims of a social science that promotes the freedom of human beings as active, natural and social creatures. In order to make sense of Habermas' adage that "in Enlightenment there are only participants", a thoroughly practical and philosophically pragmatic conception of Critical Theory needs to be developed. Here I want to suggest that any such reflexive, practical understanding of Critical Theory involves both democracy and social science. The potentially self-defeating dialectic of freedom and power can be resolved only in the ongoing process of democratization, which in turn requires a fuller understanding of the requirements of freedom in institutions.

A deep problem of the Enlightenment has been to develop a conception of human agency in terms of which freedom does not stand above nature and society but is continuous with it. There are many philosophers and social scientists who have developed such a practical conception of human beings that at the same time entails a particular ideal of freedom, including, among others, Marx, Sen, Habermas and Dewey. In "Ideal Understanding," Martin Hollis, a social scientific defender of the Enlightenment, links theoretical evaluation in the social sciences to the analysis of practical knowledge and skills. "Actors," he remarks, "have natural, social and rational powers."[1] This striking passage goes on to link reason and freedom to specifically social and normative powers and capabilities that make it possible for an actor to become an agent who shapes the social world. This idea of freedom and powers might also be the basis of a kind of social science that aims at understanding the conditions for the exercise of freedom in terms of such powers and capacities. Understood in this way, the social sciences are indeed "moral sciences" in the Millian sense. Far from creating moral experts, such a social science captures its evaluative and contested character as embedded social inquiry.

My argument for this interpretation of the connection between the Enlightenment and the moral sciences has three steps. First, I consider in more detail the understanding of the Enlightenment in Critical Theory, particularly its conception of the sociality of reason. Second, I develop an account of freedom in terms of human powers, along the lines of recent capability conceptions that link freedom to the development of human powers. These powers must include distinctly normative powers: the powers to interpret and create norms. Finally, I show the ways in which the social sciences can be moral sciences in the Enlightenment sense, not by promoting the perfectibility of human beings toward a state of virtue and happiness, but rather by overcoming those circumstances that enslave them and inhibit the development of their rational and social powers. This account provides us with a coherent Enlightenment standard by which to judge institutions as promoting development understood in terms of the capabilities necessary for freedom. The relevant social science in this area might include, among others, studies about the relation between development and democracy, specifically the robust generalization that there has never been a famine in a democratic society. Indeed, this example points toward a specific means by which freedom as development is promoted: democracy is the institutionalization of various normative statuses and freedoms, the most important of which are the freedoms and powers of citizens to assess rules and actively interpret and construct the operative norms of the social world in which they are embedded. I do not claim that this conception defines the future of Critical Theory, except insofar as it seeks to continue its project through combining an empirical orientation in social science with the normative ideals of a self-critical Enlightenment.

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[1] Martin Hollis, *Models of Man* (Cambridge: Cambridge University Press, 1977), 180.

2   WE, SOCIAL SCIENTIFIC HEIRS OF ENLIGHTENMENT

Even when critically discussing the social and intellectual heritage of the Enlightenment, it is clear that Critical Theory sides with "the party of Humanity". Its critique of the Enlightenment is not, as Foucault notes, for the sake of "faithfulness to a doctrine, but the permanent reactivation of an attitude, of a philosophical ethos of permanent critique of our historical era."[2] Adorno and Horkheimer did not attempt to deny the achievements of the Enlightenment, but rather sought to show that it had "self-destructive tendencies", that its specific social, cultural and conceptual forms realized in modern Europe "contained its own possibility of a reversal that is universally apparent today."[3] Since Adorno and Horkheimer planned to offer a positive way out of the dialectic of Enlightenment at the time that they wrote these words, this reversal is by no means inevitable. Even if their specific historical genealogy of Enlightenment out of myth is no longer as convincing, it is not enough to say with Habermas that *The Dialectic of Enlightenment* did not "do justice to the rational content of cultural modernity", however true this is in the historical analyses of Weber and Foucault on the one hand and Horkheimer and Adorno on the other.[4] For the positive task of avoiding the reversal of the Enlightenment, reconstructing the rational content of modernity is not enough, since the issue is not to affirm its universalism, but its self-critical and emancipatory capacity. If the issue is the self-correcting capacity of the Enlightenment, two questions emerge: how is it undermined? Where do we locate the exercise of this capacity? This is the "Enlightenment problem", the solution to which is self-reflexive social inquiry built into ever more powerful Enlightenment institutions.

The Enlightenment problem comes in various social scientific and philosophical guises. As heirs to the Enlightenment, we inherit not just sciences that help us achieve rational control, but also the idea of "a moral science" that could tell us how normatively structured social entities such as institutions can work better. This project of developing a moral science is articulated in Condorcet's "Sketch for a Historical Picture of the Progress of the Human Mind" with its noble goal of "joining together indissolubly the progress of knowledge and that of liberty, virtue and the respect for the natural rights of man."[5] Without endorsing this Triumph of Reason, Critical Theory offers a conception of a practical social science that is at one and the same time empirical, normative and practical. Continuing the Enlightenment project of realizing a rational society requires broadening our moral understanding, including changing our conception of the status of the social scientist.

Here I think that pragmatism with its political orientation already makes the proper move toward inquiry not as distinctive to science, but as an essential feature

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[2]Foucault, "What is Enlightenment?" in *The Foucault Reader* (New York: Pantheon Books, 1984), 42.

[3]Adorno and Horkheimer, *Dialectic of Enlightenment* (New York: Seabury, 1982), xiii.

[4]Habermas, *Philosophical Discourse of Modernity* (Cambridge: MIT Press, 1987), 113.

[5]Condorcet, quoted in Hollis, *Trust within Reason* (Cambridge: Cambridge University Press, 2002), 9.

of cooperative practices, the most self-conscious and reflexive of which is democracy. Democracy should be regarded as the preeminent Enlightenment institution for several reasons. First, if we look at all of the criticisms of the Enlightenment project, it is surprising that few defenders of Enlightenment follow Concordcet in arguing that Enlightenment is brought about not by science but by the use of reason in democratic institutions. Democracy is overlooked by many in the Enlightenment for at least two reasons: first, because democracy is tied to the freedom of the ancients; and second, because modern representative institutions were a pre-Enlightenment invention of the 16th century city-states and their republican conception of freedom. These institutions were not directly tied to the Enlightenment and its deep connection to the rise of the modern administrative state and the hope that rational experts could guide toward the proper ends of human flourishing. On the other hand, the dominant conception of democracy that we have inherited from liberal constitutionalism is deeply individualist and aggregative. The predominant liberal and Kantian inflected alternative is clearly tied to Enlightenment conceptions of freedom. Here freedom is articulated as self-determination that is expressed politically in the notion of autonomous self-legislation. Second, this understanding of democracy is in fact central to the modern constitutional tradition, in which the subjects of the laws, and thus of legal and political obligation, are also their authors. This concern with authorship sets out the issue as one of rational control, whereby democracy is the means by which the collective will of the people controls the processes of modern society. Whatever the appeal of self-legislation, it does not do justice to democratic practice as a whole and often lacks an explicit moment of inquiry that is essential to its liberating function. This ideal is also increasingly difficult to square with modern complexity, which undermines rational control via democracy, and with pluralism, which undermines both the singularity and decidability of the collective will. What is the alternative?

At the institutional level, democracy answers some of the problems of the Enlightenment project. One suspicion of the Enlightenment, articulated by its critics, is that it gives to its disciples the power to bring about a rational order. But this is to see the problem of Enlightenment as an *engineering problem*, in which social scientifically informed experts alone possess the knowledge necessary to make optimal choices. In his critique of Enlightenment cosmopolitanism, for example, Stephen Toulmin argues that its conception of rationality is committed to a "central apex of power."[6] This is not a necessary entailment of Enlightenment rationality as such. If we think of such a project instead as a democratic one, then such power must be dispersed, and experts are only one sort of participant in deliberative inquiry into solutions to problems and into the correct rules and laws that promote justice. Similarly, democratic rules are both enabling and constraining: they should be judged not simply by the instrumental success of their constraints in

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[6]See Stephen Toulmin, *Cosmopolis: The Hidden Agenda of Modernity* (Chicago: University of Chicago Press, 1990), p. 209. For a related criticism, see Danilo Zolo, *Cosmopolis: Prospects for World Government* (Cambridge: Polity Press, 1997).

protecting individual rights, but also in terms of the way in which they enable the very processes through which they are further shaped and interpreted by those subject to them. Finally, democracy is an institution that is not merely universal: it is realized in particular polities in social space and historical time that are always to some extent parochial. Indeed, constitutions are precisely the attempt to organize particular communities according to universal principles and thus to deal with this tension as an historical project.

This tension does not mean that we must embrace liberal nationalism. As many since Kant have pointed out, cosmopolitan political commitments, if suitably democratic, do not make this sort of constitutive tension go away, but rather provide further institutional mechanisms and locations for deliberation to manage it productively. The general point here is that democracy, rather than the market or even natural science, should be seen as the paradigmatic Enlightenment institution. More importantly, such an Enlightenment interpretation of democracy could provide the settings and statuses that develop human normative powers and freedoms. What is distinctive about democracy and similar distinctively modern institutions is not only that they involve practical skills, but also that the rule creation and implementation process is made explicit and subject to rational control; this reflexivity makes it possible for the rules to be tested and interpreted and thus for such a process to promote the flourishing and creativity of human powers. In this way the Enlightenment promise of achieving an explicit form of social normativity is indeed a liberating prospect.

If we take Kant as our guide, freedom is a matter of acting within the proper normative constraints, internalizing them as enabling constraints for the exercise of impartial reason. However, such freedom does not mark the difference between explicit and implicit norms. The explication of a norm or rule has an important effect on the potential reflexivity of practices. Only in being made explicit do practices become "commonable", to use a term coined by Philip Pettit. And once commonable, they become accessible to human rational powers and to the joint control of all those who participate in the practice. Moreover, such reflexivity can be institutionalized in practices of democracy in which all participants have the status of being co-authoritative over the very normative fabric of commonable rules that enable them to reason practically. This begins a virtuous Enlightenment circle, by which institutions do not merely realize normative constraints; they also create and make use of the "commons" created by explicit norms to provide opportunities for the exercise of normative powers over such contents and thus for innovative judgments that change these same practices.

Given this creative role for cooperative acts of explication of the content of norms, democratic institutions are distinctive for developing positive freedom in that they provide the context for the development and exercise of normative and communicative powers. By communicative power I mean the capacity to influence the opinions and will of others as participants in the public sphere. By normative power I mean the capacity to modify and change the rights and duties of others, as is the case when one has the powers associated with various statuses and roles,

such as that of being a citizen. This takes the account of normative powers one step further, by showing how democracy entails a particular understanding of the public exercise of such normative powers. Such a process is freedom not because it issues in consent or voluntary agreement, but because it sees obligations as the result of the joint exercise of normative powers in deliberation.

On this view constraints are not justified in terms of the satisfaction of individual wants, but in terms of the free development of human powers, where explicable and commonable rules and norms make possible forms of freedom that would not otherwise be available. Institutions and practices are thus rational to the extent that they promote such powers. Notice that democratic institutions add the condition that these rules must become explicit and available for acts of collective deliberation and interpretation. These reflexive processes are expressively free insofar as their norms self-consciously promote creative human powers. Next I develop this notion of freedom and human powers. While democracy signifies here a particular kind of reflexive, rule-generating institution, its political theory must be republican and not liberal if it is to capture the right sort of freedom for normative practices. This allows us to specify the Enlightenment problem more precisely: it is a matter of promoting the development of human powers without undermining the conditions for such positive freedom. That is, the democratic development of human powers must not be self-defeating: in the case of democratic institutions, the institutional capabilities can be developed without thereby increasing their dominating power.

Here Foucault's analysis is misleading, although instructive, about the problem of domination among persons. After showing correctly that Kant did not exclude the possibility of rational despotism, Foucault argues that there is only one way to avoid the self-defeating dialectic of Enlightenment: "How can the growth of capabilities be disconnected from the intensification of power relations?"[7] Foucault's way of putting the question is on the right track, but misleading and incomplete insofar as it suggests that disconnection is possible. Contrary to Foucault, the problem of rational domination is not a relationship between two terms, the capabilities of institutions and the relations of domination among persons, leaving out democracy as a mediating term. A self-critical Enlightenment puts the problem differently: instead of disconnecting reason from power, the increase in capabilities is not self-defeating so long as the democratic powers of citizens are appropriately institutionalized at the same time. The problem is then not with increasing institutional powers as such, but with the nondemocratic character of certain core modern institutions which are still in need of democratization. Here the discussion of the democratic peace hypothesis is illuminating: as democracies now become more warlike, it is because increases in the capabilities of citizens have not kept pace with corresponding increases of administrative and executive state power.

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[7]Foucault, "What is Enlightenment?" 48. Foucault asks the question in this way because he clearly thinks that this disconnection is possible; he also says that rejecting the Enlightenment ethos is not an option.

3   REDEFINING ENLIGHTENMENT DEMOCRATICALLY: FREEDOM AND POWERS

In this section, I attempt to fill out the connection between the model of normative agency and freedom understood as the development of human powers. Regardless of the specific metaphysical commitments of its normative model, the idea that the Enlightenment must initiate some form of practical and social scientific inquiry into human flourishing ultimately suggests some version of what Marx called "active naturalism:" a view of human beings as reflexive and creative beings with complex powers and capacities whose development opens up a space for socially embedded forms of freedom.[8] Freedom on this account is a matter of exercising these complex capacities, which include natural, social and normative powers. As Dupre points out, the causal powers of human beings enable them to create a great deal of order in the world. Insofar as they exercise their capacities socially, they are able to achieve many different ends that they would not be able to achieve alone, because the most characteristically human activities derive "not just from their internal structure or their brains, but depend on the relations of individuals to society."[9] The condition of the genuinely free individual is the result of embedding the individual in these social relationships. These relationships may either enhance or repress these powers, as is evidenced in relations of domination and subordination with their limitation on human functioning and on self-development. But this does not exhaust human powers, which also include distinctly normative powers, that is, those second-order powers related to the assignment of rights, duties and other deontic statuses.

Such human powers are practical in several senses. While the modern social sciences have traditionally been concerned primarily with practical knowledge in the sense of *techne*, when they are put in the service of democratic and emancipatory goals they may improve also reflexive practical knowledge exercised with others as in the case of deliberation and judgment. The pragmatists often did not clearly distinguish between techne and the reflexive practical knowledge of *praxis*, often construing even deliberation as a method or techne or sometimes even as a form of *poiesis* or human expressive activity analogous to the collective production of art. If pragmatism helps us to formulate a different conception of theory, it is not sufficient to develop a richer notion of the practical knowledge that guides our powers and capacities.

Thought of individualistically, the right course of action from the agent's point of view also has normative dimensions in the sense that I am using the term. Playing chess, for example, gives the players various roles with "rights and duties." In chess, however, as Hollis notes, the goals "are defined by the rules" so that "no

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[8] Marx, *Selected Writings* (Oxford: Oxford University Press, 1978), 104; 154; on human activity as world and self-transformative, *1844 Manuscripts*, 80. Marx did not appreciate the role of communicative powers, a lacuna in Critical Theory filled by Habermas. Habermas did not, however, see these powers as creative.

[9] John Dupre, *Human Nature and the Limits of Science* (Oxford: Clarenden Press, 2001), 181.

player can doubt that a move that delivers mate is the best move."[10] But in the social world we may doubt that existing rules constitute the best way to govern ourselves, and we may even doubt that we should do what is "rational", such as whether we should maximize profits according to the demands of instrumental rationality. If we seek to introduce some ideal of "economic democracy", we may reject the norm that firms act rationally by maximizing profits in every case. Here what makes the firm democratic is precisely the capacity of actors within it to question these very norms and offer different possibilities. Such deliberative practices do not take any particular set of normative constraints for granted or specific institutional roles as fixed, but rather regard them as subjects for potential transformation, along with the social relationships and powers upon which they supervene. In this way, specifically normative powers regulate social relationships and powers not merely by constraining them by rules or norms, but in being so regulated and at the same time also enabling those who possess these powers to produce "novel" possibilities of thought and expression. A theory of practical reason should then serve as one normative guide to the exercise of these powers. It could not do so alone, but must be informed by a social science oriented to freedom that, among other tasks, critically studies both the conditions for their effective exercise and the scope of their actual realization in various institutions and contexts of inquiry.

In any given institutional context, various powers and forms of freedom are deeply interconnected and interacting, so that freedom in its full exercise depends on a complex set of conditions, relationship and practices. Freedom is then a matter of the exercise of human powers in interaction. With this in mind we can see the appeal of Marx's notion of "the full development of all" as an ideal of social freedom. But such development is gained only through the exercise of these same powers. Such interconnectedness requires then that self-development and social freedom are mutually reinforcing; but it also implies that Enlightenment can be equally self-defeating if it recreates a new vicious circle between institutions and the exercise of freedom. This circularity implies that creating the conditions for exercising freedom is possible only within institutions that are already just in some sense. How do we get beyond this paradox of Enlightenment? Even if democracy as such is not the answer, it provides the framework for one. An essential feature of democracy is that it institutionalizes within social inquiry the expectation of discursive testing as a mechanism for learning and change. Such a method makes possible the introduction of new perspectives that can transform a democratic polity's normative and institutional framework.

The appeal to democracy implies that we participate in Enlightenment as citizens with appropriate normative powers. In no other role or location than as citizens in democratic institutions do members of modern societies exercise their normative powers of imposing obligations and changing statuses. Democracy itself is a creative form of inquiry typical of problem solving in cooperative social activity. A mode of inquiry is democratic to the extent that it seeks to take into

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[10]Hollis, *Models of Man*, 181.

account the positive and negative dimensions of current social conditions as well as to incorporate the various perspectives of all the relevant social actors in attempting to solve a problem. Deliberative democracy is a particular way of organizing and institutionalising such social inquiry based on collective reasoning, for which social facts are descriptions of problematic situations. Most of all, deliberative democracy permits citizens to go beyond preferences as given; unlike the market, the forum is not merely aggregative but allows participants to subject their preference to the scrutiny of others and thus to transform them.

Self-critical Enlightenment social science then has two tasks: first, to study reflexive and transformative institutions such as science and democracy, and second, to develop the proper framework for the free exercise of human powers and capacities. If democracy is itself a form of inquiry, then it is here that social science must be located so that it can promote freedom. This is evident in various social scientific studies of democracy and capabilities, such as the robust empirical generalization that there has never been a famine in a democracy. What accounts for this putative fact? It is not just that democracy entails certain freedoms and entitlements, but also that it creates opportunities to exercise these powers in such a way that the exercise of freedoms and powers is itself the object of institutional inquiry and dependent on our judgment and practical knowledge. The Enlightenment hope is then that the increase of this capacity will mean the increase of freedom, specifically freedom from domination by agents whose exercise of normative powers is not subject to any accountability. What powers of freedom does this institutional structure require? This is the problem of the democratic minimum.

4   DEMOCRACY AND JUSTICE: THE DEMOCRATIC MINIMUM

Enlightenment ideals justify democracy for any number of reasons. Many such justifications are intrinsic, as necessary for realizing various moral ends and ideals. The rights, equality and freedoms that are constitutive of the democratic ideal are substantively related to the ends of justice, including self-development and self-government. Even beyond such constitutive relations to the ends of justice, democracy is also desirable as a means to many different ends, including problem solving, pooling information, revealing preferences, and so on, all of which are contextually important means to achieving aspects of justice. Many Enlightenment reformers have placed their hopes in the ability of democracy to promote social justice. Indeed, democracy has been a linchpin of change in large part because the political status of persons as citizens has proved a robust basis for generalizing rights.[11] In many human rights documents such as the Universal Declaration of Human Rights, democracy is justified instrumentally as the best way to "foster the full realization of all human rights."[12] Empirical evidence also suggests other

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[11] For an historical account of the development of different generations of rights from individual liberties to political rights to social rights, see T.H. Marshall, *Citizenship and Social Class* (Cambridge: Cambridge University Press, 1950).

[12] UNCHR Resolution 1999/57, paragraphs 1 and 2.

constitutive and instrumental relationships. One well-known correlation is between famine prevention and democratic entitlements, such as freedom of the press and association; another may be the found in the democratic peace hypothesis.[13]

When viewed historically, it seems undeniable that over the long historical term numerous innovations often have made democracy a better means to achieve the ends of justice than its realizations in the past. At the same time, there is good evidence to cast doubt on the old adage often attributed to John Dewey, that "the only cure for the ills of democracy is more democracy."[14] While endorsing this hopeful Enlightenment stance, Dewey immediately introduces a proviso: it can remedy its ills only by becoming a democracy that is genuinely different in kind.[15] I have already alluded to the potentially vicious circularity that is introduced by giving democracy the Enlightenment ends of promoting freedom and justice. While it can never be said to disappear, the circle can become virtuous through the "democratic minimum", the achievement of a normative status sufficient for citizens to exercise democracy's creative powers to become different in kind and thus potentially more just. For democracy to be a means to Enlightenment, this sort of capacity or power must be exercised in the context of actual and thus nonideal institutions. This also means that it will be manifested in instances of institutional failure, traceable precisely to the absence of these powers among citizens.

It follows that under nonideal conditions democracy is related to justice in at least two different ways, and these complex relationships help give rise to the problem of the democratic circle. This circularity leads Rawls to distinguish between ideal and nonideal theory, where nonideal conditions are defined in terms of the likelihood of non-compliance. This leads him to distinguish domains of applicability of the theory of justice: international relations or relations among peoples are part of nonideal theory in which the requirements of political justice must be weakened for the sake of toleration among peoples.[16] An alternative to this diagnosis might be that the indeterminacy of human rights requires some reference to procedural justice in political institutions. Understood in terms of the theory of democracy, the methodological distinction between ideal and nonideal theory then simply assumes that the democratic circle cannot be broken. Instead, it should be replaced by a practical distinction between various sorts of nonideal conditions. Rather than paint all nonideal conditions with the same gray, it is better to distinguish them in terms of those that produce vicious democratic circles and those that are nonideal but still potentially virtuous. It could then be argued that in the latter nonideal case, the adage holds nonetheless that the solution to the problems of democracy is more democracy.

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[13] See Amartya Sen, *Poverty and Famine* (Oxford: Oxford University Press 1986)

[14] John Dewey, *The Public and its Problems,* in *The Later Works, 1925-1927*, Vol. 2 (Carbondale, IL: Southern Illinois University Press, 1988), 325.

[15] John Dewey, *The Public and its Problems,* 325.

[16] John Rawls, *Law of Peoples* (Cambridge: Harvard University Press, 2001), 32; and "The Idea of Public Reason Revisited" also in this volume, 172. On non-ideal theory and the issue of noncompliance, see *Law of Peoples*, 5.

Given the Kantian injunction that ideal conditions do not have empirical reality, even virtuous circles are able to operate under nonideal, but not entirely unjust, conditions. Tyranny provides the contrast class of *entirely* unjust conditions. Domination is possible without the total absence of justice, in mixed circumstances in which institutions may provide for some, but not all, of the conditions instrumental to justice. Determining how such democratic circles become fruitful under less than just conditions is the problem of the democratic minimum. Once delineated more precisely, it can then be argued that the democratic minimum is not specific to particular domains or particular institutions. This minimum or threshold may or may not be present in any particular transnational or international institution, just as it may fail to be present within constitutional states.

The former deficit is particularly apparent in the lack of transparency in many intergovernmental negotiations and in rules that permit only more powerful stakeholders in most bargaining situations. The latter case is clearly evident in the situation of citizens who are members of politically disadvantaged subunits, in particular in the institutionalized powerlessness of cities to govern themselves and solve problems under current arrangements. The purpose of the conception of the democratic minimum is then to describe the necessary, but not sufficient conditions for democratic arrangements to be a means to realize justice under nonideal conditions. Even if they are realized, a democracy will not necessarily be just in all its dealings. It may not be just in all domains in which citizens are obligated and it may not be just in relations with noncitizens affected by its decisions. To the extent that the minimum is a matter of degree, it can be specified along a number of dimensions and in a variety of procedures. But once this minimum is met, a democracy cannot become more just without becoming more democratic at the same time. While the conditions necessary for nontyranny are part of nondomination, it may well be the case that democracies in settler societies that continue to act in tyrannous ways toward aboriginal peoples have not met all their obligations to humanity. In the standard liberal view, the nontyranny condition could be fulfilled by simple noninterference, thus making the latter a plausible political means to realize more justice. What is lacking? The answer to this question provides a clue to the necessary conditions for democratically achieved justice.

The democratic minimum that breaks the democratic circle requires more of legitimate authority than that it grants the permission to be consulted. That such powers of consultation fall short of the democratic minimum can be seen through the republican contrast between citizen and slave. Some further normative power is required. Unlike the slave, the citizen has the ability to begin, to initiate deliberation; it entails the ability not just to respond, but also to set the items on an agenda. As Hannah Arendt puts it: "Beginning, before it becomes a historical event, is the supreme human capacity; politically, it is identical with human freedom."[17] This capacity marks the specific democratic contrast between citizen and slave, where the slave lacks in the simple capacity to initiate movement from one place to another or to speak unless spoken to. As Berlin notes in favor of liberal

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[17]Hannah Arendt, *Origins of Totalitarianism* (New York: Harcourt Brace, 1958), 479.

despotism, an enlightened liberal-minded despot may indeed desire to permit a large measure of personal freedom.[18] Nonetheless, whatever freedoms are granted the slave, she remains dependent on the desires of the master and dominated, because she lacks any intrinsic normative authority even over herself, and at best only can respond to the initiatives of others. The capacity to begin thus provides the basic measure for the normative status of persons required for the democratic minimum and not the maximization of available liberties by comparison with other polities. But in institutional terms, it establishes the possibility of accountability, that is, of accountability to the rules that also includes assessing their rationality.

According to some conceptions of the democratic minimum focused on accountability, the capacity to initiate deliberation is simply too strong. Some may argue that citizens do not need to be able to initiate and bring their concerns to bear upon deliberation, but rather, that they need only hold those who do deliberate accountable. Officials might be said to act nonarbitrarily when they "track" the "public good" of citizens, understood here in objectivist terms as something that officials can fail to track correctly for epistemic reasons.[19] Or, more modestly, liberals may argue that the democratic minimum consists in the right combination of "representative institutions that most reliably achieve the accountability necessary for protecting basic human rights."[20] In both cases the democratic minimum relies on institutional mechanisms that are post hoc and extremely coarse grained. They are post hoc to the extent that they allow domination to occur within a democracy even as citizens are given the power to change dominators rather than to avoid having one entirely. They are coarse-grained in their mechanisms, such as the removal from office through elections may not in the end eliminate objectionable policies at all, especially if they are deeply entrenched. Given both of these problems, the proposed minimum is simply too weak to ensure that in a democracy the circle of injustice could be overcome. Once interpreted in terms of social norms that structure a deliberative practice, the minimum must be much more interpretively fine grained, prospective and open to second order sorts of questions about institutions and their rules of procedural justice. Democracy then is understood not as a means to other ends but as a means of testing and promoting better social norms.

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[18]See Isaiah Berlin, "Two Concepts of Liberty" in *Four Essays on Liberty* (Oxford: Oxford University Press, 1969), 129. As he puts it: "Just as a democracy may, in fact, deprive the individual citizen of a great many liberties which he might have in some other form of society, so it is perfectly conceivable that a liberal-minded despot would allow his subjects a large measure of personal freedom. The despot who leaves his subjects a wide area of liberty may be unjust, or encourage the wildest inequalities, care little for order, or virtue, or knowledge; but provided he does not curb their liberty, or at least curbs it less than may other regimes, he meets with Mill's specification" of liberty. Notice that democracy need not be judged just because it would equalize or maximize all forms of liberty. Political equality developed in terms of nondomination is a threshold concept; the threshold would not be met when some have so many more political capabilities and resources than others so as to not require cooperation with all citizens.

[19]See Philip Pettit, *Republicanism: A Theory of Freedom and Government* (Oxford: Oxford University Press, 1997), 88.

[20]See Allan Buchanan, *Justice, Legitimacy and Self-Determination* (Oxford: Oxford University Press, 2004), 146.

A political order meets the democratic minimum in the first case only if it is a reflexive order. Only a constitutional order provides the appropriate reflexivity and openness to revision and deliberation that makes it a fundamental requirement of democracy, whether with respect to governance or government. The power of amendment and adjustment alone is not sufficient for the democratic minimum: what is distinctive about a constitutional order is the possibility of "reordering the order itself."[21] As Tully points out, this reflexive capacity must go all the way down (even if not all at once): "if citizens are to be free, then the procedures by which they deliberate, the reasons they accept as public reasons and the practices of governance they are permitted to test by these democratic means must not be imposed from the outside but must themselves be open to deliberation and amendment."[22] Even if citizens are not the fully self-determining authors of their own obligations, such a capacity to initiate a challenge and reorder the legal order itself (including rights, duties and boundaries) is a necessary condition for freedom in the sense of nondomination.

Such normative statuses may not be enough, especially when the institutional framework fails to offer opportunities for initiating deliberation and effective means to shape the course of deliberation through participation. In most historically democratic states, citizens may not have such normative powers in every respect: in the United States, for example, they may be free as federal citizens, but not free as residents of cities, which lack the institutional powers (such as legislative initiative) that would ensure that citizens can exercise their powers of freedom at this location. According to Gerald Frug, the development of the "legal definition" of cities as private corporations that came to define their entitlements and powers has made them increasingly powerless with respect to states.[23] The end result is that in the highly urbanized polity merging in the twentieth century, the American legal system created cities that are powerless to act on their own initiative and are thus dominated precisely with respect to the freedom to begin.

The capacity to initiate deliberation is itself necessary but not sufficient for the democratic minimum. As in the case of games, participants may acquire further rights and duties in their institutional roles as judges, executives and legislators. This unavoidable division of normative labor invests some citizens as holders of offices with further normative powers of enforcement and legislation. When acting in these roles, norms of equality require that citizens refrain from making it impossible for minorities to exercise their normative and communicative powers. If this is not satisfied, the current institutional structure has the potential for democratic domination of some citizens over others or of citizens over noncitizens. In democratic domination, citizens lack certain entitlements that create the dynamic potential for accountability in democracies generally and that allow them to re-

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[21]Charles Sabel, "Constitutional Orders: Trust Building and Response to Change", in *Contemporary Capitalism,* ed. J.R. Hollingsworth and R. Boyer (Cambridge; Cambridge University Press, 1997), 159.

[22]James Tully, "Unfreedom of the Moderns", *Modern Law Review* 32 (2002), 217.

[23]Gerald Frug, *City Making: Building Communities Without Building Walls* (Princeton: Princeton University Press, 1999), chapter 6.

alize Enlightenment aims. A much-analyzed and clear instance of institutional failure to promote normative powers is severe deprivation, in which citizens do not have the capacity to make claims to primary goods; they lack the entitlements or normative statuses necessary to make political institutions and their rules and norms promote justice. One well-analyzed case of such capability failure is the case of famines, in which the origin of the crisis is not just a drop in the supply of food, but the sudden loss of entitlement to food among the worst off in a society. Given the other entitlements and freedoms of citizenship, these and other basic freedoms should be more difficult to lose. Indeed, a reasonable formulation of this normative point is found in Amartya Sen's robust empirical generalization: that there have been no famines in a democracy. Or, to put Sen's generalization more precisely, at least not in a democracy that has achieved the democratic minimum.

5   FAMINE, FREEDOM, AND DEMOCRACY

I now want to return to the issue of how the social sciences can contribute to such an Enlightenment project. How can the democratic minimum be empirically operationalized? What might be its minimal conditions? Agency in a deliberative and reflexive democracy seems difficult to study and to measure in social scientific terms: in democracies, citizens exercise a distinctly normative agency, an agency over the rules, norms and entitlements by which their lives are governed. But citizens are also not just agents, or the authors of laws, but also subjects governed by those who have roles in political institutions as well as recipients of benefits tied to the status of being a member. Given this duality of citizenship, we might look at two sorts of cases. First, crises and periods of instability are often times in which entitlements are temporarily lost, such as is the case in famines. Here a minimal democracy which has institutionalized communicative and normative powers may be sufficient to make such entitlements secure. Second, we also should consider cases in which democratic institutions do not function well enough to promote justice and solve persistent problems, so that even with political liberties phenomena such as persistent poverty, preventable morbidity, and social exclusion have become endemic. In this case, minimal democratic practice is insufficiently robust to secure all citizens from domination.

The first cases reflect capability failures of citizens in their normative role as subjects; the second denote failures of rational control over the norms that constrain and enable agents' ability to lead lives that they find valuable. Besides his empirical work on the consequences of famines and other crises, Sen offers a rich conceptual basis for understanding these phenomena in terms of the powers or capabilities necessary for freedom in the social world. I suggest later that the same sort of analysis might also apply to the robust generalization that is called "the democratic peace hypothesis", which states that two democratic states (with the democratic minimum) only rarely go to war against each other, precisely to the extent that they promote the secure exercise of normative powers among their

citizens. The second generalization is more problematic, so that it may vary with the quality of democratic practices in securing nondomination.

Sen's explanation of famines begins with two striking facts. The first establishes that famines "can occur even without any decline in food production or availability."[24] Even when this is not the case, Sen argues that the solution of more equitably sharing the available domestic supply is nearly always an effective remedy to get beyond the crisis. Indeed, famines usually affect only a minority of the population of any political entity. Sen's hypothesis is that their vulnerability to starvation is explained by the loss of those powers and entitlements that they had before the crisis. The second fact—that famines affect minorities—goes some way in this direction by showing that the existence of famines is dependent on non-political arrangements, yielding the robust generalization that "there has never been a famine in a functioning multiparty democracy", so that "famines are but one example of the protective reach of democracy."[25] It would be tempting to associate this sort of security with the achievement of various instrumental freedoms or with one's status as a subject or client of a state or similar institution with effective administration. But even in the case of the protective function of the state much more is required of democracy to create (or recreate in a crisis) the conditions of entitlement, accountability, and the reflexive capacity to change the normative framework.

Once the explanation is put in the normative domain, so is the practical understanding of remedies and solutions. The practical effects of democracy are not tied to more effective administrative institutions or even the rule of law. As Sen notes, there are limits to legality: "other relevant factors, for example market forces, can be seen as operating *through* a system of legal relations (ownership rights, contractual obligations, legal exchanges, etc.). In many cases, the law stands between food availability and food entitlement. Starvation deaths can reflect legality with a vengeance."[26] In this sense the presence of famine is also to be explained via the operation of social norms conjoined with the lack of effective social freedom with regard to their content. The treatment of native populations in famines caused by colonial administrators is often due to the lack of substantive freedoms such as free expression or political participation. Thus, famine prevention can be achieved through fairly simple democratic mechanisms of accountability such as competitive elections and a free press, both of which distribute effective agency more widely than in their absence.

As we saw when discussing accountability in the democratic minimum, such protective mechanisms, however substantial the freedoms they promote, should not be overestimated without considering the extent of their actual exercise. They do not require the effective agency of all citizens as such, but only of those who play roles in assuring accountability in representative democracies, including opposition parties and investigative reporters. These mechanisms show that democracy

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[24] Amartya Sen, *Development as Freedom* (New York: Knopf, 1999), chapter 5.
[25] Sen, *Development as Freedom*, 184.
[26] Amartya Sen, *Poverty and Famine* (Oxford: Oxford University Press 1986), p. 165-166.

in this form is good at protecting citizens from the reemergence of tyranny and "preventing disasters that are easy to understand and where sympathy can take an immediate form."[27] At the same time, it is not at all clear that these mechanisms are sufficient for securing nondomination. Political arrangements that have a strong relationship to the prevention of famine treat citizens as entitled subjects and thus as possessing the capability to resist tyranny. These mechanisms may be expanded so as to make citizens themselves the agents that secure nondomination.

Sen clearly sees democracy not merely as a protective mechanism that empowers certain agents to act to defend the entitlements of citizens, but also as a location in which to exercise substantial freedoms, including the capability not to live in severe deprivation or to avoid the consequences of gender norms for overall freedom. It is clear that such substantive freedoms depend on normative powers. For example, India's success in eradicating famines is not matched in areas that require solving such persistent problems as gender inequality in which the normative powers are unequally distributed. Certainly, there is no robust empirical correlation between democracy and the absence of these problems; they exist in affluent market oriented democracies such as the United States. The solution for these ills of democracy is not to discover new and more effective protective mechanisms or robust entitlements, since it is hard for such a democracy to produce them. Rather, the solution is, as Sen puts it, "better democratic practice." This is not an engineering problem for the social scientist who masterfully chooses some optimal design solution, but rather a problem of empowering participants in a common deliberative practice to change the distribution of normative powers. To put it somewhat differently, the issue is not more protective democracy, but *democratization*, of extending the scope of democratic powers and entitlements and creating new possibilities of creative participation. Democracy is the project in which citizens, and not their agents for whom they are principals, exercise those normative and communicative powers that would make for better and more just democratic practice.

The "democratic peace hypothesis" is significant in this regard. It is not as robust a generalization as the correlation of democracy with the absence of famine. Democracies do go to war against nondemocracies and very infrequently against other democracies. Many explanations have been offered for why this is the case, but the issue here can be put in the same terms as in the case of famines: democracy promotes freedoms and powers in the normative role of citizens. The institutional capability to wage war increases with the executive and administrative powers of the state, which often bypass democratic mechanisms of deliberation and accountability. One mark of democratization is precisely the widening and deepening of the institutional powers of citizens to initiate deliberation and participate effectively in it. Often, this may entail qualitatively new rights. Charles Tilly has argued that warfare may have even served as a mechanism for the introduction of social rights, as the state became dependent on the willingness of

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[27]Sen, *Development as Freedom*, 154.

citizens to accept military service.[28] As modern warfare became increasingly lethal and professionalized, and as the institutional powers of the state outstripped this and other democratic mechanisms, the institutional powers of citizens no longer checked the institutional powers of states and left citizens vulnerable to expanding militarization. A new dialectic between the capacities of citizens and the instrumental powers of states has not reached any equilibrium, and the protective function of democracy in promoting peace now requires changing democratic practice, including the emergence of qualitatively different democratic institutions rather than new types of rights.

6   GLOBALIZATION AND DEMOCRACY: CREATING NEW DEMOCRATIC PRACTICES

In this section I discuss another aspect of the Enlightenment task of critical inquiry into democracy: the aim not just of improving democratic practices, but also of realizing greater and perhaps novel forms of democracy where none presently exist. As this aim cannot be reconstructed from the internal perspective of any single democratic political community, it requires a certain kind of practically oriented knowledge about the possibilities of realizing norms and ideals in *praxis* and is thus a theory of democratization, of creating a political space where none now exists. Adopting a term of Andrew Linklater, we may call this practical theory of praxis a "praxeology",[29] the purpose of which is inquiry into the "knowing how" of practical normative knowledge, that is, how it is that norms are ongoingly interpreted, realized and enacted under particular social and historical circumstances. A critical and praxeological theory of globalization must therefore solve two pressing internal problems: first, it must be shown how to organize social inquiry within and among transnational institutions more democratically; and, second, it must show the salient differences between national and transnational institutions and public spheres so that the democratic influence over globalization becomes a more tractable problem with feasible solutions.

I cannot here discuss the many different aspects of this problem, but rather suggest an alternative to the standard social scientific approaches that focus not primarily on globalization as imposing constraints on democratic institutions, but as also thereby opening up new institutional possibilities and new forms of publicity. In order to test these possibilities, critical theory must make itself a more open and multiperspectival practice; it must become global. It is in this context that we can press the questions of the normative adequacy of the democratic ideal that has been inherited from modern liberalism. Indeed, many critical theorists who defend a "cosmopolitan" conception of democracy have a surprisingly standard conception of how democracy is best organized discursively and deliberatively. For this

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[28] Charles Tilly, *Coercion, Capital and European States* (London: Blackwell, 1990).

[29] Andrew Linklater, "The Changing Contours of Critical International Relations Theory", in *Critical Theory and World Politics*, ed. R. W. Jones (London: Lynne Reinner 2001), 38.

reason, they have not asked the question whether such practices are able to sustain a sufficiently robust and cooperative form of inquiry under the new global circumstances of political interdependence.

In what respect can it be said that this novel sort of practical and critical social science should be concerned with social facts? A social scientific praxeology understands facts in relation to human agency rather than independent of it. Pragmatic social science is concerned not merely with elaborating an ideal in convincing normative arguments, but also with determining its realizability and its feasibility. In this regard, any political ideal must take into account general social facts if it is to be feasible; but it must also be able to respond to a series of social facts that ground skeptical challenges suggesting that circumstances make such an ideal impossible. With respect to democracy, these facts include expertise and the division of labor, cultural pluralism and conflict, social complexity and differentiation, globalization and the fact of increasing social interdependence, to name a few. In cases where "facts" challenge the very institutional basis of modern political integration, normative practical inquiry must seek to extend the scope of political possibilities rather than simply accept the facts as fixing the limits of political possibilities once and for all. For this reason, social science is practical to the extent that it is able to show how political ideals that have informed the institutions in question are not only still possible, but also feasible under current conditions or modification of those conditions. The ideal in question for recent critical social theory inspired by pragmatism is a robust and deliberative form of self-rule—also a key aspect of Critical Theory's wider historical project of the development of human powers and capabilities for freedom. Here the broad analysis of the main structural features of democracy takes a critical and practical turn when considering its transformation under new circumstances.

The issue of realizability has to do with a variety of constraints. On the one hand, democracy requires voluntary constraints on action, such as commitments to basic rights and to constitutional limits on political power. Social facts, on the other hand, are non-voluntary constraints, or within our problematic, constraints that condition the scope of the application of democratic principles. Taken up in a practical social theory oriented to suggesting actions that might realize the ideal of democracy in modern society, social facts no longer operate simply as constraints. For Rawls, "the fact of pluralism" (or the diversity of moral doctrines in modern societies) is just one such permanent feature of modern society that is directly relevant to political order, because its conditions "profoundly affect the requirements of a workable conception of justice."[30] This is not yet a complete story. Social facts such as pluralism have become "permanent" only to the extent that modern institutions and ideals developed after the Wars of Religion, including constitutional democracy and freedom of expression, promote rather than inhibit their development. Thus, for Rawls, regardless of whether they are considered in terms of possibility or feasibility, they are only considered as *constraints*—as

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[30]John Rawls, "The Idea of an Overlapping Consensus", in *Collected Papers* (Cambridge: Harvard University Press, 1999), 424.

restricting what is politically possible or what can be brought about by political action and power. In keeping with the nature and scope of entrenched pluralism, not all actors and groups experience the constraints of pluralism in the same way: from the perspective of some groups, pluralism enables their flourishing; for others, it may be an obstacle.

If this were the only role of putative "facts" in Rawls' political theory of modernity, then it would not be a full practical theory in the sense that I am using the term here. Rawls' contribution is that social facts differ in kind, so that some, such as the fact of pluralism, are "permanent" and not merely to be considered in narrow terms of functional stability. Without locating a necessary connection between its relations to feasibility and possibility, describing a social fact as "permanent" is not entirely accurate. It is better instead to think of such facts as "institutional facts" that are deeply entrenched in some historically contingent, specific social order rather than as universal normative constraints on democratic institutions.

This approach allows us to see the "facts" of modern societies as practical: they are precisely those determinations that are embedded in relatively long-term social processes, whose consequences cannot be reversed in a short period of time—such as a generation— by political action. Practical theories thus have to consider the ways in which such facts become part of a constructive process that might be called "generative entrenchment."[31] By "entrenchment of social facts", I mean that the relevant democratic institutions promote the very conditions that make the institutional social fact possible in assuming those conditions for their own possibility. When the processes at work in the social fact then begin to outstrip particular institutional feedback mechanisms that maintain it within the institution, then the institution must be transformed if it is to stand in the appropriate relation to the facts that make it feasible and realizable. All institutions, including democratic ones, entrench some social facts in realizing their conditions of possibility.

Consider Habermas' similar use of social facts with respect to institutions. As with Rawls, for Habermas pluralism and the need for coercive political power make the constitutional state necessary, so that the democratic process of law making is governed by a system of personal, social and civil rights. However, Habermas introduces a more fundamental social fact for the possibility and feasibility of democracy: the structural fact of social complexity. Complex societies are "polycentric", with a variety of forms of order, some of which, such as non-intentional market coordination, do not necessarily have to answer to the ideals of democracy. The social fact of complexity limits political participation such that the principles of democratic self-rule and the criteria of public agreement cannot be asserted simply as the proper norms for all social and political institutions. This seems ideally suited to understanding how globalization limits the capacity of democracy to entrench itself. As Habermas puts it, "unavoidable social complexity makes it impossible to apply the criteria [of democratic legitimacy] in an

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[31]William Wimsatt, "Complexity and Organization." *Proceedings of the Philosophy of Science Association*, ed. R. S. Cohen (Dordrecht: Reidel, 1974) 67-86.

undifferentiated way."[32] This fact makes a certain kind of structure ineluctable; since complexity means that democracy can "no longer control the conditions under which it is realized." While plausible, this claim lacks empirical evidence. Habermas here overestimates the constraining character of this "fact", which does little to restrict a whole range of indirect, institutionally mediated institutional designs. These mediated forms of democracy in turn affect the conditions that produce social complexity itself and thus stand in a feedback relation to them. The consequences of the "fact" of social complexity are thus not the same across all feasible, self-entrenching institutional realizations of democracy, and some ideals of democracy may rightly encourage the preservation of aspects of complexity, such as the ways in which the epistemic division of labour may promote wider and more collaborative problem solving and deliberation about ends. How might this alternative conception of social facts guide a critical theory of globalization?

When seen in light of the requirements of practical social science and the entrenchment of facts and conditions by institutions, constructivists are right to emphasize how agents produce and maintain social realities, even if not under conditions of their own making. In this context, an important contribution of pragmatism is precisely its interpretation of the practical status of social facts. Thus, Dewey sees social facts as always related to "problematic situations", even if these are more felt or suffered than fully recognized as such. The way to avoid turning problematic situations into empirical-normative dilemmas is, as Dewey suggests, to see even facts practically: "facts are such in a logical sense only as they serve to delimit a problem in a way that affords indication and test of proposed solutions."[33] They may serve this practical role only if they are seen in interaction with our understanding of the ideals that guide the practices in which such problems emerge, and thus where neither fact nor ideal is fixed and neither is given justificatory or theoretical priority.

The debate between Dewey and Lippmann about the public sphere and its role in democracy offers a good example of critical and practical social inquiry concerning social facts. In response to Lippmann's insistence on the preeminence of expertise, Dewey criticized "existing political practice" including the occupational and epistemic division of labor. At the same time, he recognized that existing institutions were obstacles to the emergence of such a form of participatory democracy and thus saw the solution in a transformation both of what it is to be a public and of the institutions with which the public interacts. Such interaction, he argued, would provide the basis for determining how the functions of the new form of political organization will be limited and expanded, the scope of which is "something to be critically and experimentally determined" in democracy as a mode of practical inquiry. The question is not just one of current political feasibility, but also of possibility, given that we want to remain committed in some broad sense

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[32]Habermas, *Between Facts and Norms* (Cambridge: MIT Press, 1996), 305.

[33]John Dewey, *Logic: The Theory of Inquiry. The Later Works 1938*, Vol. 12 (Carbondale IL: Southern Illinois University Press, 1986), 499.

to democratic principles of self-rule even if not to the set of possibilities provided by current institutions.

How do we identify the unsettling fact of globalization? Here the role of the critical social sciences is to examine the possibilities of the structural transformation of various conditions that make democratic practices possible. Dewey sees the normal, problem solving functioning of democratic institutions as based on robust interaction between publics and institutions within a set of constrained alternatives. When the institutional alternatives implicitly address a different public than is currently constituted by evolving institutional practice and its consequences, the public may act indirectly and self-referentially by forming a new public with which the institutions must interact. This interaction initiates a process of democratic renewal in which publics organize and are organized by new emerging institutions with a different alternative set of political possibilities. Of course, this is a difficult process: "to form itself the public has to break existing political forms; this is hard to do because these forms are themselves the regular means for instituting political change."[34] This sort of innovative process describes, for example, the emergence of those transnational publics that are indirectly affected by the new sorts of authoritative institutions brought about by managing "deregulation" and globalization. This account of democratic learning and innovation seems not to be limited by the scope of the institutions, even as the potential for domination also increases under current arrangements.

What sort of public sphere could play such a normative role? In differentiated modern societies (in whatever institutional form), one role of the distinctive communication that goes on in the public sphere is to raise topics or express concerns that cut across social spheres: it not only circulates information about the state and the economy, but it also establishes a forum for criticism in which the boundaries of these spheres are crossed, primarily in citizens' demands for mutual accountability. But the other side of this generalization is a requirement for communication that crosses social domains: such a generalization is necessary precisely because the public sphere has become less socially and culturally homogeneous and more internally differentiated than its early modern form. Instead of appealing to an assumed common norm of "publicity" or set of culturally specific practices of communication, a *cosmopolitan* public sphere is created when at least two culturally rooted public spheres begin to overlap and intersect, as when translations and conferences create a cosmopolitan public sphere in various academic disciplines. But if the way to do this is through disaggregated networks (such as the Internet) rather than mass media, then we cannot expect that the global public sphere will exhibit features of the form of the national public sphere. Rather, it will be a public of publics, of disaggregated networks embedded in a variety of institutions rather than an assumed unified national public sphere.

The same point could be made about taking existing democratic institutions as the proper model for democratization. To look only at the constraints of size in

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[34] John Dewey, *The Public and Its Problems. The Later Works, 1925-1927*, Vol. 2 (Carbondale, IL: Southern Illinois University Press, 1988), 255.

relation to a particular form of political community begs the question of whether or not there are alternative linkages between democracy and the public sphere that are not simply scaled up. Such linkages might be more decentralized and polycentric than the national community requires. Without a single location of public power, a unified public sphere becomes an impediment to democracy rather than an enabling condition for mass participation in decisions at a single location of authority. The problem for an experimental institutional design of directly deliberative democracy is to create precisely the appropriate feedback relation between disaggregated publics and such a polycentric decision making process. The lesson for a critical theory of globalization is to see the extension of political space and the redistribution of political power not only as a constraint similar to complexity but also as an open field of opportunities for innovative, distributive and multiperspectival forms of publicity and democracy. Such an interactive account of publics and institutions gives a plausible practical meaning to the extending of the project of democracy to the global level. It also models in its own form of social science the mode of inquiry that this and other publics may employ in creating and assessing the possibilities for realizing democracy. A critical theory of globalization not only points out the deficits of current practices, but also shows the potential for properly organized publics to create new ones. Since the new practices need not be modeled on the old ones, it is not a theory of democracy as such, but of democratization.

Put abstractly, just as in the project of improving existing democracy, realizing new democratic practices aims at developing effective social freedom for all citizens. Both aspects of this practical project may be thought of in terms of two basic political powers or capacities. First, it demands that the normative powers sufficient to resist domination are not simply derived from the formal status of citizenship or membership in a specific community. The republican adage that "to be free is to be a citizen of a free state", now requires multiple realizations, so that agents are empowered as citizens in a robust and overlapping set of institutions and in a variety of roles in democratic practices. Second, given the plurality of institutions and modes in which status can be realized, communicative powers also are needed to be "a free participant in the public spheres " that cut across these various contexts and establish robust connections among potential locations for the development of freedom and powers.[35] Central to these powers are joint control of the institutional commons and the capacity to initiate deliberation, and thus to exercise public reason with others. In a democracy, each must be able to exercise her reason "without let or hindrance", and not simply appeal as subjects to authorized agents who respond in light of their own criteria and grant entitlements in exchange for cooperation within existing practices. In some cases it is necessary not only to criticize such norms, but also to change the practices themselves.

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[35]Habermas, *Between Facts and Norms*, 146-147.

7   CONCLUSION

By discussing democracy in this way, I have tried to show just how deep the connections are between critical social science and democracy as central to the Enlightenment project. While existing democracies have their own problems, some of which are responsible for their inability to solve persistent social problems even in highly prosperous societies, the problem of democratic domination can be solved reflexively by better democratic practice. These improved democratic practices provide the context for the development of essential human powers and freedoms, and fruitfully check the normative powers of institutions with the creative powers of thought and communication.

For heirs of the Enlightenment today, this seems like the best option, even if it is possible that the ideals of democracy as embodied in protective democratic institutions are now using the normative powers of Enlightenment institutions to pursue other goals. This is now most clearly manifested in the other empirical generalization about democracy that I mentioned earlier. It no longer seems so clearly the case that democracies comprise an ever-expanding zone of peace. The conditions that make this generalization robust are internal to democratic practices and may now be disappearing, as fear and the need for security for many replace the rational interests in peace. If we are all indeed heirs of the Enlightenment in thoroughly criticizing the present age, we must know that the connections between democracy, peace and the absence of certain causes of human suffering are contingent historical facts and fragile achievements. No matter how robust the empirical generalizations about them turn out to be, critical social science shows that their contingency is based on the fact that they depend on the exercise of freedom. For that reason, they remain true only if we make them so. This contingency shows that not only can the Enlightenment sometimes defeat itself when the heightening of the power of administrative and executive institutions becomes settled practice without the appropriate deepening and broadening of the institutional powers of citizens. Powerful global market institutions may also defeat the goals of democratic practice. The emergence of new versions of the Counter-Enlightenment, which reject the aims of peace and freedom, is a constant historical possibility as well. In response to both, the Enlightenment must renew itself by creating a new and qualitatively different form of democracy.

RACE IN THE SOCIAL SCIENCES

Michael Root

1 INTRODUCTION

Race is often used as a descriptive or analytic category in the social sciences when studying differences between individuals within the United States in social or economic traits.[1] Social scientists routinely classify Americans by race when trying to describe or explain differences in income, employment, health care, crime, school performance, home ownership, drug addiction or marriage and divorce within the population and often find that the differences in the U.S. between the races in each of a number of social or economic variables or traits are significant. In particular, they often find that the values of these variables differ more between racial groups than within them [Gittleman, 2004]. Moreover, the differences between racial groups, their studies show, are persistent; year after year, the median or average value of a given social or economic variable for one racial group is found to fall below the median or average value for another.[2]

Social scientists have a reason to use race as a descriptive or analytic category in their studies of individual differences in a social or economic trait even if they disapprove of racial classification or dream of a day when people are not classified by race or when a person's race makes no difference to her social or economic status. A social scientist might say that race is a myth or an illusion but maintain that as long as members of a population classify each other by race and treat the members placed in one racial category differently from the way they treat members placed in another, a person's social or economic position will vary with race, and the category of race can have descriptive and explanatory power within a study of the variations.

2 THE NATURE OF RACE

For many years, race was taken to be biological race. People believed that blacks and whites differ in genes much as males and females differ in a chromosome.

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[1] Race is used, as well, in studying difference in socioeconomic status in other countries in which, like the U.S., many members of the population have origins in different continents.

[2] [U.S. Census Bureau, 1999], accessed online at www.census.gov/hhes/poverty/histpov/hstpov4.html on November 18, 1999. For a study of how income and wealth vary with race in the U.S. see [Gittleman and Wolff, 2004].

The biological conception that prevailed until recently included the following two
tenets: (A) people of different races differ with respect to many genetic traits and
(B) the populations identified as different races, at some point, were reproduc-
tively isolated, genetic differences developed between them, and the differences
passed from one generation to the next and to the present day. On the biological
conception of race, the races are natural kinds, and members of each race share
some underlying property or essence that causes each member to look or act more
or less like every other.[3]

2.1 Race as a Biological Category

Today, most biologists oppose both tenets and understand that the races are not
natural kinds. They allow that biological differences between human populations
customarily called different races are at best statistical; the populations, if biolog-
ically different at all, differ only in the average frequencies of a few polymorphic
genes [Cavalli-Sforza, 1994]. In addition, the differences are not concordant; the
differences between populations with respect to one gene vary independently of
any difference in another, and, as a result, there is no cluster of genes or genetic
traits possessed by all and only individuals customarily sorted at some site as
members of the same racial group [Lewontin, 1972].

Biological race assumes that our customary races – the people we customarily
classify as black or white – are divided by genes or heritable traits, but most bi-
ologists now understand that no cluster of genes or heritable traits divides them.
Though there are heritable differences between us, they do not cluster and do not
pick out the classes we call 'races'.[4] So, for example, we differ in skin color, and
these differences are heritable, but skin color is inherited independently of other
traits like blood type or eye color. Moreover, differences in color are continuous
rather than sharp and vary as much within as between racial groups, and differ-
ences in skin color divide us into subgroups that cut across rather than match the
groups we call races [Jones, 1981]. The fact that there are no biological races is
contingent, for the mechanisms of inheritance and selection could have divided our

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[3]The term 'race' first appears in English in the 16th century but seems to have been used
to pick out people with different customs or nationalities (See [Blanton, 1977] for history of
the use of the term); however, in the 18th and 19th centuries, 'race' acquired a specifically
biological meaning, and the groups picked out as different races came to be looked upon as
different subspecies or lineages of man. The biological concept of race was accompanied by the
view that the members of some races were superior in ability or virtue to the members of others
and was often invoked to justify the control of one group of people by another and, in particular,
to defend the Atlantic slave trade and the colonization of Africa and Asia by the countries of
Europe.

[4]A number of prominent American anthropologists contributed to the decline of the biological
conception of race. Ashley Montague, for example, argued in a number of popular books and
articles (e.g., [Montague, 1980]) that race is a social rather than biological category, and Franz
Boas debunked many of the claims of 19th century racial science (see his book [Boas, 1982])
and showed, using a great deal of ethnographic evidence, that the cross-cultural generalizations
advanced by proponents of the biological conception of race were mistaken.

species into biologically significant varieties or subspecies. Had our natural histories been different, we might have been more different than we are. Nature has a recipe for making biological races but ignored it. The recipe is this. First, isolate a breeding population. Second, wait for some distinctive heritable characteristics to appear. Third, give their conjunction a selective advantage. Fourth, let selection operate for a very long time, but be sure to keep the population isolated. Human evolution did not proceed according to such a recipe. Human populations, according to our best evidence, have not been geographically isolated for long enough periods of time; during all of natural history, there has been too much breeding between human populations to give us biological races.

2.2   Race as a Social Category

What are we to conclude from the fact that there are no biological races? Some people conclude that there are no races, that race is a myth or an illusion or that race is not meaningful in any scientific sense [Wheeler, 1995]. But race can be real or scientifically meaningful even if racial categories are not biological in the sense of satisfying (A) and (B). Marital status and occupation are not illusions, but there is nothing in human biology that places an individual in any of these categories either. Genes do not make us married or single, but the generalization that, in the U.S. today, single men are more likely to die of heart disease than married men is not only meaningful but true, and the social sciences can be expected to explain why.

Race is like marital status: no one would be married or single had we not invented matrimony. However, given that we did, we now divide ourselves along discernible boundaries, into categories like 'husband' and 'wife' or 'single' and 'divorced' and treat each other differently depending on which of these categories we belong to. So too with race: we assign each other a race and treat each other differently depending on that race. As a result, social scientists can discover that the values of a social or economic variable are different for one race than another even though race is not biological; they can discover that rates of death and disease vary with race just as they can discover that they vary with marital status even though marital status is not in our genes. In other words, race can be a scientifically salient category even though there are no biological races, and race can mark the risk of a biological condition like diabetes or heart disease even though race is not itself a biological condition but a social status [Root, 2001].

Though race is not a biological category, to the extent that a person is assigned a race based on her skin color or ancestry, race, unlike many other social traits, e.g., occupation or religion, is biologically rather than culturally transmitted. Race differs from marital status and most other salient social categories not in nature but in how the category is assigned. People in the United States often assign a race to each other based on a biological trait like skin color. Moreover, they often assign a child the race of a biological rather than an adoptive or custodial parent. That is, the biological offspring of two members of race R are members

of R, while the adopted children of two members of R are not unless at least one of their biological parents is. We can adopt children of other races, but, given our current system of racial classification, we do not make them members of our race by adopting them no matter how much of our culture we impress on them or how eagerly they embrace it.

Race in the U.S. is different from citizenship, for there are both naturalized and natural born citizens here but only naturally born blacks or whites. So while some residents can convert from Haitian to American citizenship, none can convert from black to white. Someone can pass as black or white, but passing is not being. A naturalized American citizen does not pass as one but is one. Though in the United States race is often assigned in virtue of a biological trait like ancestry or skin color, race is a social rather than a biological category, since (A) and (B) are false. Grouping people who share a genetic trait together and calling them a race does not make race a biological category. A country could limit citizenship to people with a particular eye color, and eye color is a biological trait, but were blue eyes to become a condition of citizenship, citizenship would remain a social rather than biological kind.

3   THE SCIENTIFIC STUDY OF RACE

Though race is a social and not a biological category, racial differences within a population can be studied scientifically, and race is currently an object of study within the social and the biomedical sciences [Lee, 2001]. Race is different from many other categories employed within the social sciences in once having been thought to be biological, and when today social scientists say that race is not scientific or that race is a myth, they do not mean that race does not describe or explain anything but that the descriptions or explanations are social rather than biological.

Race is a social rather than biological category, but that does not mean that the category is subjective and that a person is whatever race she takes herself to be. There is a difference between race and perceived race, for a person who passes for black or white only passes. Were being and being perceived the same, race would not be real but simply a matter of appearance. That is, race would not be real if to be black were simply to be seen as black, since 'real' implies a contrast and, in particular, a difference between being and seeming to be. A black person can pass as white in the United States no less than a male can pass as female, even though race is an invented and sex a natural category, as long as there are public criteria for assigning an individual to the category or deciding whether a person is a member or not.

3.1   *Racial Taxonomy*

People in the United States have classified each other by race since the founding of the country but not always in the same way. The federal government has classified

the population by race since the first census in 1790, but the racial groups into which the members are sorted have changed from decade to decade [Anderson, 1988]. There were nine census races in 1930 and 5 in 2000, and six of the nine races in 1930 were not races at all in the 2000 census. As a result, race, unlike a biological category, e.g., blood type or skin color, does not travel. Members of one race at the time of one census were members of a different race ten years later. In 1930, ethnic Japanese and ethnic Koreans in the United States were members of different races; in 2000, they are members of the same. Many blacks in 2000 would be mulattos had they lived in 1880. Because race is a social rather than biological category, a person's race can change with time even if she remains exactly the same and retains all of her ancestry.

Even if a man were to remain molecule for molecule or thought for thought the same when he moved from one country to another, his race could change; his race does not supervene on his mind or body and could vary even if his biological or psychological traits and his ancestry were to stay the same. Many people who are black in the U.S. today, for example, would be white were they living in Brazil, since in Brazil whether a person of African origin is black or white is a matter of her income and wealth and not simply her skin color or ancestry. Moreover, in some parts of the world people do not classify each other by race at all and do not treat each other differently based on either skin color or origin. As a result, there is no more reason to believe that the members of every human population have a race than that the members of every population have a social security number or zip code [Root, 2000].

Since race does not travel, cross-cultural uses of race as a descriptive or analytic variable in the social sciences are not very reliable. For example, the difference in median income between black and whites can be greater in Brazil than the United States not because blacks are exposed to more employment discrimination in the one country than the other, but because in Brazil a higher income can move person from one racial category to another. Since countries in which individuals are classified by race can employ different racial categories or assign race in a different way, comparing one country's racial statistics with another's is not always possible and often misleading.

Social scientists who employ race in their studies of differences in socioeconomic status within the United States must be careful not to assume that race can be used to describe or explain similar differences elsewhere. They should not take race, a trait defined for one population, and simply project the trait onto another or assume that if race can be used to describe or explain a difference in a social or economic trait in one country, race can be used to describe or explain a difference in another.

3.2 Racial Explanation

Social scientists routinely use race to describe differences between members of a population in a social or economic trait, but they often use race to explain some

of the differences between them as well. Race could describe a difference between members in a trait T without explaining why members of one race differ from members of another in T, for race could be a proxy for a cause of the differences in T rather than a cause of the difference. Were a difference in race a proxy for a difference in income, for example, and the difference in income a cause of the difference in T, then race would describe but not explain the difference in T. In other words, the association between the values of the two variables, between race and T, could be spurious, and as result, not explanatory [Humphreys, 1989].

When a social scientist does use race to explain why members of a population differ in T, her explanation is often a sketch rather than a complete explanation of the difference; sometimes she explains the difference in T by subsuming the difference under a racial generalization (usually statistical). If median income of whites in the U.S. is twice that of blacks, she might explain why a black man's income is lower than a white man's, for example, by citing his race. In doing so, she assumes that the generalization is law-like. Racial generalizations, however, do not seem to be law-like, and, as a result, the explanation seems to lack explanatory power.

Racial categories are local, and statistical generalizations about differences between the races are generalizations about a particular population of people. Nevertheless, though local, a racial generalization could be used to explain why two individuals within the population differ in T if differences in race were not merely associated with differences in T but were a cause of them as well.

3.3   Race as a Cause

Many social scientists will cite race as a cause of differences between members in a social or economic trait T if they have evidence of a strong association between race and T within the population and evidence that the association between the variables persists when other differences between the members are eliminated. The social scientist infers that race is a cause of the difference in T if the difference is present between members of the population who differ in race but are alike in other characteristics with which differences in T are often associated. That is, race is taken to be a cause if the association between race and T is not confounded by differences between the members in other traits that, in some circumstances, are observed to vary with T [Holland, 1986].[5]

Some social scientists, however, are reluctant to conclude that race is a cause of a difference in a social or economic trait between members of a population based on any analysis of correlations. They favor a deductive over an inductive approach to causal inference and do not take race to be a cause of a difference in a trait T between members of a population without a model or mechanism that indicates how a difference in race could cause T to differ between the members.

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[5]For a more in-depth discussion of the concept of causality across the social sciences, see [Woodward, this volume].

Racial discrimination is a familiar mechanism. Should blacks have a lower median wage than whites, race could be a cause of the difference if employers preferred white to black workers and, as a result were willing to pay white workers a premium for their color. Blacks and whites would differ in their wages because of how employers respond to the racial difference between them. Race, strictly speaking, would not cause blacks to have lower wages than whites: racial discrimination in the labor market would cause the difference. Given the mechanism of the market, however, correlations between race and wages would not be spurious, and differences in wages could be said to be the result of a difference between the workers in race [Arrow, 1973].[6]

3.4   Race as a Norm

According to some social scientists, racial differences in educational achievement are explained by differences in the social norms to which members of a racial group are expected to conform; the reason why black and white students differ in their grades or test scores in most public middle schools in the U.S., according to one prominent explanation of variability in minority school performance, is because black and white students are faced with different peer norms for academic achievement. Not every black or white student conforms to his peer norms, but for those who do, the difference in the norms, rather than racial discrimination, explains the academic difference [Fordham, 1986].

However, even if high grades or test scores in some schools induce peer rejection within some racial groups and admiration within others, racial discrimination could explain why one group adopts norms requiring members to engage in behavior opposing school success and the others do not. Were racial discrimination to explain the difference in norms between the groups, then racial discrimination would be a distal even if not a proximate cause of the difference in academic achievement; racial discrimination would cause black students to adopt a peer norm which, when the students conform to the norm, causes them to perform poorly in school.

When a social scientist says that a racial difference in a trait is the result of a difference in norms, she might mean that the difference in norms causally explains the difference in the trait; in particular, she might mean that members desire to conform to the norms of their group and decide how to act based on whether they believe an action conforms to the norms or not. When blacks and whites decide how hard to study in school, they choose to do what, for their group, is considered to be normal, but what is considered to be normal varies from group to group. The different choices that black and white students make, according to this account, cause the difference in their grades or test scores, and the

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[6]Were race valued in the employment market, then even if there were no association between a worker's race and his productive ability, a difference in race between two workers could cause an employer to offer them a different wage, and, as a result, the median wage of white workers in that market could be greater than the median wage of black workers even if they were the same in every respect other than race.

different choices are caused by the difference in the norms to which the students are expected to conform their school performance. In short, racial explanations that cite norms can be understood to be a form of causal explanation, and the causal chain leading to the racial difference in a trait will usually include the practice of racial discrimination.

4   RACE AND AUTHENTICITY

The races were never more real in the United States than when the laws required their separation, for the laws gave each member of the population a reason to treat a member of a different race differently than a member of her own. Even members who refused to comply with the laws or who flouted the regulations had to take them into account when making a choice between one seat on a bus and another. To the extent that members did what they were required to, the differences between racial groups in the U.S. in social or economic traits were greater than the differences within them, and a social scientist could explain why they were greater by describing the racial regulations and the degree to which the people conformed to them [Hacking, 1999].

Where there are standards for how members of a race are to behave, members who do not conform are sometimes labeled "lacking" or "inauthentic". A code of dress can become a standard for how black members of population are to look, for example, and blacks who choose not to "dress black" might be seen as false by other members of the community [Foucault, 1979]. Talk of authenticity invites controversy. Who decides how members of a racial or ethnic group ought to think or act? When blacks were subject to official segregation, the answer was the law. Today, the answer is much less clear. Nevertheless, some blacks do criticize other blacks for not being "black enough" and so continue to assume that race is a trait that can and ought to be regulated.

5   THE POLITICS OF RACE

A race becomes an identity if members of a population attach more importance to each other's race than to anything else about them; though people have multiple and alterable identities, membership in a racial group can become more central to how someone defines herself or is defined by others than her membership in any or many of the other groups she belongs to. To identify oneself with a race and base one's political preferences on one's racial identity is to engage in what has come to be called 'the politics of race' [Gutmann, 2003]. The politics of race is played out in the U.S. in debates about legislative districting and affirmative action, and both are familiar topics within the social sciences.

5.1   Racial Representation

In the U.S., the politics of race bears on how boundaries should be drawn when members of a population are placed in voting districts. In a number of states in the U.S. in which black voters constitute a significant percentage of the electorate, no blacks were elected to Congress even after the passage of the Voting Rights Act of 1965 and other federal legislation designed to enforce the 15th Amendment (guaranteeing all U.S. citizens the right to vote) and to eliminate discrimination in federal and state elections. Most white voters in these states voted only for white candidates, and in every district in which white voters were in the majority every black candidate was easily defeated, and since whites were a majority in every legislative district, no black candidate was elected to Congress.

Whites were a voting majority in every federal legislative district given the way the legislature of the states had drawn the boundaries of the district; where black voters were 20% of the state's eligible voters, for example, district lines were so drawn that whites were a majority in every district [Pilders, 1995]. Had the district lines been drawn so that minority voters were a majority in at least some of the districts, the chances that a black candidate would be elected to one of the state's congressional seats would have been better. One argument for race-conscious rather than race-neutral legislative redistricting is that, as long as many voters do engage in racial politics, the federal government should favor a district line that would increase the proportion of minorities within a legislative district over a district line that would decrease it, in order to give black voters a reasonable opportunity to elect black candidates to office.

5.2   Racial Discrimination

Social scientists have written often about the nature of racial discrimination and why, in the U.S. at least, the practice is so robust, but they sometimes disagree over how the practice should be defined or measured [Anderson, 2001]. Most social scientists distinguish between overt and institutional forms of racial discrimination. With overt racial discrimination, one person denies another an opportunity because of his race. An employer overtly discriminates against black workers, for example, if he refuses to hire them because he dislikes blacks or because he believes that those to whom he is accountable, e.g., his customers, do. Overt racial discrimination, most social scientists agree, is much less common in the U.S. today than before the passage of the federal civil rights laws.

Institutional racial discrimination is often defined in terms of past acts of overt discrimination; a person engages in institutional racial discrimination if she denies another an opportunity in virtue of a trait that, though not racial, is a result of an earlier act of overt racial discrimination. So, for example, if an employer has a race-blind hiring policy but hires only graduates of a college with a whites-only admissions policy, then though the employer does not practice overt racial discrimination, he practices institutional racial discrimination, since the feature on which he refuses to hire blacks, viz. lack of a particular degree, is the result of

a college admissions policy that overtly discriminates against blacks.

Without knowing the race of his applicants, the employer is able to discriminate against blacks to the extent that he counts as a qualification for the position an achievement that, due to overt racial discrimination, black applicants were not able to attain. Institutional discrimination has been widely studied by social scientists, and, as their studies show, a selection process that, on its face, is blind to race can have an adverse impact on members of a particular race if the members were victims of race-conscious selection some time in the past.

The civil rights legislation adopted by the U.S. government in the 1960's and 70's was designed to eliminate overt racial discrimination in employment and schooling. The new laws or regulations prohibited an employer or school official from choosing a white over a black applicant in virtue of his race. However, they did not prohibit an employer from choosing between applicants based on an achievement that, due to past acts of overt racial discrimination, blacks and whites did not have an equal chance to attain.

The U.S. government, in an effort to undo the effects of past acts of overt racial discrimination, adopted programs to allocate jobs and resources to members of racial minorities. The programs took a variety of forms but share the label 'affirmative action'. Each includes a race-conscious policy to increase the likelihood that a member of a racial minority will be selected in a competition for a particular scare social or economic resource, e.g. schooling or employment [Gutmann, 1996]. Many social scientists have studied the effects of these policies, and many have concluded that they reduce social or economic inequality. However, in recent years, the courts have ruled that many of the policies deprive whites of equal protection under the law and, as a result, are legally impermissible. Moreover, public opinion surveys indicate that affirmative action has become very unpopular. Nevertheless, institutional racial discrimination continues to limit the prospects of many members of America's racial minorities, and social scientists continue to explain many of the socioeconomic differences between whites and non-whites in the U.S. as the result of past or present discrimination.

5.3   Critical Race Theory

While some critics of affirmative action maintain that, as a means of reversing the harms of past acts of racial discrimination, racial preferences go too far, other critics maintain that they do not go far enough; they maintain that racial differences in income or wealth will not go away without a change in the standards employed by legislatures and courts in dealing with questions of justice. Their criticism rests on a theory of the U.S. legal system, called 'critical race theory', according to which the law exists to support the interests of those with the most political or economic power [Delgado, 2001]. The legal system, on this view, enables the powerful to use the courts to make their advantages appear deserved or legitimate. Proponents of critical race theory maintain that law is politics by another name and even when the courts seem to be neutral or value free, they are always partisan.

Though critical race theory is most closely associated with the work of a number of legal scholars, the theory has found support among social scientists as well, and though largely a U.S. movement, the approach owes a great deal to nineteenth European political theorists like Karl Marx and the twentieth-century Frankfurt school of German social philosophy. While the European critics were interested in how legal standards or ideas of justice help to sustain a society divided by class, critical race theorists are interested in how they help to maintain inequalities of race.

According to the proponents of critical race theory, the most important question for a social scientist to ask when studying a prominent feature of the U.S. judicial system, e.g., the rule of precedent, is how the feature limits the prospects of members of a racial minority. Moreover, on their view, social scientists should not simply describe how members are affected by that feature, they should work to overturn the standards or ideals of justice that make the feature seem fair or equitable. In this respect, critical race theory, like other critical approaches to the social sciences, opposes the view that the social sciences can or should attempt to be neutral with respect to those issues of moral or political value over which members of a society are likely to be divided; proponents of critical race theory do not accept the familiar distinction between describing a social practice and saying how the practice ought to be or the idea that social sciences should be value-free [Weber, 1968].

6   ASSIGNING RACE

In order to classify members of a population by race, a social scientist has to assign each member to one or more of some fixed number of racial categories. Many social scientists in the United States, when stratifying a sample or target population by race, rely on the classification system and standards for the collection and presentation of data on race adopted by a bureau or agency of the federal government or simply employ data already coded for race. As a result, they adopt the government's conception of race and assume that the government's assignments of race are reliable and accurate.

If, according to the government's standards, each member of the population is a member of one of five races, then the social scientist describes how a social or economic trait varies between five groups, and if the government divides the population into four races, she describes how the trait varies between four. Most social scientists follow the federal government and treat race and ethnicity as different but overlapping categories. A member of the U.S. population, according to the Office of Management and Budget (the federal office responsible for how agencies of government collect and present racial data), can be Hispanic and either black or white, and a person classified as white can be a member of any one of a number of different ethnic groups. Moreover, while a child in the U.S. acquires the race of a biological parent, he can acquire the ethnicity of his adoptive or custodial mother or father, given the way racial and ethnic categories are customarily assigned by

agencies of government.

Nevertheless, some social scientists treat race as an ethnic category; they assume that since there are no biological races, a racial label can only pick out an ethnic group [Omi, 1989]. In addition, some equate race with ethnicity because many of their subjects do. Many Americans of Asian descent, for example, if asked their race are more likely to answer with an ethnic category like 'Korean' or 'Chinese' than a racial category like 'Asian'.

6.1   The Fluidity of Race

Most social scientists assume that race is a fixed characteristic and that the size of racial populations does not vary in any significant way with how individuals are assigned to a race; they take the race of members as fixed and try to describe or explain a difference in a trait between individuals whose race is different. For many individuals in the U.S., however, race is not a fixed characteristic but varies depending on how race is counted and, in particular, whether the category is based on self-reports, observer reports, or birth records.

As a result, statistical measures of racial disparity in a socioeconomic trait can vary with differences in the way social scientists stratify populations by race, and the social sciences need to consider how a race should be assigned to a member of a population when describing or explaining differences between groups in a trait. Given the complexities of racial identity in the U.S., there is no single best way for individuals to be racially categorized. How best to assign race depends on a social scientist's interest or on the character of the trait whose variation within the population she is trying to describe or explain.

For some social scientists self-reports are the gold standard when classifying by race; the social scientist takes a person to be the race she assigns herself. Though there are other ways to assign race to members of a population, these social scientists view self-reports as a proxy or surrogate for a person's race on her birth record or the race she is most often assigned by others. As a result, on their view, whether race is assigned using self-reports, birth records or other-reports makes little difference when using race to describe or explain differences within a population in a social or economic trait.

Assigning an individual the race she assigns herself is often the easiest or most respectful way for a social scientist to assign her a race. By allowing each individual to be the arbiter of her own race, the social scientist displays the social nature of our system of racial classification and gives individuals control over their own identity. Nevertheless, the easiest or most respectful way for social scientists to identify the race of their subjects might not give race as much descriptive or explanatory power as a less easy or respectful way.

If self-reported and other-reported race are different in many cases, then other-reported race will better explain differences in a social trait within the population to the extent that these differences are due to racial discrimination, since an individual's exposure to racial discrimination is based less on the race she assigns

herself than on the race she is assigned by others. As a result, self-reported race should be the gold standard in studies of racial differences in a social trait within a population only if there is good evidence that self-reported race is a proxy for the race one person is likely to be assigned by others.

6.2   The Reliability of Racial Statistics

A workshop at the National Academy of Sciences concluded in 1996 that research is needed to assess the data compatibility between racial identification done by self-reports and the reports of others. Studies conducted since suggest that, at least in cases of children with mixed race parents, Hispanics and foreign-born Americans, self-reports and other reports do not yield comparable counts of blacks and whites. Moreover, within young multiracial populations, the race members assign themselves often varies with context. That is, in the U. S., the self-identified race of adolescents with mixed race parents seems to be fluid rather than fixed.

Multiracial individuals, according to the National Longitudinal Study of Adolescent Health, frequently assign themselves a race different from the one they are thought to be by an interviewer; for example, only 67% of the children of black-white unions and who identified themselves as white were identified as white by interviewers, while 95% of those who identified themselves as black were identified by interviewers as black [Harris, 2000]. If 33% of the multiracial children who report being white are exposed to no less discrimination than children who report being black, then, in these cases at least, other-reported race is a better marker of exposure to racial discrimination than self-reported race.[7]

Many people report their race differently depending on the options they are given. Many respondents to the 1996 U.S. Census who identified themselves as a member of a minority race, when told to choose only one of the Census races, identified themselves as members of more than one race in the 2000 Census, when given the opportunity to do so.[8] By requiring a respondent to identify herself as a member of one of four racial groups, the Census, prior to 2000, hid interracial births and made it difficult for individuals to report them and the social and biomedical sciences to count them.

Individuals born in the United States are more likely to assign themselves an observer-reported race than foreign-born Americans are, according to a number of recent studies, since native-born Americans are accustomed to using the census races to characterize people, while foreign-born Americans are more accustomed

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[7]In 2002, 6 states and the District of Columbia piloted a "Reactions to Race Module" on the Behavioral Risk Factor Surveillance System (BRFSS) that includes the question, "How do other people usually classify you in this country?" followed by the OMB race categories and the category "Hispanic and Latino". Since the BRFSS also asks the OMB ethnicity and race questions, social scientists should be able to compare the responses and measure the degree of correlation between imputed other-reported and self-reported race.

[8]The opportunity to report more than one race is based on the growing recognition of interracial marriage in the United States and also on increased opposition to the rule of hypodescent, i.e., the rule that the children of a mixed union or mating between members of different racial or socioeconomic groups should be identified as members of the less privileged group.

to using ethnicity or nationality to identify themselves [Waters, 2000]. As a result, with increasing immigration to the U.S. of people from Africa and Asia with conceptions of self-identity different than those common in the U.S., differences between self-reported race and observer-reported race can be expected to increase, and consequently self-reported race will become a poor proxy for exposure to racial discrimination and a less useful category to use to describe or explain a difference between members of U.S. population in a social or economic trait, to the extent that the difference is due to overt or institutional racial discrimination.

Frequently, when multiracial children are asked their race, their answer depends on who is asking or where they are asked the question. Only 88% of respondents to the National Longitudinal Study of Adolescent Health, for example, gave the same responses to the race question when asked at school and when asked at home. If race is to be used as a descriptive or analytic category in the sciences, then the methods used to assign race to members of a population should be reliable, and if assigning race based on self-reports is not, then self-reports should not be the method of choice in studies that purport to describe or explain racial differences.

Prior to 1960, the U.S. Census enumerated race based on phenotype (most census takers inferred a respondent's race from her skin color and other bodily features); in 1960, the practice of self-definition began, and all members of a household were counted as black if the head of household reported being black. In the 1960 census, many Hispanics who had been counted black in 1956 did not report being black, especially if they arrived in the U.S. from a Spanish-speaking country in which they were not counted as black. As a result, in the case of foreign-born Hispanics, at least, self-reported and observer-reported race are often different.

Moreover, according to studies conducted by the U.S. Census Bureau, when respondents are asked whether they are black and, in addition, whether they are Hispanic or non-Hispanic, their response often depends on the order in which the two questions are asked. If the race follows the Hispanic question, more respondents report being black than if the race question is asked first; when asked first, many Hispanics report that they are not any of the five racial categories listed in the census survey.

6.3   Context Specific Racial Identity

Hispanics, like mixed-race Americans, seem to have a variety of context-specific racial self-identities, but, despite their self-identities, if many are consistently identified as black by others, then a social scientist who is interested in how access to housing, education, mortgage lending, health care services or employment opportunities vary with race has a reason to take other-reports rather than self-reports to be the best measure of the person's race and to stratify the population by the races the members assign one another rather than those they assign themselves.[9]

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[9]Other-reported race, within some populations, may be as variable as self-reported race. When describing or explaining how a trait T varies within such a population by race, a social

In the 2000 Census, 6.8 million people, or 2.4% of the total U.S. population, reported having two or more races. According to a rule adopted by the Bureau of Census on the aggregation and allocation of multiple race responses for use in civil rights monitoring and enforcement, responses that include one minority race and "White" are allocated to the minority race. Why the minority race? Because, in the context of civil rights, what matters most is not whether a person sees herself as white but whether others see her as a minority; if a person who sees herself as both "White" and a member of a minority race is treated as a minority by members of the majority who practice racial discrimination, then the agencies that monitor and enforce the civil rights laws have a reason to identify as black anyone who identifies herself as both black and white.

As more questionnaires or surveys allow respondents to report more than one race, or as the mixed race movement in the U.S. grows, more people will identify themselves as more than one race, and the less reasonable it will be for the social sciences to treat self-reported race as a marker or proxy for a person's exposure to racial discrimination. As a result, the reason behind the current rule for aggregating and allocating multiple responses to the race question in the U.S. Census is also a reason for scientists to favor other-reported over self-reported race in assigning race to individuals. Instead of asking members of a population what race they assign themselves, a sociologist or anthropologist should ask them what race other people usually take them to be, whenever there is reason to suspect that differences between members in a social or economic trait is primarily due to racial discrimination.

When the U.S. Bureau of Census classifies as black respondents who self identify as white and black, adding them to the count of blacks rather than whites, they are not failing to count the actual number of blacks or whites in the population; for the census, like any other data set, captures an individual's actual or underlying race relative to a particular purpose, and, in the case of the census, one major purpose is to monitor civil rights [Harris, 2002]. Whether a data set in the social sciences captures the actual or underlying race of members of a population also depends on the particular purpose for which the data are collected. Usually, in the social sciences, the purpose is to describe or explain a variation in a socially significant trait within a population. Relative to describing or explaining the variation in one trait, a member might be identified best as a member of one race, but relative to describing or explaining the variation in another, she might be identified best as a member of another.[10] As a result, from the perspective of the sciences, race should be understood as a fluid rather than a fixed characteristic of persons. A person's actual race should be allowed to vary with the trait the social scientist

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scientist should consider whose other reports are most likely to affect T. Where T is the risk of a traffic stop, the reports of police officers of a motorist's race matter most. Where T is the risk of invidious discrimination in employment, the employer's reports of a worker's race matter most.

[10]When self-reported and other-reported race differ within a population of high school students, self-reported race, for example, might better explain than other-reported race why black students more often expect themselves to fail than white ones do, while other-reported race might better explain why the black students do more often fail.

has chosen to study, and a person's race should not be expected to travel from trait to trait any more than from place to place.

7   THE CONSERVATION OF RACE

Some people maintain that though race has been used in the U.S. since the founding of the republic to place individuals into categories, racial classification is so closely linked to racial discrimination that race should no longer be used to classify individuals or place them into groups. Since race has been used so often in the cause of social or economic injustice, racial categories, they would argue, should not be conserved, and the U.S. should become a race blind rather than race conscious society. Others argue that despite the history of injustice in the U.S. in the name of race and despite the presence of racial prejudice and discrimination, race provides people with a social or collective identity, and racial categories should not be eliminated but retained.

W. E. B. Du Bois wrote, over one hundred years ago, that America's great problem is the color line, but he also wrote that the races should be conserved. Du Bois did not believe that race was a natural kind, but racial categories, he thought, picked out people who share a point of view [Du Bois, 1992].[11]A race is a cohesive class of people, on Du Bois's view, but the cohesiveness is not defined or explained by shared genes, descent from an isolated ancestral population or any common morphological traits; the cohesiveness is a matter of social rather than natural history.

The history of the world, according to Du Bois, is not only the history of individuals but also of nations and races, and though a race is not a class of individuals with a common biological makeup, members of the same race, on Du Bois's view, can share a calling or a purpose. Du Bois, as a number of commentators have noted, seems to have had a messianic view of race [Lewis, 1994]. Each race, Du Bois seems to have believed, has a particular message and genius to contribute to humanity, and the Negro people, according to Du Bois, have a duty to maintain their racial identity in order to carry the message and contribute their particular genius to the world.

Races, on Du Bois's view, will not be conserved unless people conserve them; we have to continuously draw the lines between the races, he thought, or else the races will disappear, and he does not want them to disappear. For Du Bois, assimilation is not the solution to the problems of racial prejudice or discrimination, for should people become assimilated, they will not have a race to give them a social identity.[12]

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[11]The term 'race' identifies a group of people, he wrote, "who are both voluntarily and involuntarily striving together for the accomplishment of certain more or less vividly conceived ideals of life", [Du Bois, 1992.

[12]Many opponents of invidious racial discrimination maintain that in order to eliminate such discrimination racial classification itself must be eliminated. Richard Lewontin, in "The Apportionment of Human Diversity", for example, writes: "Human racial classification is of no social

Though few people today believe, as Du Bois did, that a race has a mission, many believe that race is worth conserving. Current efforts to redress the effects of racial discrimination, they argue, require race-conscious rather than race-neutral selection in education or employment; in addition, racial classification should be conserved, some would say, in order for people to maintain a sense of racial pride or accomplishment. There will be no continuing black literature, music, art or philosophy, they would argue, unless we conserve racial classification, and our lives would be poorer without them. Racial classification makes our society more pluralistic and multicultural, and our society, on the view of the conservationist, benefits from a market of many cultures or races no less than from a market of many ideas.

Nevertheless there are a number of reasons not to conserve race. First, racial classification makes racial profiling possible in law enforcement; were policemen or judges not conscious of race, black drivers would not be stopped and searched for illegal drugs at four times the rate of white, and black defendants awaiting trial would not be twice as likely as white defendants with the same socioeconomic status, or SES, to be denied bail. Second, racial classification is politically and socially divisive and promotes a politics of race over a politics of common interest; each person, in voting the interest of his race, can leave everyone less well-off than he or she would be were no one to vote by race at all. Third, racial classification makes the continuation of false biological theories of race possible; as long as we continue to divide people by race, individuals will believe that there are biological races and that some are better than others.

Many social scientists are committed to value-neutrality and to the view that while they are able to describe or explain how members of a population classify themselves, they are not able to prescribe or recommend how the members ought to. As a result, though many social scientists study how members of the U.S. population use racial categories and how the categories affect the distribution of socioeconomic status within the population, most remain silent on whether members should use these categories, and silent on the issue, of concern to Du Bois, whether race ought to be conserved or eliminated.

Many social scientists will argue, however, that as long as members of a population use racial categories, race should be used in studies of differences between members in a social or economic trait to the extent that the trait varies with race [Lee, 2001]. In other words, race, on their view, should be conserved in the social sciences as long as race is conserved within a population studied by the sciences. As a result, most social scientists would oppose a campaign, like one in California, to prohibit state or local governments in the U.S. from using race to classify current or prospective students, contractors, or employees, for were governments not to classify members of the population by race, social scientists would not be able

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value and is positively destructive of social and human relations. Since such racial classification is now seen to be of virtually no genetic or taxonomic significance either, no justification can be offered for its continuance". Du Bois would agree that race is of no genetic significance and has been positively destructive of social and human relations but not that it is of no social value.

to discover racial disparities within the population, which might be what some
government officials are hoping for.

8   CONCLUSION

Race was once thought to be a biological category much as sex is thought to
be; people classified as 'black' were thought to differ in their genetic makeup from
people classified as 'white' as males differ from females in having a Y chromosome.
However, studies of human genes and, more recently, DNA oppose any biological
conception of race. Nevertheless, since people continue to classify one another
by race and treat a person of one race differently than a person from another,
race remains a useful descriptive and analytic category in the social as well as
biomedical sciences, and scientists have a reason to continue to stratify populations
by race and use race to describe or explain how members differ in a socioeconomic
or biomedical trait [Root, 2003]. Social scientists have a reason not to be race-blind
as long as their subjects are race-conscious.

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FEMINIST ANTHROPOLOGY AND SOCIOLOGY: ISSUES FOR SOCIAL SCIENCE

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1 INTRODUCTION

There are a variety of ways that one could examine feminist contributions to anthropology and sociology within the context of a handbook on philosophy of science. The first and most obvious is to simply catalogue the various contributions that feminism has made to each of these disciplines, in part through the increasing presence of women in these fields. Sandra Harding refers to this approach as the "women worthies" or the "women's contributions" projects in a feminist science [1986]. These projects are respectively, rediscovering and honoring women who were forgotten contributors and cataloguing their efforts. As Harding notes, these projects, while worthwhile, do not fundamentally alter the nature of the disciplines in question and do not offer much from the perspective of philosophy of social science.

A second possibility would be to note the changes in content that feminism has worked in these two fields, and, indeed, there have been many such changes. Again using Harding's terminology, the "victimology" project of chronicling the various forms of neglect caused by androcentric science falls into this category. This approach is more germane to the question of how feminism affected the development of anthropology and sociology. Feminism filled gaps both in what was studied and the categories with which the disciplines organized the objects of study.[1] While interesting and transformative of these disciplines in many ways, the fundamental nature of the scientific enterprise in these fields was nonetheless not challenged through these critiques either.

These two approaches to the role of feminism in shaping anthropology and sociology deal only with the *addition* of women and women's concerns to an existing discipline, both when women are the scientists and when they and their lives become the object of study. Feminism offers something unique, revolutionary, or transforming for these social sciences only if it tackles the theoretical frameworks and methodologies that define these sciences. Judith Stacey and Barrie Thorne offer the following analysis in their "The Missing Feminist Revolution in Sociology". "The initial period is one of filling in gaps – correcting sexist biases and creating

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[1]For instance, gender was introduced as a relevant social category.

new topics out of women's experiences. Over time, however, feminists discover that many gaps were there for a reason, i.e., that existing paradigms systematically ignore or erase the significance of women's experiences and the organization of gender. This discovery... leads feminists to rethink the basic conceptual and theoretical frameworks of their respective fields" [1985, 302]. While content critiques are important for understanding both the development and status of feminism in these two fields, it is the constructive critiques, the critiques of the "conceptual and theoretical frameworks" of these social sciences that are most philosophically interesting. Questions feminists have raised about methodology, theory, and the nature of knowledge in their disciplines intersect more directly with contemporary debates in the philosophy of science and raise the question of whether a feminist successor science is necessary or even possible and if so what it would be like.

1.1   The scope of the discussion

Using the perspective of the philosophy of science both narrows and broadens the discussion of these social sciences. The narrowing results from examining the *philosophical issues* that arose as feminist anthropology and sociology evolved. The broadening comes from the fact that these philosophical issues run through all of the disciplines that intersect here: philosophy, feminism, anthropology, and sociology. Feminist methodology in sociology and anthropology has raised basic epistemological questions, such as what counts as evidence, what good evidence is, and in what sense these social sciences can be considered objective. These questions are contiguous with more general questions about feminism and science, feminism and knowledge. Though issues discussed here are explored in the context of feminist anthropology and sociology, the discussion is embedded in the broader discussion of the role of values in science and questions of knowledge in context. In addition to presenting an overview of how these debates have played out and looking at their current state, I will also consider ways in which these discussions can inform our understanding of the production of scientific knowledge more generally.

It is standard to identify at least two types of feminist projects in the philosophy of science ([Smith, 1987]; [Wylie, 1998]). As others have done, I refer to these as a "critical project" and a "constructive project".[2]   The critical project begins with the discovery that women, their lives and concerns, are absent both as social scientists and as objects of study. With the greater participation of women, there are changes in the disciplines. Women anthropologists, such as Elsie Clews Parsons (1874-1941), Phyllis Kaberry (1910-1977), and Audrey Richards (1899-1984), brought a different approach to studying culture and society that incorporated attention to women and their roles in a new way.[3]   Elsie Clew Parsons, for instance, focused on questions of gender and society in the early part of her career,

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[2]So, for instance, Dorothy Smith, in her introduction to *The Everyday World as Problematic: A Feminist Sociology*, makes this distinction in relation to her own work describing it as both "feminist critique and an alternative to standard sociology" [1987, 2].

[3]The social scientists mentioned here are intended as examples only, and is in no way an exhaustive list.

during which she worked primarily in sociology. In the second half of her career, she turned to anthropology and studied gender roles, including the phenomenon of gender crossing in Native American societies. Though these were not her only interests, her concern with gender roles marks her clearly as one of the earliest feminist anthropologists.

Sociology followed a similar path, of recognizing women and including them, both as researchers and as objects of research. This, in turn, led to the further recognition that not only were women missing, but the very categories that would have enabled us to see that they were missing were not available. This critique is deeper and more systematic. As Dorothy Smith puts it, "When we started this critical work, we did not realize how far and deep it would go" [1987, 1]. Recognizing gender as a significant category led to the realization that gender permeated the social world and that power was distributed along gender lines (in addition to race and socioeconomic status). Historically, these realizations coincided with a changing understanding of the nature of social scientific knowledge itself and an increasing awareness of the role of the social in knowledge production. Not surprisingly, the next round of criticisms was epistemological. Were the means of knowledge production adequate or appropriate for an understanding of the social world that would serve the interests of women? If not, what alternatives are available? It is here that the constructive project of feminist philosophy of science begins.

Much of the work of feminist sociologists, anthropologists, and epistemologists that I will discuss here has revolved around the constructive project. To take sociology as an example, Dorothy Smith proposed a sociology that "begins in the actualities of women's experience" [1992], raising questions about an alternate feminist methodology. This is a puzzling idea given that the traditional conception of scientific methodology is neutral relative to its subject matter. Some methods may work better than others in particular arenas for pragmatic reasons and there is debate about whether the same methods are appropriate to the physical and social world, but the idea that one needs to use a different methodology to discern the truth about men and women seems suspect. Why should knowledge for women be different than knowledge for men? Isn't knowledge neutral in this regard and really for everyone?

1.2   Feminist methodology?

The idea that there might be distinctive "women's ways of knowing" gained some support in the early 1980s. Carol Gilligan's influential [1982] book, *In a Different Voice: Psychological Theory and Women's Development*, suggested that women had alternative understandings of ethical questions. Also influential in many quarters was the book by that very name *Women's Ways of Knowing* [Belenky *et al.*, 1986]. This work was contiguous with the work of feminist sociologists and anthropologists during this period.

However, both feminists and philosophers of science (including feminist philosophers of science) had important objections to the idea that there was something distinctive, privileged or even fundamentally different about the way women know. Criticisms from at least two perspectives challenged any account of women's knowledge that identified it as distinctive.

First, feminists noted that the very diversity of women's experience mitigates against any project that requires a difference between women's ways of knowing and men's. The very idea presupposes a uniformity of women and their experience that feminists were coming to see did not exist. The notion that all women were some particular way with regard to knowledge (or anything else) evokes a "universal woman" as replacement for the "universal man" that feminism had rejected. In addition, there is the more general critique from those who reject the notion that the central features of science, methodology, or even reason itself could be gendered [Wylie, 1998].

A second roadblock to feminist methodology was that it appeared to duplicate the error of believing that method will provide the key to knowledge, that there is one right method for getting "real" knowledge. Related is the criticism that seeking the right method is looking for a technical fix for a problem of substance. A series of criticisms of the search for a feminist method appear in the late 1980s and early 1990s ([Wylie, 1992]; [Harding, 1987]; [Longino, 1987]), and with them the idea of feminist methodology was increasingly difficult to defend. But in part because of the development of a healthy tradition in feminist sociology and other social sciences (political science, and to some extent anthropology) by the late 1990s, the debate got a second wind in philosophy and has been more explicitly tied to the sorts of epistemological questions that are our concern.

A related issue that spans the critical and constructive feminist projects is the idea that feminist science should provide knowledge "for" women. If a sociology or anthropology "for women" is different from a sociology or anthropology for men or a gender-neutral sociology or anthropology (assuming that there could be such a thing), it would suggest that knowledge is relative to goals. This would mean that knowledge is not objective, in the sense of being value or interest free. Questions about objectivity therefore became a central focus of feminist discussions of methodology, and will provide the central theme of this chapter.

Though there are similarities in the issues and history of the two disciplines, they are different both in their focus and in their history. Feminist ideas attach themselves to different traditions in each and while they share a commitment to activism, a focus on the particularity of women's lived experience, and a concern about non-exploitative methodologies, the ability of feminists in each of these disciplines to address these issues is shaped by other factors in the discipline. I will treat them separately but in the process I will make comparisons and return to the question of what general lessons can be drawn for feminist science studies.

1.3   What makes a social science feminist?

Before turning to a discussion of feminist anthropology and sociology, it is important to address the general question: what makes a social science *feminist?* Alison Wylie identifies three features that seem to be common among social scientists that identify themselves as feminist. First, feminist social science has as an aim "to empower women by recovering the details of their experience and activities" [1992, 226]. Sociologists had failed to observe women, but more problematically the descriptive categories used by sociologists had failed to capture the relevant features of women's lives. So a feminist sociology should, in the first instance, avoid these two errors. One way to do this is to uncover the power structures that keep women in positions of subordination, although the best way to do this is open to debate. Wylie suggests that there may be a third requirement for feminist social science, "that problems addressed... are of concern to particular groups of women and useful to them" [1992, 237].

Examining the work of those who self-identify as feminist anthropologists and sociologists we can see three more specific issues and debates that have occupied their interest. The first is ethnomethodology/ethnography. The promise that some kinds of subjectivity lead to greater objectivity or more generally, better social science, through better understanding of those being studied has been addressed in both disciplines, though the way the discussion evolves in each is different. The second is the question of whether there is a particular sort of methodology that would be appropriate to feminist research. The most interesting contender for such a title has been standpoint theory or methodology. Standpoint theory is most closely identified with sociology, however, many who have discussed standpoint have argued that it can be seen as a methodological approach for all of the sciences in general [Harding, 1986]. What standpoint theory is, how it works as a scientific methodology and in what way it produces knowledge will be the primary focus of the following section on feminist sociology. Finally, there is a debate about quantitative vs. qualitative methodology and whether quantitative methods are inimical to feminist interests. In many ways, this is the least interesting of the debates because it is increasingly clear that quantitative approaches have been used effectively to support feminist goals. For example, there are quantitative studies that provide compelling evidence that countries with stronger women's rights or that provide equal education to girls tend to do better economically overall.[4] The increased use and success of quantitative methods in the social sciences goes beyond the scope of this chapter.

While the urge to find unifying features of feminist social science is both understandable and hard to resist, I would like to suggest an alternative way of thinking about what it is that supports grouping social scientists as feminists. Rather than looking for necessary and/or sufficient conditions or even "family resemblance", I propose that we think of feminist sociology (and probably "feminism" more gener-

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[4]Some examples of the sort of research I am referring to are Isobel Coleman [2004], M. Steven Fish [2002], and T. Paul Schulz [2002].

ally) as descriptive of a particular attitude or group of attitudes or what Bas van Fraassen has called a "stance" and idea he develops in trying to identify what it is to be an empiricist. Though there may be certain beliefs that empiricists have, it is not a commitment to any particular set of beliefs that makes one an empiricist but rather a commitment to a particular approach.[5] In the case of trying to identify what counts as feminist sociology, we run the risk of identifying feminism too closely with some particular feminist position. If we think of feminism as an attitude rather than a position or even a group of positions, we can avoid this problem.

In the case of feminism, the features of the attitude that we should focus on seem to me to be the following. First, feminist sociologists and anthropologists identify themselves as feminist. Second, that they identify themselves as feminists means minimally that they are conscious of gender as a relevant or at least, *potentially*, relevant category of analysis. Third, they see knowledge as knowledge for some purpose (knowledge for, by, and about women). Finally, the perspective that feminist social scientists seek to have is the perspective of women. This is not one perspective because, of course, there are many women and women's interests, goals, and desires will vary depending on features of their circumstances. This feature of feminist social science challenges the ideal of objectivity in science.

However, thinking about feminism as a stance rather than a position has the advantage of reshaping the discussion about objectivity. By thinking about feminism as a stance, we recognize that commitments may alter how an object is studied but there is no reason to think that they alter the object itself. The questions that are of interest in evaluating knowledge are questions about what one wants to do with our knowledge; in what ways can we use it to improve our lives? This approach is in the tradition of pragmatism. Rethinking objectivity suggests that it is not simply a question of a match between theory and the world, but rather a relationship between ourselves, theory, and world. This is one of the main lessons that comes from an examination of feminist anthropology and sociology.

2   FEMINIST EPISTEMOLOGY

Since Sandra Harding offered a three-part division of feminist epistemologies in *The Science Question in Feminism*, it has become standard when discussing work in this area to identify it as belonging to one of three approaches: feminist empiricism, standpoint epistemology, or postmodern feminist epistemology. Feminist empiricists are usually characterized as advocating a stricter adherence to empiricist principles as a means of eliminating androcentric bias in science. This approach is consistent with the idea that all that is necessary for an improved, nonsexist science is "adding women".

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[5]I have supported the idea that this is a better way to think of philosophical "positions" elsewhere. In the context of rethinking the realism/antirealism debate in science, I advocate that these "positions" be seen as attitudes or stances that are adopted locally by particular scientists for particular purposes [2000].

As we have seen in the discussion above, the more contemporary and radical forms of feminist sociology aim at more than this. It is not surprising then to see that if we employ Harding's characterization the more radical approaches in feminist sociology are standpoint and postmodern approaches. Though both postmodernism and standpoint methodologies have their advocates among feminist sociologists, standpoint has generated a body of interdisciplinary literature that is lively and recent.[6] As a constructive proposal, standpoint projects have been among the most successful, with a number of feminist sociologists either explicitly making use of standpoint or acknowledging a debt to it.[7]

That having been said, it is both unclear what should be identified as "standpoint theory" and the extent to which standpoint is viable as a project, theory, approach, or methodology for a feminist sociology. The questions that one might raise about it are the same questions that one might raise about a standpoint approach to science more generally. One of the most important of these is the general question about the feasibility of a science that is explicitly incorporates values. Shouldn't science be objective and aren't "impartial" and "value free" among the relevant meanings of "objective" in this regard? Standpoint approaches also explicitly call for a science that "begins in women's experience", as well as calling for a sociology "for women". What these descriptions mean is part of the ongoing controversy about standpoint, but minimally it advocates an approach to sociology that begins from a particular viewpoint.

In addition, if standpoint is a feminist standpoint, as some theorist say that it should be, then it is also explicitly political.[8] Concerns about a politicized science are raised most particularly in relation to the question of how a politicized science can possibly be a good science. Critics that express concern about these issues often raise the specter of Nazi science or Lysenkoism. This points to some of the one of the most controversial aspects of standpoint theory or feminist philosophy of science more generally. If the claim is that standpoint sociology is a better sociology, at least a better sociology for women, then it would seem to be incumbent on its advocates to explain how this is possible. How can a science be good if it is not objective? In what sense can a feminist sociology be a better sociology? There are a variety of ways of thinking about these issues, ranging from rejecting the connection between standpoint as a method and the deeper epistemological questions it would seem to raise to reconfiguring our thinking about scientific objectivity.

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[6]Sharlene Nagy Hesse-Biber and Michele L. Yaiser identify Chela Sandoval, Kum-Kum Bhavani, Adrien Katherine Wing, and Mari Matsuda as postmodernist feminists of in their collection *Feminist Perspectives on Social Research* [2004].

[7]Among these are Dorothy Smith, Shulamit Rheinharz, Patricia Hill Collins, Marjorie DeVault, Liz Stanley, and Sue Wise.

[8]Among the issues that advocates of standpoint differ over is the question of whether the standpoint that is to be adopted is that of women or of feminists. Dorothy Smith describes it as women's standpoint, whereas Nancy Hartsock calls it a feminist standpoint. The questions about objectivity rise in some form on either description.

2.1 Standpoint theory

One aspect of the controversy over standpoint has to do with its origins which are disputed and multiple. Because standpoint is closely identified with Dorothy Smith in sociology, I am going to use Smith's version as the starting point of the discussion. However, I will also explore questions about the possibility of extending some of the key insights that gave rise to standpoint approaches to a more general feminist philosophy of science, including the options that have been proposed, and the difficulties with it that are in the process of being negotiated.

Beginning in the late 1970s, Dorothy Smith developed a method that she described as "beginning from the standpoint of women". Smith's approach is political, though not as explicitly so as some of the other approaches that come from roughly the same period. For instance, Nancy Hartsock's approach should be in political science is more clearly a Marxist notion of standpoint. Since, for Smith, standpoint is a way of revealing, exploring, and ultimately transforming power relations, it is political in a more general sense. I will focus on Smith's version, though it is important to note that many of the misunderstandings that have haunted the discussion of have resulted from some features of Hartsock's approach specifically.[9]

Though Smith describes her approach in earlier work, it is her more recent defense and clarification of the approach that I will use as a basis for discussion. In 1992 in *Sociological Theory* and again in 1997 in *Signs*, she responds to critics and clarifies the history and original intention of the standpoint method with which she is identified.[10] In these responses, she recounts her original insight and experience that standpoint is an attempt to capture and that she believes was lost in its subsequent formalization by other thinkers.

> The experience, of course, was complex, individualized, various. It's hard to recall now at that time we did not even have a language for our experiences of oppression as women. But we shared a method. We learned in consciousness-raising groups, through the writings of other women..., in talk, and through an inner work that transformed our external and internal relationships. We explored *our experiences as women* with other women — not that we necessarily agreed or shared our experiences. ... Remaking sociology was a matter that arose out of practical demands. Established sociology distorted, turned things upside down, turned us into objects, wasn't much use. I thought that we could have a sociology by responding to people's lack of knowledge

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[9]Hartsock uses object relations theory as a means of identifying the origins of the differences in the viewpoints of men and women as they examine the world around them. This has resulted in her being labeled an essentialist. As we shall see, if standpoint requires an essentialist account of women, then it will not be viable for the purposes that feminists wish to employ it.

[10]The critics responded to in the [1992] article are Patricia Hill Collins, Robert Connell, and Charles Lemert. In 1997, she is commenting on Susan Hekman's "Truth and Method: Feminist Standpoint Theory Revisited".

of how our everyday worlds are hooked into and shaped by social rela-
tions, organization, and powers beyond the scope of direct experience.
[1992, 89]

From what Smith says here, it is clear that standpoint is intended to be more
than viewpoint or perspective. It is *achieved* out of discussions that take place,
with consciousness-raising mentioned as a model for such discussions. It involves
what is frequently referred to as "studying up", examining and learning about the
power relations and institutions that perpetuate oppression from the perspective of
the oppressed. Standpoint is not simply the result of being a woman or a member
of some non-dominant group, though being in that social position and embodied
as a woman is necessary in order to take the steps to achieve the standpoint. Since
it is from the located, embodied standpoint that researchers have their insights,
Smith claims that this is not a theoretical position, but an experiential one, and,
consequently, she refers to standpoint as a *method of inquiry*, not a theory. It is
a method that begins with the viewpoint of women but does not end there. The
projects for which this method is suitable take insights from the lives of women
and use them to give an account of social structure that reveals the power relations
that maintain the oppression of women. This is, at least in part, what it means to
have a sociology *for* women. It is a sociology that provides a way of understanding
what it is about the world in which they find themselves that perpetuates their
oppression. There is more to a sociology for women than this however. In addition
to explaining, there is the further purpose of transforming the social world using
this understanding. So standpoint is achieved, explanatory, and normative.

Smith rejects what she refers to as the "theorizing" of standpoint and its identi-
fication as a feminist epistemology [1997]. Since she refers to women's standpoint
rather than feminist standpoint, it was not her intention to promote a feminist
standpoint theory, such as Harding describes.[11] She notes that it is Sandra Hard-
ing's work that is responsible for the theorizing that she rejects. She clarifies',...
I am not proposing a *feminist standpoint* at all: taking up women's standpoint
as I have developed it is not at all the same thing and has nothing to do with
justifying feminist knowledge" [[1997] 2003, 264]. She also states, "For me, then,
the standpoint of women locates a place to begin inquiry before things have shifted
upwards into the transcendent subject" [1992, 90].

Smith asserts that she is not interested in epistemological issues and does not
want to engage in discussions about objectivity. She avoids this engagement by
focusing on standpoint as a *method* and in doing so evokes the distinction between
a context of discovery and a context of justification. Standpoint is not about issues
of justification, in her opinion, but of discovery.

Recent work in the philosophy of science, however, has raised questions about
the relationship between values and science in such a way as to throw doubt on

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[11]In fairness to Harding, when she identified the three types of feminist epistemology she did
not claim that Smith was an advocate of feminist standpoint but rather used the awareness that
she had of Smith's work as one of a group of related kinds of approaches that she generalized in
this way.

whether the distinction between a context of discovery and context of justification is viable. The closely allied distinction between cognitive and noncognitive values has also been called into question. The connection between the two distinctions is that traditionally it has been argued that if values do enter into science, they do so in the context of discovery through the presence of noncognitive values. Such noncognitive values would include social values, such as the differences in what is valued by men and women, oppressor and the oppressed. These social values would show up when one focuses on the sociological starting point of the particular and ordinary experiences of women, i.e, from women's standpoint.

So if we are to consider the question of whether standpoint or any other feminist approach is going to subvert sociology through the introduction of "illegitimate" values or the "illegitimate" use of values, we must first be clear that there are such illegitimate values and that they can in fact be identified. If there is a legitimate distinction between cognitive and noncognitive values, then the cognitive values are those that should be used in the justification of our theories (the context of justification) whereas the noncognitive values, though they may still play some role in the process of knowledge production (in the context of discovery, perhaps) do not have a legitimate role in justification. So when Smith says that her account is not a theory of justification, not an epistemology, and not presented as a theory, she seems to be operating within the framework of a philosophy of science that finds this distinction legitimate.

In a recent article, Sandra Harding affirms that what standpoint calls for is a discussion of the context of discovery. "While many philosophers do not discuss the context of discovery, standpoint theorists think that such discussion is essential" [2004b, 32]. Harding claims that such theorizing can occur as part of a "logic of discovery" and that it is a legitimate arena for philosophy of science. Her point is that philosophy of science needs to include examination of the "context of discovery" in addition to the "context of justification". But both the utility of distinguishing the contexts and viability of the distinction are questionable. The traditional connotations associated with the distinction may be misleading and a complete rethinking of the production of scientific knowledge may be more useful.

While it is widely acknowledged that values enter into scientific reasoning, it has traditionally been argued that they nonetheless do not affect the objectivity of science.[12] There are two parts to this argument. The first is to draw a distinction between cognitive/epistemic and noncognitive/nonepistemic values. Cognitive or epistemic values are those that we value for their ability to enable us to make correct judgments.[13] So, for instance, the predictive or explanatory power of a theory might be an example of a cognitive/epistemic value. These are sometimes thought to be "truth-preserving" values that are appropriate to the context of

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[12] "Most people today agree that values enter into science — some values, somehow, somewhere. Few people, if any, still uphold the notion that science in all its aspects is a value-free endeavor." [Machamer and Walters, 2004, 1]

[13] Sometimes these judgments are claimed to be true, truth-like, or empirically adequate. I am using "correct" as a more neutral term.

justification. Noncognitive or nonepistemic values, on the other hand, are thought to be social, cultural, contextual values that are present in the process of knowledge production but do not threaten objectivity if they are confined to the context of discovery.[14] So the acknowledgment of values in science is claimed not to threaten the objectivity if both 1) we can distinguish between these two sorts of values; and 2) we can distinguish between the context of justification and the context of discovery.

However, a number of writers have recently noted that the distinction cannot be sharply drawn when we are examining criteria of theory acceptance, or more radically, have claimed that we cannot really make the distinction at all.[15] Larry Laudan urges a further investigation into the relationship between the cognitive and the social [2004] and Hugh Lacey questions the distinction between a context of discovery and context of justification [2004].[16] Helen Longino notes that the distinction reflects what she believes to be an untenable dichotomy between the rational and the social, where the cognitive values have been identified with the rational and the non-cognitive with the social [2002]. Less radically, Miriam Solomon suggests a complete analysis of how theory choice is achieved needs examination of all the "vectors", social and other that are relevant in a particular case [2001]. Even if it were possible to resolve these issues and make the distinction, Smith's insistence that standpoint as she has conceived it is not about epistemological matters is misleading. Although standpoint may have been originally proposed as a methodology, methodology and epistemology are intertwined and the putative value of the approach cannot be understood without addressing the underlying epistemological issues.

2.2   Standpoint criticized and clarified

In 1997, the journal *Signs* published an article by Susan Hekman along with responses from several whose work she had critiqued. Hekman argued that feminism's early flurry of interest in standpoint, which she dates to the publication of Nancy Hartsock's [1983] *Sex, Money and Power*, died down as it appeared that there were untenable presuppositions underlying the approach. She identifies two that are particularly problematic: that standpoint demands that some viewpoints, i.e., those of women, are epistemically privileged; that it does not acknowledge the diversity of women, essentializing woman and replacing the universal man of modernism with a universal woman. However, Hekman argues that standpoint is

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[14]Philip Kitcher affirms a version of this account in Chapter 3 of his 2001 *Science, Truth and Democracy*, concluding that the "ideal of objectivity need not be dismissed as a fond delusion [41]".

[15]See Peter Machamer and Gereon Walters (eds.), *Science, Values, and Objectivity*, Pittsburgh: University of Pittsburgh Press, 2004.

[16]Lacey thinks that social values should be "kept in their place", however in his account he proposes that we think of science in terms of three "moments" rather than two contexts. These are adopting a strategy, accepting theories, and applying knowledge and they do not break down neatly into the former classification.

nonetheless promising and should be reconfigured and reaffirmed in a way that would no longer make it dependent on these assumptions.

Hekman's account makes claims about standpoint that appear to be false. First, it does not seem that there was ever any assumption on the part of advocates of standpoint that women were automatically epistemically privileged. Both Smith's claims that standpoint is a *starting place* for inquiry, a place from which to gather evidence and that standpoint needs to be *achieved* would seem to count against this idea. Furthermore, those who have used standpoint, including Smith, have always been sensitive to the diversity of women and have not treated women as a homogenous group. The prominence of such social theorists as Patricia Hill Collins and Uma Narayan among those who think through race, class, and other diversities using standpoint would seem to indicate clearly that there is nothing in standpoint *per se* that precludes the recognition of multiple standpoints. Collins [1986] for instance, advocates the use of a standpoint to analyze race and class in addition to, and in combination with, gender. In describing the ways in which Afro-American women might have insights that other sociologist might not, for instance, Collins says "traditional sociological insiders, whether white males or their non-white and/or female disciples, are certainly in no position to notice the specific anomalies apparent to Afro-American women, because these same sociological insiders produced them. In contrast, those Black women who remain rooted in their own experiences as Black women – and who master sociological paradigms yet retain a critical posture toward them — are in a better position to bring a special perspective not only to the study of Black women, but to some of the fundamental issues facing sociology itself" ([1986, S29]; [2004a, 121]). Collins thus advocates maintaining the standpoint of a Black woman while working as a sociologist and in doing so points out that such a standpoint requires a recognition of diversity among women. The outsider/insider *may* be capable of seeing things that other sociologists cannot but is not assured the ability to do so merely in terms of his or her social location.

Donna Haraway also develops a standpoint project that calls for the acknowledgment of a myriad of standpoints.[17] She reminds us that each of us has experience that is multi-dimensional and so provides us with the means of occupying more than one standpoint. "The topography of subjectivity is multi-dimensional; so, therefore, is vision. The knowing self is partial in all its guises, never finished, whole, simply there and original; it is always constructed and stitched together imperfectly, and *therefore* able to join with another, to see together without claiming

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[17]Haraway's work sometimes seems more appropriately described as standpoint and sometimes as postmodernist. Her concerns about reconstructing a notion of objectivity, even while acknowledging the necessity of awareness of diversity of viewpoints, would seem to make her less postmodernist than standpoint theorist. She also uses what is clearly a standpoint position from which to develop her account of primatology. However, she also differs from feminist empiricists and some other standpoint theorists in rejecting the dichotomy between relativism and objectivity. As she sees, they are both linked to the "god trick", objectivity as the "view from nowhere" and relativism as the "view from everywhere", which is nowhere as well [1988].

to be another" [1999, 179].[18]

Much of Hekman's criticism seems therefore, to be based on misreading or misunderstanding, though it must be acknowledged that this sort of misunderstanding is fairly common. The notion that standpoint advocates automatic privilege is present in a number of critiques of feminist thought including those of Susan Haack, Noretta Koertge, and Cassandra Pinnick.[19]   Though Hekman's criticisms may be off the mark, the responses they have elicited are clarifying. As Alison Wylie points out,

> Whatever form standpoint theory takes, if it is to be viable it must not imply or assume two distinctive theses with which it is often associated: *First*, standpoint theory must not presuppose an *essentialist* definition of the social categories or collectivities in terms of which epistemically relevant standpoints are characterized. *Second*, it must not be aligned with a thesis of *automatic epistemic privilege*; standpoint theorists cannot claim that those who occupy particular standpoints (usually subdominant, oppressed, marginal standpoints) automatically know more, or know better, by virtue of their social, political location [2004, 341].

Sandra Harding identifies four related features of standpoint that distinguish it from perspectivalism, with which she claims it is often confused. It "intends to map the practices of power, the ways the dominant institutions and their conceptual frameworks create and maintain oppressive social relations. Secondly, it does this by locating, in a material and political disadvantage or form of oppression, a distinctive insight about how a hierarchical social structure works. ...Third, the perspectives of the oppressed cannot be automatically privileged.... Finally, standpoint theory is more about the creation of groups' consciousness than about shifts in the consciousness of individuals" [2004b, 31-32]. If we understand standpoint in this way some, though perhaps not all, of the worries that have been generated by the approach are eliminated.[20]

There is a second way that Hekman's analysis is off the mark. She claims that after the early 1990s interest in standpoint flagged, in part because of its association with the undesirable views noted above. Though it may be the case that there were not many new advocates for standpoint among philosophers, a number of sociologists continued to work in this tradition. Marjorie DeVault's *Liberating*

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[18]I have quoted here from the reprint in Biagioli which differs slightly from the original [1988] article which reads "Subjectivity is multi-dimensional; so, therefore, is vision" [1988, 586].

[19]These critiques make compelling arguments against "straw women" and betray a misunderstanding of much of the more radical feminist science studies and frequently conflate feminist standpoint theories with postmodernists theories. See Pinnick, C., N. Koertge, and R. Almeder, *Scrutinizing Feminist Epistemology* [2003].

[20]However, Harding claims that those controversies that are both good for standpoint and for philosophy of science more generally in that they bring an array of groups into conversation and bring into focus the ways in which philosophy of science can be socially relevant [2004b].

Method [1999] and Sharlene Nagy Hesse-Biber and Michelle L. Yaiser's edited collection, *Feminist Perspectives on Social Research* [2004] address epistemological issues and feature philosophical engagement with feminist methodology prominently. Throughout the 1990s, there was a continuing discussion about feminist methodology, including, but not limited to, standpoint theory. So the idea that this is primarily an epistemological debate taking place among philosophers may have led Hekman astray in this regard. The vibrancy of the work in this area, the growing reflection on what it is about standpoint and other approaches that might be considered feminist, as well as the growing interdisciplinary interest in these discussions have been important factors in the revival of interest in standpoint among feminist philosophers.

In spite of these failings, Hekman does focus on the important issue of objectivity, although she is not original in doing so. As the discussion of standpoint or any other feminist methodology turns to epistemology, the primary question from the perspective of the philosophy of science is whether the sociology produced by these methods is good science or better science than sociology that does not use these methods. Finding a way to answer the question of what makes standpoint an improvement over "traditional" science still remains to be done. However, as Hekman and other have pointed out, without some standard of what counts as good science, without some criterion for making this judgment, this key feminist claim cannot amount to anything. So what can we say about whether standpoint offers an improvement in sociology and how can it be said? If the questions about standpoint can be relegated to methodology, something distinct from epistemology as Smith seems to hope, or if we can confine them to the logic of discovery then perhaps they will not need to be addressed. As I have already intimated however, such a defense would require the ability to distinguish the cognitive/epistemic from noncognitive/nonepistemic and it is not straightforward how this can be achieved. In addition, there are reasons for believing that we may get a clearer picture of the practice of producing scientific knowledge if we do not cling to this distinction.

2.3 Objectivity

Two of the clearest attempts to address these are those of Sandra Harding and Alison Wylie. However, before I turn to their work, I want to clarify the question. Feminism is clearly political and so not value-free. To argue that a feminist science is a better science appears to challenge the idea that objectivity is a virtue for science, if we take the notion of objectivity to mean something like, value-free, impartial, unbiased, or free from political influence. So on the face of it, there is a conflict between the traditional conception of good science and the one proposed by feminism. It is possible of course that objectivity should just be abandoned as a goal and there are those that have claimed that this is what feminists are advocating. At least among feminist philosophers of science, there do not seem to be any who explicitly make this claim, but it is not surprising that this perception persists. Consider the following from *The Gender and Science Reader*:

"Scientists, in general, believe that their work is beyond cultural or social influence – that they are discovering, rather than inventing, Nature. This perception has permeated the general population such that it is difficult to convince people that science is not objective truth. Moreover, it is the self-proclaimed 'objectivity' of science, along with its elitist, gendered and racist stances (whether overt, covert, or unintentional) that create friction with social studies of science. Scientists should become aware of analyses of their disciplines based on class and race" [Lederman and Bartsch, 2001, 2–3]. If objectivity of some sort is not a goal, then how are we to adjudicate competing claims about what counts as better science? In what sense can it be claimed that feminism improves science?[21] Worse still, how do we evaluate the very basis of feminist theorizing? As Hekman puts it in relation to standpoint theory specifically, "Feminist standpoint theory raises a central and unavoidable question for feminist theory: How do we justify the truth of the feminist claim that women have been and are oppressed?" [2004, 226].

What objectivity is and how it might be threatened needs to be explored if we are to reconfigure, rehabilitate, or even reject an ideal of objectivity. Its ambiguity has been acknowledged and explored in several recent discussions on this issue. Elizabeth Lloyd [1995], Hugh Lacey [1999], Heather Douglas [2004], and Marianne Janack [2002] provide examples of some of the best attempts. Janack's categorization of varieties of objectivity is one of the most complete and I have reproduce it here:

1. objectivity as value neutrality;

2. objectivity as lack of bias, with bias understood as including:

   (a) personal attachment;
   (b) political aims;
   (c) ideological commitments;
   (d) preferences;
   (e) desires;
   (f) interests;
   (g) emotion.

3. objectivity as scientific method;

4. objectivity as rationality;

5. objectivity as an attitude of 'psychological distance';

6. objectivity as 'world-directedness';

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[21]It could be that the claim means no more than that it improves science *for women*, in the sense that it addresses those things that are important to them, but even this claim seems to be on shaky ground without a more robust concept of objectivity.

7. objectivity as impersonality;

8. objectivity as impartiality;

9. objectivity as having to do with facts;

10. objectivity as having to do with things as they are in themselves; objectivity as universality;

11. objectivity as disinterestedness;

12. objectivity as commensurability;

13. objectivity as intersubjective agreement. [2002, 275]

Which of these is the ideal of objectivity that a commitment to feminist science, and consequently feminist sociology, challenges or does it challenge all of them in some way? Which are worth retaining and which not? How are they related to each other?

Notice that in Janack's list there are three main senses of objectivity having to do with 1) the attitude of the subject; 2) method; and 3) the relationship between the belief/knowledge/theory and the world. The sense having to do with the subject is spelled out in 2, 5, 7, 8, and 11.[22] Only 3 is unambiguously methodological. The relationship between the world and the theory appears in 6, 9, 10, and 12, though arguably 6 can also be seen as descriptive of the subject and 12 might be commensurability among subjects and so more akin to 13. 13, intersubjectivity, is often used as an attempt to span all three senses of objectivity, for it can mean intersubjectivity about method or agreement about the world and possibly both. Finally, that these are all senses in which we use the term objectivity suggests that there is a relationship between them, that they all work together in some sense to produce *all* of what we mean by "objectivity".[23]

For our purposes, the most important connections are between objectivity as value-neutrality, disinterestedness, and impartiality and objectivity as scientific method. It is the lack of value-neutrality, disinterestedness, or impartiality in method that worries critics of standpoint. So the crucial question in the context of the discussion of standpoint is whether or not it is possible to engage in a method that is partial, perspectival, particular and consequently, value laden and interested but which nonetheless produces objective knowledge. Can standpoint produce knowledge that can be judged "objectively" better than knowledge produced in other ways and if, so, how?

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[22]1 and 4 are ambiguous between subject and method. Is it that the knower is rational of that the method that is being used is a rational method? Typically it is both and this captures some of the inelimnable ambiguity in the term.

[23]Though Douglas uses different categories, this is what she intends by her claim that the complexity of "objectivity" is irreducible.

2.4   Sandra Harding: strong objectivity

In her [1993] article "Rethinking Standpoint Epistemology: What is 'Strong Objectivity'?" Sandra Harding elaborates the account of objectivity that she believes a feminist philosophy of science should employ. She calls it "strong objectivity", and contrasts it with what she refers to as "objectivism", a kind of objectivity that demands disinterestedness, impartiality, impersonality, and is value-free.[24] According to Harding, critics of standpoint theory "have assimilated standpoint claims either to objectivism or some kind of conventional foundationalism or to ethnocentrism, relativism, or phenomenological approaches in philosophy or the social sciences" [2004, 127].

The thinking has been that either one has a foundationalist epistemology that grounds our claims through some appropriate methodology and fundamental knowledge (empiricism, for instance) or we are pushed into some sort of relativism because of the dependence of our judgments on "subjective" elements like interests and values. The first horn of the dilemma spawns criticisms that identify standpoint as advocating the epistemic privilege of women (or other outsiders). The other horn abandons the possibility of knowledge with relativism leading to skepticism.

The only option would seem to be objectivism, the view that Harding identifies with a traditional account of objectivity. Objectivism is the idea that science must be objective in the sense relating to the subject and capture the nature of reality through a value-free or, at least, value-neutral methodology. The problem with objectivism is that it is insensitive to the reality that values inevitably play a role in science and does not enable us to distinguish when values play a positive role and when they do not. "Objectivists claim that objectivity requires the elimination of all social values and interests from the research process and the results of research. It is clear, however, that not all social values and interests have the same bad effects upon the results of research. Democracy-advancing values have systematically generated less partial and distorted beliefs than others" [2004, 137]. Harding proposes an alternative to objectivism, a version of objectivity that she calls "strong objectivity". Strong objectivity requires that we determine which values and interests are less likely to distort beliefs, not that we purge our beliefs of values and interests, a task that would be impossible according to Harding.[25]

Standpoint theories provide a means of improving on objectivism through providing strong objectivity. They do this by focusing on the subject as crucial to knowledge, explicitly identifying the subject and reflecting on the role the subject plays in knowledge production.

> Strong objectivity requires that the subject of knowledge be placed
> on the same critical, causal plane as the objects of knowledge. Thus

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[24]This account is first developed fully in her *Whose Science? Whose Knowledge?*

[25]There seems to be a discrepancy between Harding's account of strong objectivity which suggests that values should be incorporated as part of an epistemology, or account of justification in her [1991] book and the recent [2004b] article calling for a logic of discovery. I am not really sure how they are to be reconciled and I have not addressed that conflict here.

strong objectivity requires what we can think of as 'strong reflexivity'. This is because culturewide (or nearly culturewide) beliefs function as evidence at every stage in scientific inquiry: in the selection of problems, the formation of hypotheses, the design of research, (including the organization of research communities), the collection of data, the interpretation and sorting of data, decisions about when to stop research, the way results of research are reported, and so on. [2004a, 136]

With that awareness, we can assess which communities of subjects and which values they hold produce better knowledge. Better knowledge comes not by eliminating the subjective (beliefs and values) altogether in order to conform to some false ideal of objectivism, but through examining whether understanding improves when these new viewpoints are incorporated. Harding makes it clear that it is not just the viewpoints of *some* women or *some* groups that are to be considered — she advocates for a *plurality* of standpoints. Though it is true that standpoint requires us to acknowledge a sort of relativism, the relativism is sociological; an acknowledgment of the differences in the ways that various groups see the world. However, the question of whether seeing the world from the perspective of one group rather than another enables them to achieve their goals and so gives a successful account of the world can be evaluated according to strong objectivity. As Harding puts it, "Standpoint theory provides arguments for the claims that some social situations are scientifically better than others as places from which to start off knowledge projects, and those arguments must be defeated if the charge of relativism is to gain plausibility" [2004a, 131].

Put differently, because the subjects are themselves so intimately a part of knowledge production, the means by which it comes about must itself be studied. "... [A] maximally critical study of scientists and their communities can be done only from the perspective of those whose lives have been marginalized by such communities. Thus strong objectivity requires that scientists and their communities be integrated into democracy-advancing projects for scientific and epistemological reasons as well as moral and political ones" [2004a, 136]. Strong objectivity not only requires, but generates standpoint.

Harding's account of strong objectivity has not been widely embraced. For one thing, it raises as many questions as it sets out to solve. By what standards are we to determine which values are more conducive to good science than others? This is taken as obvious in Harding's account, though she gives as her primary example that those values aligned with democratic ideals have been shown to result in better science than those that are not. There are few that would dispute this, nonetheless understanding why it is so is problematic. What is it about democratic values that leads to the greater success of a science that incorporates them? Other philosophers of science have attempted to answer this question. So, for instance, Helen Longino has claimed that an egalitarian society that supports discussion incorporating many diverse voices is more likely to produce good science than more closed societies. Though Harding neither gives a similar account, nor

explicitly endorses Longino's, it would seem that this is the sort of account that she has in mind. Another option is that we can think of scientific method as a sort of bootstrapping procedure, where we have standards by which we judge whether theories are good and when something doesn't work we revise both the belief judged and the standards by which we judge. These suggestions point toward a more complete account of strong objectivity, however they would need to be worked through to provide a satisfactory account.

2.5   Alison Wylie: rehabilitating objectivity

In a series of papers, beginning in the late 1990s, Alison Wylie has also grappled with the question of how to understand what is valuable in standpoint. Whereas Harding attempts to straddle the postmodernist/modernist divide, to have it "both ways", as she herself puts it, Wylie's understanding of standpoint connects it more directly with traditional standards of theory assessment in philosophy of science. She too seeks an account of objectivity that makes sense out of the successes of feminist methodology, and though she acknowledges that the concept is problematic rather than abandon it altogether she proposes a reconstruction. She argues that it "may be useful in showing what a standpoint theorist can claim about epistemic advantage without embracing essentialism or an automatic privilege thesis" [2004, 344-345].

Wylie notes that Hekman (among others) uses "objectivity" as a property of knowledge claims. This is objectivity as the relationship between belief, theory, or knowledge and the world. As already noted, this sense of objectivity is generally thought to be closely allied with, or perhaps dependent on objectivity in the sense of method. Wylie identifies a list of properties that are thought to be epistemic virtues indicative of the appropriate relationship between theory and the world. She justifies her list by noting that such properties are standard and agreed upon by "authors as diverse as Kuhn, Longino, Dupre, and Ereshefsky" [345]. The properties she is thinking of are empirical adequacy, explanatory power, internal coherence, consistency with other established bodies of knowledge, and perhaps some other virtues. She is not that concerned about precisely which virtues are on this list, and there might be disputes over one or more of the proposed properties and the degree to which it is important. But her point is that such a list could be compiled. She notes that empirical adequacy stands out from the others in that it appears on all such lists, though "empirical adequacy" is ambiguous as there are at least two ways in which it can be construed. It is either "fidelity to a rich body of localized evidence (empirical depth), or ... a capacity to 'travel' (Haraway) such that the claims in question can be extended to a range of domains or applications (empirical breadth)" (345). I will return to the significance of this ambiguity.

The other sense of "objectivity" that is relevant to this discussion is objectivity as distinterestedness, impartiality, or lack of bias. As previously mentioned, it is these two senses of objectivity that seem to come into conflict most clearly when one adopts standpoint theory as a methodology. Traditionally it is thought that

objectivity of the first sort is to be achieved by objectivity of the second sort: we achieve objective knowledge by adopting objective approaches to the world, eliminating bias through the appropriate methodologies. Standpoint undercuts this conception of knowledge production.

Wylie notes that there is a *prima facie* conflict between standpoint and objectivity in the first sense, although this list of properties cannot all be maximized at the same time. Which of these properties we are likely to prefer and so choose to maximize is likely to be dependent upon our interests, purposes, intentions, and goals. Even with empirical adequacy, which seems to have some primacy in virtue of being on all lists, trade offs exist. The two senses of empirical adequacy, breadth and depth, frequently conflict and sometimes other properties will conflict with empirical adequacy.[26] Once we begin to think of objectivity as conforming to some aspects of this list of virtues, it is also clear that certain features on this list could be more useful to maximize than others depending on standpoint. In fact, we can see that standpoint might even become a factor in *increasing* objectivity by throwing light on the sorts of empirical adequacy, explanatory power, or other virtues that are relevant for a particular project.

With this analysis, Wylie discusses a variety of virtues that have been claimed for standpoint theory in relation to the possibility that they might increase a theory's success at achieving the epistemic virtues we identify with objectivity in the sense of "getting at the facts". So, for instance, standpoint theorists have claimed an epistemic advantage for those in positions of subordination. Among the advantages claimed are access to evidence, special inferential heuristics, interpretative and explanatory hypotheses that may not be available to others, and "critical dissociation from the taken-for-granteds that underpin authoritative forms of knowledge" [346]. There is nothing automatic either about the epistemic advantages that might accrue to these disadvantaged groups, nor is there any assurance that the sorts of epistemic advantage that they have will increase the objectivity of science. That remains to be seen in specific instances after considering the purposes for which the knowledge is required. So Wylie affirms standpoint theory as a further resource for those who are engaged in science studies and attempting to understand the nature of scientific knowledge. She argues that by looking at objectivity as meeting some set of virtues from a standard list in addition to serving the goals of feminists, we can see why it is that standpoint might be able to contribute to this objectivity project.

What Wylie is saying about objectivity could be a version of Sandra Harding's strong objectivity. She agrees that the standards of objectivity should be applied to the methods and values shaping the scientific activity itself, but she disputes that we will get only one answer when we ask if the theory is good according to these criteria. One of her key insights is the importance of the interests and the goals that we have in shaping what it is that we accept as knowledge. This

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[26]For example, we use Newtonian physics which is less accurate rather than Einsteinian physics for many calculations because the less accurate but simpler calculations are pragmatically adequate.

approach is contextual and pragmatic. However it is also governed by agreed upon standards that are to be maximized in some combination and to some degree. Which standards should be maximized in this case and to what degree emerges as the knowledge project evolves.

Just as in Harding's work, we find ourselves faced with the question of the role of values in science. Wylie does not distinguish a context of discovery and a context of justification. The questions of justification are determined in the light of the contextual values, including those explicitly connected with the standpoint through which we are viewing the theory. These values do not determine directly whether the theory is one that we should accept, but they do determine which of the epistemic virtues are the most important for the current context. For Wylie the context of justification and the context of discovery merge (the justification always takes place within a context that also defines discovery). This is reminiscent of Longino's account of how contextual values play a role in the determination of evidence. One of the key differences is Wylie's examination of how standpoint can improve science by providing epistemic privilege (not automatic privilege but achieved privilege) and turning up evidence that might otherwise not be seen.

Both Harding and Wylie propose accounts of objectivity that provide a means of explaining how it is that standpoint might improve science and enrich our understanding of science. Both accounts require a kind of bootstrapping approach, where we sort out standards as we go based on success or failures that we have with our theories in particular situations. Wylie's account is more explicitly contextualized, however, there is no reason to think that Harding's views are in anyway inconsistent with this contextualization.

Wylie's account has two elements that pull against each other and each seems worrying. She starts with the "standard list" of epistemic virtues and so is using an empiricist approach, which, while comforting in its familiarity, is not unproblematic. Nearly all of the features on her list have been challenged at one time or another. Although she is not proposing necessary and sufficient conditions for choosing the best theory with these criteria, even the requirement that some, empirical adequacy, for instance, need to be met requires defense. It is also clear that empirical adequacy is not sufficient to produce a robust account of theory choice. Underdetermination arguments show this much at least. Furthermore, her account requires that we are able to distinguish the cognitive/epistemic from the non-cognitive/nonepistemic values, since the list of virtues is defined by what cognitive/epistemic properties are valued. In any given situation, it may be possible to do this; however, to give a generalized account is harder. Why should we treat these as epistemic values when it is so clear that they vary within the contexts? In what sense then are they to be identified as epistemic values?

The other criticism has to do with her inclusion of empirical adequacy on this list. Since she treats empirical adequacy as one of the epistemic virtues that will be used in theory choice, she does not single it out as having a different status than the others, except to note that it is the one that appears on everyone's list. By allowing for the possibility that empirical adequacy is not to be a hallmark of

any theory that is to be accepted she gives up a sense of objectivity that seems crucial to prevent relativism. The principles in each of these accounts maintain some notion of objectivity while at the same time being explicit about the ways in which values can legitimately enter into theory choice. But Harding's and Wylie's versions raise questions that motivate a further discussion of objectivity.

3   OBJECTIVITY: AN ALTERNATIVE ACCOUNT

Helen Longino has noted that the semantic view or the model-theoretic account of theories may provide resources for feminism.[27] I agree, but only if the view makes use of its difference from traditional empiricist accounts. The account that I sketch below provides insight into the success of standpoint theory, as do Harding's and Wylie's, but also offers a notion of objectivity that permits feminists to make the sorts of claims about feminist science that they believe are warranted.

Thinking about science in terms of models is an approach that has been advocated by Patrick Suppes, Bas van Fraassen, Ron Giere, Nancy Cartwright, and Margaret Morrison, among others. Van Fraassen describes scientific theories as families of models [1980]. Cartwright claims that models don't represent but rather "models mediate between theory and the world" [1999, 179]. Giere calls models, "the primary representational entities in science" [1999, 5] and Peter Godfrey-Smith claims "a model is a structure (either abstract or concrete) that is used to represent some other system" [2003, 238]. I will use an understanding of "model" akin to that of Cartwright as a starting point, and I will not tackle the debate about representation head on at the moment. For Cartwright, when models represent, it is only one of the things they do and not necessarily the most important thing that we should be focused on. A model should be seen as a tool rather than a method of representation, so when it represents it does so in order to achieve some other aim, not as its primary goal.[28]

The philosophers mentioned above also give different accounts of what sorts of entities models are. Giere has a very broad conception and one that works fairly well. A model can be a mathematical model, a physical scale model, a diagram, or even a conceptual model. In fact, there is probably not a good, non-trivial, general characterization that can be given of the concept, but what is interesting and useful is the analysis of specific models constructed for specific purposes.[29]

As Margaret Morrison puts it, models are useful for understanding the way

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[27]In *The Fate of Knowledge* [2002], Longino advocates that we try a model-theoretic approach, but there is very little in the book that makes direct use of what I think the approach offers by way of resources for thinking about knowledge.

[28]This is really an issue about the aims of science. I reject the idea that there is one uniform aim of science and that it is to describe the features of the world and that it does this through linguistic representation.

[29]For instance, Cartwright, Morrison, and Morgan [1999] have been involved in a project to analyze economic models. Ursula Klein has looked at a change in the use of equations in chemistry and has treated these equations as models, or "paper tools". Another approach looks at computational models in chemistry and other sciences.

that science works "because models have a rather hybrid nature (neither theory nor simple descriptions of the world)... they are able to mediate between the theory and the world and intervene in both domains" [1999, 44-45]. The model is a way of interacting with the world and thus knowing it. We adjust our models in response to the world (empirical phenomena) and we use our models to intervene in the world. Models are tools for knowing as well as being the way in which we know.

How do we get from models to a conception of objectivity that will rescue scientific knowledge from relativism and/or skepticism and yet still show how values, including feminist values, play an intrinsic role in science? The key to answering this question comes from thinking about what it is that models enable us to know. Lorraine Daston and Peter Galison offer the following observation: "All sciences must deal with this problem of selecting and constituting 'working objects,' as opposed to the too plentiful and too various natural objects" [1992, 85]. Daston and Galison similarly suggest a modeling of what they call the "working objects", but what I will call the "objects of scientific knowledge". In order to do science, we first need to make choices about what features of the multivarious natural world we are going to focus on and in doing this we "construct" the objects of scientific knowledge.[30]

But what makes us pick out some features of the world as part of our scientific objects and ignore others? We pick characteristics that we think are going to enable us to answer the questions that we have before us at the time. These questions are expressions of our interests and it is through those interests that we emphasize or focus on certain aspects of the world rather than others. The question of whether the features that we are focusing on are the "right" ones is an empirical question. Are those the features that will allow us to construct models that give us answers to our questions, enable us to do what we want to do in the world, and successfully meet our goals? The construction of the model and the construction of the scientific objects proceeds in tandem and over time. We might chose certain features thinking that they are salient and discover that they are not and so revise our model. Our model might work until our interests shift and then we find that we need to rethink the objects. The use of interests in constructing models fits with standpoint theory. Standpoint calls for an explicit awareness of these interests as they are used in constructing our model of reality and legitimizes their role.[31]

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[30]This is not to say that we construct the world. The scientific objects are not isomorphic with objects in the world.

[31]Though this account bears some similarity to Giere's "perspectival realism", Giere gives a realist account of science using models. However, it is clear that there is nothing inherent in the model-theoretic approach that requires a commitment to realism. Van Fraassen uses the approach and is the arch anti-realist. I am depending on realism for what I want to say about objectivity.

3.1   Values and interests

What we value directs our actions or at least we try to get it to direct our actions. What does modeling have to do with values? Models are tools and tools are for some specific purpose. Modeling requires making choices about the natural world; we focus on the features of the natural world that we believe are salient to what we want. As a result, what we value is an integral part of the construction of the model. In this way, values are intrinsic to science and to our negotiating our way through the world in general. This is why it is so difficult to make a global distinction between epistemic/non-epistemic values, cognitive/non-cognitive values, or cognitive/constituitive values.[32] If values enter into the very conception of objects, if they shape the "scientific object" of study, then they are not add-ons, or extra-scientific, social factors that enter into our judgments about which among many empirically adequate theories we ought to accept. They are an intrinsic part of knowledge production and need to be identified and examined as such.

This way of thinking about scientific knowledge provides a means of understanding what might be of value in a feminist standpoint approach, and the successes of this approach in sociology can be explained in terms of its conformity to this model of scientific knowledge. When Dorothy Smith advocates a sociology that is "for women", the idea that the knowledge is a tool and serves the interests of someone is explicit. The model that she advocates we construct is a model of the social world in which features that contribute to maintaining the power relations that keep women in positions of subordination are highlighted.

3.2   Objects, models, and values

This model-based account would seem to entail a form of relativism; science is relative to interest and goals. However, the account roots objectivity in values rather than seeking an account that is value-free. This is only possible if there are objective claims that can be made about values. There are some things that we as human beings *should* value because there is a fact of the matter of what enables us to flourish. There is a fact of the matter about what is good for us. Now what I mean by flourishing may, within a particular set of boundary conditions, have a wide variety of interpretations and possible instantiations, but it will have to meet certain minimal requirements. We need to have the basics to survive, for instance, food, shelter, human companionship. Minimally, when our models are constructed to fulfill those needs then our science is objective.[33]

I am proposing that we turn the problem of science and values on its head. The question of objectivity has been presented as a question of how science can still

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[32]The latter is Longino's distinction.

[33]It will clearly have to be guided by some conception of what a good life is and there are problems here at the moment, the only thing that I can think of in this regard is that we would have to have a conception of what makes the best *human* life and this is going to involve some judgments about what it is to be human. These issues are also matters of empirical investigation and continually open to reassessment. It isn't clear in the end that some sort of fundamental relativism can be avoided but that is to be worked out.

manage to be objective if values enter into it. But values are objectively based in projects which will be better for human beings than others, allowing humans to achieve goals that are more tied to their well-being, health, and flourishing. That such projects will produce better science is a belief that motivates those who pursue feminism and other democratic ideals in the social sciences. This means that the question is not how science can be objective if values enter, but rather which values are the ones that will give us objectivity in this sense.

This way of thinking about objectivity has obvious implications for standpoint theory and feminism more generally. Feminist sociologists frequently describe their project as one that provides knowledge *for* women. The idea that knowledge is to serve specific goals, relative to the interests of particular groups is clearly present. Additionally, standpoint theorists direct us to understand the institutions and social relations that maintain subordinate groups in their positions. The call to examine these institutions from women's standpoint can be seen not only as a call to understand the way the social and political world has been constructed but to seek alternatives that is alternative models of the social consistent with women's goals.

4   POSTMODERNISM AND METHODOLOGY

I have focused primarily on standpoint theory in sociology because it is has generated so much recent discussion. However, when Sandra Harding first proposed that there were three approaches to feminist philosophy of science, she identified standpoint as holding an "unstable position" between feminist empiricism and feminist postmodernism. Some of the early criticisms of standpoint, including the rejection of a universal woman, the concerns about essentialism, and the idea that a standpoint involves automatic epistemic privilege, merge with postmodernist criticism. Postmodernist critique in sociology and anthropology poses a challenge to objectivity, including the revisions to the traditional notions that were discussed in the previous section. The cultural relativism of anthropology when coupled with postmodernism challenges any understanding of objectivity, either in terms of method, subject, or relationship of account to the world. For this reason, a discussion of postmodernism provides a bridge to a discussion of feminist anthropology in the context of the issues that dominate this chapter.

There are two dominant and related issues with which feminist anthropologists are engaged. The first centers on methodology and it is here that the concerns of feminist anthropologists overlaps with those of feminist sociologists. However, a second issue in feminist anthropology raises issues that though present in feminist sociology have been more disruptive of feminist anthropology. The difficulties arise in relation to ethnography, postmodernism/poststructuralism, and relativism.

Questions of a feminist methodology in anthropology are often addressed more broadly as questions about a feminist methodology for the social sciences. So, for example, Maria Mies [1983] offers seven methodological postulates for feminist research, as well as nine strategies [1991] that respond to, extend, and deepen the

ideas in those postulates. Her primary concern however, is not the specifics of the postulates or strategies but to note that they stem from a "different understanding of science from the one we find in the dominant scientific paradigm. We initially called this alternative understanding 'feminist' because the political aim of our efforts is most clearly expressed in this term. This goal is the overcoming of women's exploitation and oppression" [1991, 70]. She goes on to say specifically that feminist research is more concerned with transformation than with knowledge and seeks active emancipation rather than spectator knowledge [1983].[34] Shulamith Rheinharz [1992] offers ten themes for feminist research methods, but some of the most important shared features also have to do with political activism. Mies states it explicitly as one of her strategies. "When selecting a research topic or problem, we should ask how that research has potential to help women's lives and what information is necessary to have such impact" [1991, 101].

In many of these points feminist theory in anthropology resembles standpoint theory in sociology, but most importantly for the current discussion, feminist methodology in anthropology includes connections to and understanding of those that are studied in a way that suggests a rejection of the traditional ideal of "objective knowledge" altogether. Rheinharz notes that "Feminist research frequently attempts to develop special relations with the people studied (in interactive research)" [1992, 204]. The sorts of relations that Rheinharz has in mind are ones that blur "the distinction between the formal and personal relations" [263]. This might happen through developing personal friendships with those who are being studied, giving assistance, or participating in activist movements with them. All of these require having a particular rapport with those being studied.

The more extreme view on rapport sees it as a prerequisite for feminist research. "By achieving rapport, the feminist researcher reassures herself that she is treating the interviewee in a nonexploitative manner. Rapport thus validates the scholar as a feminist, as a researcher, and as a human being. It symbolizes her sisterhood, her interviewing skill, and her ethical standing" [1992, 265]. Rheinharz does not herself support such a position and worries that "[e]xpecting to achieve 'rapport,' a concept that remains undefined, it is possible that the researcher will block out other emotions and reactions to the people she is studying. She might even romanticize the women or see them in stereotypic ways, because of her focus on 'achieving rapport.' And if she does not 'achieve rapport,' she may forego the study altogether. In my view it would be unfortunate if we were to introduce self-imposed limits to our research possibilities because of the notion of rapport" [1992, 265-266].

Rheinharz's reservations are shared by others. Leslie Bloom [1997] explores the difficulties she encounters when doing fieldwork and attempting to apply feminist methodology. In her eagerness to identify with her respondent, she made assumptions about their relationship based on their shared self-description as feminists

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[34]By this, I take it that she means "knowledge" as it might be defined by traditional epistemology. The question of whether there might be other ways of thinking of knowledge is addressed later.

and then was disturbed when unexpected difference emerged in the course of the research.[35] She identifies the difficulty this created for her in her failure to be fully committed to the tenets of feminist research, which direct the researcher to put herself on the same 'critical plane' as the respondent. She concludes, "While we may never eliminate the sense of being 'locked in uneasy sisterhood' when profound differences threaten to rupture the feminist research relationship, these powerful practices of feminist methodology may result in us using the discomfort as a catalyst and source of energy for feminist transformational praxis" [1997, 119]. In this particular case, the lesson that Bloom took away was a greater understanding of herself and the way in which her beliefs and presuppositions shaped her understanding of her respondent.

As feminist researchers have sought nonexploitative relationships with those that they study, they have used the resources of ethnography within their disciplinary tradition. In problematizing the relationship between social scientist and subject ethnography appears to have a *prima facie* affinity with feminist thought, at least this is what some have claimed [Stacey and Thorne, 1985]. However, the *prima facie* strength of anthropology as a model feminist social science has been challenged in a number of ways. Judith Stacey and Barrie Thorne [1985] admire feminist anthropology, in part, because of its stronger ethnographic tradition. They wonder why more feminist sociologists have not turned to this tradition as a methodological resource because of its seeming compatibility with feminist ideals. However, in 1988, Stacey writes "But now after two and half years of fieldwork experience, I am less sanguine and more focused on the difficult contradictions between feminist principles and ethnographic method I have encountered than on their compatibility". She worries that "the ethnographic approach masks a deeper, more dangerous form of exploitation. ... Precisely because ethnographic research depends upon human relationship, engagement, and attachment, it places research subjects at grave risk of manipulation and betrayal by the ethnographer..." [1988, 22-23]. The very nature of the relationship between the researcher and the researched that is supposed to provide a less exploitative approach creates a greater opportunity for exploitation. The closeness of the relationships both reveals and produces felt obligations that can create profound moral dilemmas for the researcher.[36]

Stacey examines the ability of postmodernism to address these conflicts by using resources of the *new* ethnography. "As I understand it, the postmodern ethnographic solution to the anthropologist's predicament is to fully acknowledge the limitations of ethnographic process and product and to reduce their claims. Like feminists, critical ethnographers eschew a detached stance of neutral observation, and they perceive their subjects as collaborators in a project the researcher can

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[35]The difference was over Zionism and was occasioned by the start of the first Iraq war. Bloom's sympathies for Israel were challenged by her respondent's anti-Zionist stance which Bloom identified as anti-Semitic.

[36]Stacey gives specific examples of such dilemmas, for instance, a case in which a "research collaborator" requested that details of her history be left out of product of the research, the ethnographic account, thus comprising its "truth" [1988, 24].

never fully control. Moreover, they acknowledge the indispensably intrusive and unequal nature of their participation in the studied culture. . . . Finally, postmodern ethnographers, influenced by deconstructionist fashions, aim only for 'Partial Truths'. . . " [1988, 25].

While this may provide a diagnosis for the researchers' moral unease, in the end Stacey is skeptical that postmodernism can provide an answer to the dilemmas that ethnography raises. It is better to acknowledge the problem, but the acknowledgment alone does not provide resources to solve the problem. "The postmodern strategy is an inadequate response to the ethical issues endemic to ethnographic process and product that I have encountered and described. It acknowledges, but does little to ameliorate the problems of intervention, triangulation, or inherently unequal reciprocity with informants; nor can it resolve the feminist reporting quandaries" [1988, 26].

As sociologists have turned to the new ethnography, anthropologists have cautioned that the alliance between postmodernism and feminism in anthropology is full of peril. In a widely cited [1989] article, Frances Mascia-Lees, Patricia Sharpe, and Colleen Ballerino Cohen warn that anthropologists would do better to seek a model for self-reflexive work in feminism rather than turning to postmodernism or the new ethnography; they explicitly juxtapose feminism to postmodernism. In their critique of the latter, they note that many of the insights that postmodernists claim are already in the feminist literature, but not acknowledged by postmodernists. Furthermore, "[w]hile anthropology questioned the status of the participant-observer, it spoke from the position of the dominant and thus for the 'other.' Feminists speak from the position of the 'other'" [1989, 11]. Mascia-Lees, Sharpe, Cohen also question the motives of anthropologists who turn to postmodernism, noting that Nancy Hartsock made a similar point. Why have anthropologists adopted the postmodernist turn away from truth just at a moment in history when "women and non-Western peoples have begun to speak for themselves" [1989, 15]? When the Other finds a voice, the power of such a voice is discounted.

Although it comes from a different quarter, the warning that Mascia-Lees, Sharpe, and Cohen are issuing reflects the dilemma of the standpoint theorists discussed above. If anthropology embraces postmodernism with its rejection of "Truth", it loses the ability to serve feminist (and other) goals. "Politically sensitive anthropologists should not be satisfied with exposing power relations in the ethnographic text, if that is indeed what the new ethnography accomplishes, but rather should work to overcome these relations" [1989, 33]. The importance of activism, the ability to do something in the world with what one learns and the vital role that plays for feminist anthropology depends on at least some pragmatic sense of "getting things right". Mascia-Lees, Sharpe, and Cohen claim that this may be better accomplished from the perspective of the oppressed group.

The problems and dilemmas that arise from people studying other people have been examined in the language of postmodernism with its challenge to "metanarratives". As Nancy Fraser and Linda Nicholson [1997] describe a postmodernism

that they attribute to Lyotard, a metanarrative is an overarching story that legitimates the knowledge-producing practices of the culture; the story purports to show that those practices lead to Truth. The ideal of objectivity in most of its meanings is part of that metanarrative. Given even this loose description, Harding's claim that standpoint is intermediate between postmodernism and empiricism can be made clearer. Standpoint is postmodern in that it challenges the modernist story about truth, objectivity, and scientific progress. Standpoint also calls for reflexiveness, but this is a characteristic of all feminist social science to some degree or another. In addition, the versions of standpoint examined here are allied with feminist empiricism in its goal of improving science through feminist critique and so it embraces some elements of the modernist picture since such an assessment requires some standard of what counts as better and worse science. I have argued that there are options for such a standard that do not require reverting to a modernist or traditional epistemology, but there is another alternative, which would be to abandon the quest for such a standard altogether. The result would be embracing some form of relativism, but then on what grounds could feminism be advocated?

5 ANTHROPOLOGY, ETHNOLOGY, AND POSTMODERNISM

The relationship between postmodernism, feminism, and anthropology is a complex and debated one. There are debates about the compatibility of postmodernism and feminism([Mascia-Lees *et al.*, 1991]; [Kirby, 1991]) and consequently the possibility of a feminist anthropology ([Strathern, 1987]; [Walter, 1995]).
The strong influence of postmodernism and the seriousness with which cultural and social anthropology have treated the postmodernist critique create challenges for feminist anthropology in a number of ways some of which have already been mentioned. For instance, feminist anthropology as it strengthened in the 1970s relied heavily on Sherry Ortner's thesis of universal subordination of women and gender asymmetry. Such universalism is inimical to postmodernism. In addition, the sex/gender distinction has played a crucial role in these debates. As mentioned earlier, the idea that sex and gender could be distinguished and that while sex was biological, gender was cultural, was taken as defining of an area of study for feminist sociologists particularly, since their area of study was the social and not the biological. The postmodernist idea that sex, no less than gender, is socially constructed would seem to undercut this presumption. If gender and sex are not distinct and sex is a cultural category as well much of the groundwork of feminist anthropology needs to be rethought.

As we have seen, standpoint has also been used in feminist anthropology, either explicitly or implicitly. Yet it occupies an intermediate position between postmodernism and empiricism in feminist sociological theorizing, its epistemological allegiance in feminist anthropology is less clear. On the one hand, standpoint justifies a path into the world of the research subject (insider) but at the same time demands that the subject speak for herself (since the researcher is an out-

sider). The legitimacy of the insider/outsider report is challenged because there is no ground on which it can be authoritative. On the other hand, one means of addressing this problem is to create "rapport". The researcher understands the research subject because she identifies with her *as a woman.* This identification is reinforced by the theoretical justification of standpoint theory. But this depends on an assumed sameness of the researcher and the subject. So identification of researchers with subjects provides a pull towards universalism and away from the particular [Visweswaran, 1997].

Visweswaran [1997] suggests that the reliance on some of the features of feminist methodology discussed above, combined with the dominance of ethnography, have rested on an unexamined use of the notion of woman and gender. "[F]eminist ethnographers have been largely unresponsive to feminist challenges to gender essentialism, relying on gender standpoint theory, which erases difference through the logic of identification" [1997, 616]. Lynn Walter puts the point more generally, highlighting the tension between the goals of feminism and the tradition of anthropology. Anthropology operates with the assumption "that culture is the collective representations of a society, both understood as totalities. Difference is circumscribed within the boundaries of culture.... Conversely, feminist anthropology... disputes the coherence of culture in society by pointing to the alternative, subversive, and radical challenges in constructions of them". In addition, anthropologists assume that they "can authoritatively represent the other culture because, as outsiders, they have no special interest in the outcome of the communicative practice of that community" [1995, 278]. But this is not the case for feminists as they do have special interests in the outcomes of gender struggles. Put in the language of standpoint, they are not outsiders strictly speaking but rather outsider/insiders.

Vicki Kirby [1991; 1993] defends the postmodernism critique more strenuously arguing that feminist anthropologists need to confront the tendency of anthropology to cling to one of its most basic assumptions, the belief that is "something ultimately recuperable and knowable" [1993, 127]. She claims that feminist repress or sanitize the question of difference, reducing the issue to plurality, In so doing, she believes they are aligning themselves with the discipline of anthropology and thus alienating themselves from (interdisciplinary) feminism more generally. She calls for feminist anthropologists to grapple with the ambiguities that they are faced with in represented Others and not assume a unity of women. "In other words, although feminists are loosely bound under the collective banner of a political movement, we are also *divided* from each other. We are differently located within these global narratives that translate, and very often efface, the material specificity of the perspective that *must* separate us. Hence the need to reconceptualize difference differently, so that 'otherness' is more subtly nuanced and the political terrain of its identity reinscribed" [1991, 129]. She proposes no formula for doing so, however and is critical of even those who she admires as they do attempt to grapple with these issues, claiming, for instance, that Donna Haraway does not recognize her own use of "othering" in her accounts and so falls back into the traditionalist trap. She claims Haraway's belief "that feminism should

be judged a politically preferable discourse because it can escape the violence of representational dichotomies is itself an unwitting reiteration of a developmentalist and essentialising logic" [1991, 129]. So she exhorts feminist anthropologists to embrace feminist postmodernism and at the same time rejects the legitimacy of any claim that it is a superior discourse.

Kirby's diagnosis of the issue of difference and the rejection of any form of objectivity or judgment about correctness of accounts is clearly problematic. It is not clear how ambiguity can serve as a basis of political action, and yet, she recognizes quite clearly that in choosing representations through which we make decisions to act requires taking some viewpoint and privileging it over another. Is it possible to take the insights of the postmodernist critique and construct a feminist anthropology? Again, some account of objectivity would seem crucial, not only to feminist anthropology but to address the postmodernist critique of anthropology more generally.

There have been feminist anthropologists who have addressed the problem of essentialism and difference while attempting to remain sympathetic to postmodernism. Elizabeth Enslin [1994] bemoans the dominance of ethnography in feminist anthropology and offers a more fine-tuned approach to the question of identity. She advocates Haraway's notion of 'objectivity' based on partial connection and considers some specific examples of insider/outsider ethnography that focus on the marginality of the researcher as a tool for better understanding the relationship between the oppressed and the oppressor [1994, 549]. Her invocation of Haraway includes the use of the notion of 'situated knowledge'. "Situated knowledge is not a new form of individualism, relativism, or identity politics whereby one can argue any position based on one's sexual, ethnic, national, or economic location. On the contrary, it is knowledge gained from 'feminist objectivity' that is situated rather than detached" [1994, 555]. The partial and situated knowledge that Enslin advocates is best understood in relation to practice. It is knowledge "for, by, and about" women in the sense that it enables them to accomplish the goals that they have at a particular time and place. There are samenesses that allow identification and recognition of these goals, but whether or not the accounts that are offered by the feminist anthropologists work or not is a matter of fact. They are samenesses but not universals. So for Enslin, these are the choices that the anthropologist must make: the connections between researcher and researched are not given by any universals but are the result of conscious decisions based on grappling with one's own identity as a researcher.

5.1   *Feminist anthropology?*

Enslin's struggle to articulate some form of objectivity together with Kirby's postmodern push away from legitimatizing any knowledge claims provide a snapshot or endpoints of what seems to be a dead end in the discussion. Are feminism and postmodernism compatible? If not, then as long as anthropology is postmodernist, feminism has no home there. But the debate about feminism and postmodernism

is not resolved and it is beyond the scope of this chapter to attempt to resolve
that issue. However, I have taken a stand in that debate in the previous section
and have suggested an account of science that is sensitive to some of the insights
of postmodernism can be compatible with and even useful for feminism. Whether
such an account, or either of the other accounts of objectivity explored (Harding's or Wylie's) would provide a greater understanding of feminist anthropology
remains to be seen and in particular whether it could address the issues posed
by ethnographic research: the problematic relationship of the social scientist to
subject.

It is also worth exploring the nature of the postmodernist critique of anthropology and the question of to what extent feminism should be allied with that trend
in anthropology. Medford Spiro [1996] notes that there are postmodernists who
argue that anthropology should not be considered a science. If that were the case
then further discussion of the discipline in the context of feminist philosophy of
science would be pointless. Spiro rejects this claim, on fairly traditional modernist
grounds and the claim could be rejected within the framework of a philosophy of
science that is sensitive to the interplay of values and science as well. Again, it
is beyond the scope of this chapter to solve the question of scientific status of anthropology or the worth of postmodernism, although I believe that anthropology
has a claim to status as a science and that there are elements in postmodernist
critique that are valuable.

The literature on feminist anthropology is thin after the mid-nineties. The
anthropology/feminism/postmodernism debate that I have focused on seems to
have reached an impasse at that time. Feminism appears more clearly in applied
anthropology and what has been called "advocacy anthropology". But theorizing feminism in anthropology seems at a standstill, with many of those engaged
in the debate having moved to other topics. Whereas the conceptualization of
standpoint in sociology enabled substantial discussion of its epistemological status, the strength of postmodernism in anthropology problematized the relationship
between the researcher and the subject in a way that made the use of standpoint
theory in feminist anthropology difficult.

Feminist anthropology as advocacy or applied anthropology offered a venue for
feminist activism, however, it also holds promise from a revitalization of epistemological discussion. The activist projects that these approaches generate provide an
arena for a more pragmatic approach to knowledge, such as the model-theoretic
approach sketched above. An examination of how anthropological accounts have
been used to support feminist causes could provide a means for getting clearer on
how such accounts grow out of the interests of those for which the knowledge is
produced. For instance, I would urge an examination of Diane Bell's *Ngarrindjeri Wurruwarrin: A World that is, was, and will be* [1998] as she engages both
in applied anthropology and ethnography and discusses what she is doing as she
shifts from one to the other. As a result she grapples with some of the issues that
have been discussed in the context of trying to produce knowledge for a particular
purpose. The book is an account of her experience as an anthropological expert

for the Ngarrindjeri who sought to prevent the building of a bridge on the grounds that it would destroy a sacred women's site. The developers argued that this was a fabrication and the book chronicles their failure to prevent the building of the bridge but also the way the dispute over the question of the fabrication of the story unfolds. An account like this provides an opportunity to examine the interplay back and forth between theory and experience as the "model" is constructed.

Bell's work has been controversial, as has much advocacy anthropology. Some of this controversy is generated by the false ideal of science as value-free and I would advocate approaching it through one or more of the alternatives that has been suggested in the discussion of standpoint theory. Once we acknowledge that values play a role in science in the ways that feminist concerned with epistemological issues have claimed, then the specific investigation of how they play that role and an evaluation of how the science is shaped in any particular situation is not only warranted but also required if we are to understand social scientific practice.

6 CONCLUSIONS

Feminist sociology and anthropology provide rich areas of further study for those interested in key questions in philosophy of social science. The debates that are occurring here could have ramifications for an account of science in a more general way. First, feminists are grappling with questions of the role of values in science as the debates over standpoint clearly show. It is generally acknowledged that values do play a role in science and the way in which this happens and how we can still argue that science is objective in some relevant sense remains a key point of discussion. Feminists are grappling directly with these issues. Further, feminists are in the forefront of investigating the relationship between the knower and the known, a central theme in the anthropological literature. This literature reveals a continuing division over the value of postmodernism, standpoint theory, and feminism more generally. While it is sometimes claimed that these debates say nothing new and are not particularly *feminist*, feminist philosophers of science are among those seeking to clarify these two central debates in the social sciences and have made substantial contributions to the understanding of values in science and what this means about the objectivity of science.

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WHAT'S 'NEW' IN THE SOCIOLOGY OF KNOWLEDGE?

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INTRODUCTION

Exploring the impact and promise of a disciplinary practice seems to require that one privilege the latest trends it has manifested over the long trajectory of its emergence. That is, what's *new* claims more salience than what is old. In the field of the sociology of knowledge, we do not lack for announcements of novelty.[1] The general drift of such novelties appears to be "from knowledge to culture." There has also been a recent impulse to *pluralize* the concept — hence, "knowledge*s*."[2] While this essay attempts to grasp what all this novelty betokens, let me affirm, at the outset, a more than nostalgic appreciation for the "old" sociology of knowledge.

We have seen — thanks, in particular, to the exertions of David Kettler, Nico Stehr and Volker Meja — a remarkable revitalization of interest in the "old" sociology of knowledge.[3] There is now a burgeoning literature not only about its *history* but about its *contemporary relevance*.[4] What exactly was the "old" sociology of knowledge trying to do? Is that still what the "new" sociology of knowledge is about? How does the current slogan, "social construction of reality," relate to the theoretical tradition known as the sociology of knowledge?[5] I contend the answer to this question is anything but self-evident. I am disposed to suspect that postmodernism may well have slipped the moorings that held the very idea of a sociology of knowledge together. *Knowledge* and *sociology* have become "essentially

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[1]See, e.g., [Law., 1986; Swidler and Arditi, 1994; McCarthy, 1996; Evers, 2000].

[2]In 1988 Donna Haraway entitled an important essay "Situated Knowledges..." A few years later, Ellen Messer-Davidow, David Shumway and David Sylvan published a major collection on the problem of disciplines and interdisciplinarity under the title *Knowledges* [1993]. More recently still, Peter Worsley issued a book entitled *Knowledges* [1997].

[3]Kettler *et. al.* have not only published substantial new textual sources from Mannheim himself (e.g. [Mannheim, 1982]), but produced a body of secondary literature reassessing Mannheim and his project and situating that in both its original and the contemporary context. See: [Kettler, Meja and Stehr, 1974; Stehr and Meja, 1982; Meja and Stehr, 1990; Stehr and Meja, 1984].

[4]"We are now witnessing a renewal of interest in the sociology of knowledge, perhaps even a kind of renaissance." [Stehr and Meja, 1984, 4] See [Kettler and Meja, 1988; Meja and Stehr, 1988; Harms and Schroeter, 1990; Longhurst, 1989].

[5]For some parallel ruminations, see [Hacking, 1999] and, making a point of the inversion, [Searle, 1995].

contested concepts."[6] What *is* "knowledge," for us? And what, "sociology"? How can we even hold these terms together?

Sociology of knowledge seems riddled to the core with problems of epistemology, methodology and politics that open out onto the whole morass of "postmodern social theory" and its so-called "social construction of reality."[7] How can we claim knowledge of knowledge without raising all the age-old quandaries of epistemology? It helps not a whit to pretend that such issues don't matter *and yet* to profess to "know" determinately that the "subject" is "constituted" by "language" or "culture" or anything else. There is a *sort* of performative self-contradiction that should embarrass even a postmodernist. In fact, in advancing any claims in a post-foundationalist discourse we face acute problems of reflexivity: How, like Münchhausen, are we to lift ourselves by our own bootstraps?

We have been presented with the "death of the subject," the "death of the author," a crisis of *writing* which is not simply ethical-political but epistemic.[8] It is hard to take the first person singular seriously, especially in the nominative case: the "hermeneutic of suspicion," the semiotics of deconstruction, demands: who/what speaks *through* me?[9] There is a deeper quandary, however, bringing us back into the orbit of the sociology of knowledge, namely, the problematization of the first person *plural*: what does that mean, "we?" *Who* is "we?"[10] If the ground has been cut out from under the individual subject, occasioning what is now called "social epistemology," how is this latter endeavor to carry on its work?[11] From what standpoint? With what claims to *knowledge*?

Are we sure the issue *is* knowledge(*s*)? Why not *beliefs*, or *opinions*, or *valuations*? Or simply *information*? What is at stake in the word? Does the plural indicate congeries of discrete knowledge-*claims* or, instead, different "ways of

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[6][Gallie, 1968].

[7]"How can a knowing subject, who has particular interests and prejudices by virtue of living in a specific society at a particular historical junction and occupying a specific social position defined by his or her class, gender, race, sexual orientation, and ethnic and religious status, produce concepts, explanations, and standards of validity that are universally valid? How can we both assert that humans are constituted by their particular socio-historical circumstances and also claim that they can escape their embeddedness by creating non-local, universally valid concepts and standards? How can we escape the suspicion that every move by culturally bound agents to generalize their conceptual strategy is not simply an effort to impose particular, local prejudices on others?" [Seidman, 1981, 134])

[8]See [Foucault, 1973; Barthes, 1989; Paul Smith, 1988]. But see [Carrithers *et al.*, 1985], for an effort to retrieve the essential ground.

[9]"'Who am I?' is about (always unrealizable) identity; always wobbling, it still pivots on the law of the father, the sacred image of the same. Since I am a moralist, the real question must have more virtue: who are 'we'? That is an inherently more open question, one always ready for contingent, friction-generating articulation." [Haraway, 1992, 324].

[10]"How do 'we' judge or prioritize epistemic standards that include empirical adequacy, explanatory comprehensiveness, quantitative precision, empirical predictability, logical coherence, conceptual economy, aesthetic appeal, practical efficacy, and moral acceptability? ... Who, in other words, is the 'we' that legitimates justificatory strategies?" [Seidman, 1981, 134].

[11]See [Fuller, 1991], and the journal by that same title, which avows: "social epistemology's clearest intellectual roots are in the sociology of knowledge." ("Preview," *Social Epistemology* 11 (1997), 139.)

knowing?" What can the *sociology* of knowledge endeavor? Does it seek to *explain* semantic choices, i.e., the content of specific claims, or the categories conceptually ordering them, or even their warranted assertibility or truth? Or does it pursue processual "ways" of knowing, as in the "styles of reasoning" explored by Crombie and Hacking, or such contrasting modes of discourse as rhetoric vs logic, description vs explanation, analysis vs synthesis?[12] And is this best explored as a question of *consciousness* and *subjectivity* or as a matter of *textuality* and *code*? Or, alternatively, is the knowledge we are after more holistic and experiential: what happens to persons — *embodied* knowledge, how it "feels," acquaintance rather than cognition (*kennen* rather than *wissen*)? And is it individual or collective experience we should explain? The slogan "from knowledge to culture" suggests that the essential question is not merely about "ways of knowing" but, more pertinently, about the *constitution of communities. Whose* knowledge(s) seems as important as the *what* or *how* of "knowing."[13] Politically, that can insinuate a reflexive anxiety about representation: Is speaking *about* cultures of others not a matter of speaking *for* them — suborning them into *our* discourse?[14] The postmodern crisis of ethnography seems to have revolved around that conundrum.[15] If the knowledge(s) in question belong to *agglomerated others*, how are we to constitute them into groups (or discern how that has befallen or been achieved by them)? Above all, what does it mean to talk about *social construction*? How can the *social* explain, when we scruple about what (or whether) that can signify, concretely, and we can't even agree on what an explanation should or can do?

"Sociology" — what is *that*, anymore? Since 1970 when Alvin Gouldner published *The Coming Crisis of Western Sociology*, "at least 150 articles concerning the 'crisis of sociology'' have appeared.[16] Indeed, the "sense of disorientation in American sociology . . . has never really abated since 1970."[17] It is thus a commonplace to observe that "since the mid-1960s the discipline has endured a crisis of identity."[18] Steven Seidman, noting this "almost permanent sense of crisis," proclaimed: "sociological theory has gone astray."[19] Even more drastic was Norman Denzin's pronouncement: "sociology is dying. The death of the social is upon us."[20] Since the dissolution of the Parsonian orthodox consensus, "the

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[12] [Crombie, 1994].

[13] The question, "Whose knowledge" is at issue in two very prominent publications of the recent era, first MacIntyre, [1988] and then Harding, [1991].

[14] "We have to keep a discomforted eye on the historical pedigrees and current orthodoxies of what is sometimes called 'ethnography,' a practice of representing the cultures of others. The practice, like the word, already extends social distance and constructs relations of knowledge-as-power." [Johnson, 1986/87, 70]. Fuller, [1996] offers a precise formulation of this logic of representation. Haraway, [1992] lays out the same argument.

[15] [Clifford and Marcus, 1986].

[16] [Gouldner, 1970]; the observation about the 150 articles comes from [Steinmetz and Chae, 2002, 113].

[17] [Steinmetz and Chae, 2002, 112].

[18] [Kuklick, 1983, 292].

[19] [Seidman, 1981, 131].

[20] [Denzin, 1987, 179].

very idea that sociology has a 'core' has become doubtful."[21] One adherent of this lapsed orthodoxy lamented in 1993 what he called *The Decomposition of Sociology*.[22] At the end of the century, Stephen Cole organized a massive inquiry into *What's Wrong with Sociology?*[23] Over the discipline there had clearly settled a mood for which several commentators found the 19th-century phrase *fin de siècle* of renewed resonance. Anthony Giddens saw sociology beset with "feelings of disorientation and malaise" identified with the notion of *fin de siècle*, and Jeffrey Alexander entitled a 1995 book *Fin de siècle Social Theory*.[24] Stuart Hall made the point elegantly: "When I was offered a chair in sociology, I said, 'Now that sociology does not exist as a discipline, I am happy to profess it.'"[25]

In that light, it behooves us to consider that "sociology" in the phrase *sociology of knowledge* may not be identical with that in the conventional — especially the *American* — sense of the discipline.[26] Indeed, from the beginning at least of its American reception, the sociology of knowledge has proven stubbornly marginal, even potentially subversive *vis á vis* mainstream sociology.[27] Recognizing this aspect of the sociology of knowledge opens up the prospect that it might be understood as a potential *challenge* to disciplinary sociology and the source for alternative social theory.[28] Historically, the "sociology of knowledge" has been not only something specific and explicit *within* sociology, whose trajectory appears straightforward, but also a *space of theory* in which many other disciplines have tacitly deployed arguments. This latter aspect calls for discriminating reconstruction of a kind of "interdisciplinarity" *avant la lettre*. Much of the impetus behind the "new" sociology of knowledge, as I will note, derives from *outside* the mainstream of the sociological discipline.

1   THE OLD SOCIOLOGY OF KNOWLEDGE

Something explicitly conceived as a "sociology of knowledge" only emerged in the 1920s in Germany. Thus, the conventional schema for its historical reconstruction has typically organized it into three phases.[29] The first phase is the *prelude* to its explicit formulation, i.e., European social thought to around 1920. The second phase is the *classical moment* of crystallization: the 1920s in Germany. The third phase is conceived as a *normalization* into general sociology, especially in

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[21][Lynch and Bogen, 1997, 484].

[22][Stephen Cole, 2001], based on a special issue of *Sociological Forum* in 1994 edited by Cole on the same topic. See also: [Mouzelis, 1995].

[23][Lynch and Bogen, 1997, 484.]

[24][Giddens, 1994, 56; Alexander, 1995, 5 and passim].

[25][S. Hall, 1990, 11].

[26]Steinmetz and Chae, among many others, note "systematic differences between European and North American sociology." [Steinmetz and Chae, 2002, 126]. See also [Lamont and Wuthnow, 1990].

[27]For a particularly important early instance of this insurgent potential, see [Mills, 1939; 1940].

[28]This is the intimation in [Hekman, 1986]. The contrast of "social theory" with "sociological theory" has already been established; see [Fuller, 2001, 361].

[29]Stehr and [Meja, 1984, 6]. See, e.g., [Coser, 1968] and, more extensively, [Remmling, 1973].

the United States, roughly from 1930 to 1970.[30] This narrative now needs to be extended to take cognizance of the assertion, since around 1970, of a "new" sociology of knowledge.

1.1   Forerunners

Probing for "anticipations" of ideas is a highly problematic historical exercise. Nonetheless, it is useful to consider the sociology of knowledge in the light of its forerunners for two reasons. First, what emerges from such a consideration is the enormous scope and depth of issues which funnel into the crystallizing moment, whereby the connection of the sociology of knowledge to a much larger sense of Western social thought becomes palpable. Second, the sociology of knowledge gets situated against the specific backdrop of the emergence of "social science" as an *academic-disciplinary project*.[31] Accordingly, the tensions of its interdisciplinary heritage and political affiliations come to the fore.

The key association from which to set out is the idea of *critique*: the submission of the forms of social relations to rational-critical examination with a transformative intent. The two dimensions of this critique have been epistemology and politics. That is, from the outset this mode of thought, virtually synonymous with "Enlightenment," raised questions of *validity* in order to create possibilities for *revision.* Articulation of frailties in human knowledge aimed not at quiescence but at transformation. This was the Baconian *leitmotif* running from his own seminal discrimination of the "idols" of human knowledge through the grand *Encyclopédie* of his admirers, Diderot and D'Alembert, to Kantian *Kritik* and the writings of the *Idéologues* and the "utopian" socialists of the French revolutionary era.[32] The decisive heir of all this was Karl Marx.[33] *The German Ideology* remains one of the seminal texts for anyone taking up the concerns of a sociology of knowledge.[34] In the hands of Marx, "socialism" shifted emphasis from utopianism to *Ideologiekritik.*[35] Theoretical debunking of established ways of thinking appeared as central as enunciating the political vision of an alternative social order.

But if *Ideologiekritik* became one of the central forms of politics, it was complemented from the outset by an impulse to use the techniques of critical scrutiny

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[30]The cultural geography of these phases is not insignificant. Social thought in Europe has always had a stronger current of philosophical and political reflection, and one might also add a stronger literary-rhetorical flair than American social science. The "old" sociology of knowledge carried that valence of European culture, making it uncomfortable for mainstream American social science.

[31]In fact, one of the main undertakings of contemporary sociology of knowledge, in my view, is the exploration of (academic) disciplinarity and interdisciplinarity: the formation and transformation of intellectual specialties and their interaction.

[32]See [Remmling, 1973], for one piece of this history. I address aspects of it in [Zammito, 2002].

[33]Marxists, of course, find the "sociology of knowledge" a deviation, sometimes a "mere, bourgeois" deviation from the authentic tradition of *Ideologiekritik*. This is the stance adopted, for example, by the Frankfurt School. See [Bailey, 1994].

[34]Marx, 1998.

[35]On this tradition, see [Lenk, 1984].

in an ostensibly more "objective" manner: to make a "science" out of it. That entailed both the recognition of socio-historical difference and the effort to encompass it in thought. The Enlightenment "science of man" was an early expression. The grand substantive philosophies of history of the early 19th-century (from Condorcet to Comte and Hegel to Marx) represented daring but premature synopses. *Historicism*, the methodological codification of a *disciplinary* history, set out from the aporias of these earlier ventures, fusing *Historik* with *Hermeneutik* to rework the enormous heritage of historical materials that would not fit into any neat *Universalgeschichte*. Incontrovertible historical difference, complemented by proliferating experience of non-European cultures through contact, conquest and commerce, made methodological systematization imperative. To rival the established discipline of history, in Western Europe a disciplinary "sociology" emerged, from Comte to Mill to Durkheim, as a "positive science" aligned with triumphant natural-scientific method.[36] Universality clashed with uniqueness and rebounded on the interpreter to pose problems of evidential adequacy and cultural relativism. Wilhelm Dilthey and the Neo-Kantians, especially Heinrich Rickert, struggled with these aporias and set the stage for the disciplinary assertion of "social science" in Germany.[37]

As the twentieth century dawned, Émile Durkheim in France and Max Weber in Germany emerged as the grand masters of this founding moment of disciplinary sociology: each seeking as much to secure this new "science" from the "mere" politics of Marxist *Ideologiekritik* as to specify its methodological warrant against the epistemological and cultural quandaries of the historicist-hermeneutic tradition.[38] Thus the central problems out of which the very idea of disciplinary social science arose were: political contestation, historical-cultural "relativism," and the positivist idealization of natural science as the definitive standard of objectivity. *Situating* knowledge-claims against differentially "social" backgrounds posed challenges on all three registers. Two issues became *reflexively* intrusive: the question of the *intellectual* as a particular *type* within the social order (especially after the Dreyfus affair), and the question of academic *discipline* ("sociology") as the proper vehicle for "scientific" mastery or "truth."[39] The classical moment of the sociology of knowledge arose as these enormously complex "internal" currents converged in a catalytic "external" situation of maximal stress.[40]

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[36]On the contribution of this vein of thought to the emergence of the sociology of knowledge, see [Schmaus, 1996].

[37][Dilthey, 1996; Rickert, 1986].

[38]Two older studies still seem very effective in capturing this, [Aron, 1957; Antoni, 1959]. It is crucial to recover historically that "during the first decade of the century, the term 'sociology' had been loosely associated with Marxism." [Stehr and Meja, 1990b, 4.] Similarly, when Eckhard Kehr sought to introduce "social history" in Germany in the 1920s, it was suspect as merely "socialist" history.

[39]See Max Weber's two great lectures, "Politics as a Vocation" and "Science as a Vocation," which are meditations on just these themes. [Weber, 1958]

[40]It is apt to introduce the contrast "internal" vs "external" in this context, even if problematically, in "scare quotes," for it remains central to the conceptual field in which sociology of knowledge, indeed, social science more generally, has developed historically. The best treatment

1.2 The Classical Moment

The emergence of the sociology of knowledge needs itself to be "situated." It demands to be understood as one of the distinctive expressions of "Weimar Culture," the extraordinarily conflicted moment of Germany in the 1920s, in which creativity played hopscotch with catastrophe and lucidity gave way to conflagration.[41] This is vivid especially in the career of Karl Mannheim.[42] Mannheim rode this whirlwind; indeed, it shaped his life and work indelibly. From his Hungarian origins and revolutionary association with Georg Lukacs (theoretically reflected in the latter's monumental Neo-Marxist *History and Class Consciousness*), Mannheim emigrated to Germany, where he absorbed the "crisis of historicism" in the writings of Ernst Troeltsch, the disciplinary establishment of German sociology led by the Weber brothers, the elaboration of Dilthey's *Weltanschauungslehre* and its redescription as a "sociology of knowledge" by Max Scheler, and, finally, the existential revision of phenomenology in the work of Martin Heidegger. All these impulses (to name only the major ones) he worked up in papers over the 1920s.[43] He burst into the spotlight with a presentation to the Congress of German Sociologists in 1928, which occasioned a tumultuous reception.[44] He followed up on this with the publication the following year, to even greater controversy, of *Ideologie und Utopie*, the grand statement of his sociology of knowledge and the point of departure for all subsequent work in the field.[45]

Mannheim has become synonymous with the classical moment of the sociology of knowledge. Clearly it was not his achievement alone. Not only did he not coin the term; he was in many ways simply digesting far vaster theoretical discourses into his own, with the inevitably idiosyncratic inflection that comes of that. But Mannheim did work through all the issues and crystallize them into what became recognized as a distinct research program, galvanizing a critical response which has made him historically indisseverable from the field he identified.

The central concept Mannheim articulated was *Seinsverbundenheit*: the "existential connectedness" of knowledge. For Mannheim, this existential connection was *social* — a matter of the *differential situation of groups in social life* — and the "perspectivism" this imposed rendered knowledge problematically partial and partisan. Thus he was drawn to the central problematic of *Ideologiekritik*. But he was simultaneously interested in *bridging* these partialities, both as a political program and as a theoretical one. Thus he wished to develop an *interpretive method*, which would enable the accurate reconstruction of these partial perspectives, and a *vantage point*, from which to adjudicate and perhaps reconcile their differences. This led to his theories of "documentary meaning" or interpretive sociology, of

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of these contrast terms is [Shapin, 1992].

[41] [Gay, 1968; Ringer, 1969; Forman, 1971; Kurucz, 1967]; on Mannheim in this context see especially pRinger, 1992; Frisby, 1992].

[42] [Longhurst, 1989; Simond, 1978].

[43] [Mannheim, 1952; 1956; Wolff, 1971]. And see the new publications from manuscripts of the 1920s: [Mannheim, 1982].

[44] His talk and the battery of responses is now available in [Meja and Stehr, 1990].

[45] [Mannheim, 1948]. See [Lieber, 1974].

"total" ideology, and of the "free-floating intelligentsia." All of these were highly controversial conceptions. They drew severe criticism in the original German setting and have sustained considerably more in subsequent reception-history. The most salient complaint against Mannheim has been that his idea of a "social determination" of thought condemned him to relativism or reductionism, or both. Another, of particular relevance to the "new" sociology of knowledge, was against his resolution to exclude natural science, logic, and mathematics from the reach of his method.

The Nazi *Machtergreifung* drove Mannheim once again into emigration, eventually to England, where he sought to assimilate his work into the dramatically different traditions of discourse of Anglo-American sociology. The English translation of his major work, *Ideology and Utopia,* in 1936, began this transposition. Publication of collections of his essays, translated and edited by others, continued it. He himself turned to other pursuits in his new context, and the fate of the field he had launched passed into the hands of others. Particularly crucial, in this historical moment, was its reception in the United States, where sociology as a discipline was coming under the grip of a powerful and lasting orthodoxy, Parsonian functionalism, and a generation of emigré German sociologists felt compelled to fit themselves into that system and its different tradition of "scientific" discourse.

1.3 Normalization

Mannheim seems to have achieved his American reception on the coattails of Max Weber.[46] The key reviews of *Ideology and Utopia* and discussions of Mannheim's sociology of knowledge in general — especially by Talcott Parsons and Robert Merton — are tightly linked to the reception of a broader "German sociology" identified with Weber.[47] This reception entailed a pruning process, which sought to cut away from proper disciplinary sociology what Americans took to be a problematic German metaphysical penchant.[48] The result was a "normalization" of the sociology of knowledge to American conventions.[49] That had two components. The first was thoroughgoing criticism of Mannheim's philosophical positions from the vantage of the Received View of logical positivism/empiricism,

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[46] Perhaps the most influential review of Mannheim's work for Americans was literally contained in a major work on Weber: [Schelting, 1934].

[47] Key texts of the American reception are reproduced in Curtis and Petras, eds., 1970. Not only Mannheim but Weber himself underwent revision: away from interpretive to nomothetic ideals of "science." Steve Fuller, summarizing the argument of J. R. Hall, 1999, notes that Parsons "decisively shifted the ground of social theorizing from Max Weber's original focus on comparative-historical, case-pattern, ideal-type methods to a more natural-scientific approach that stresses analytically distinct causal variables as the ultimate objects of social inquiry." [Fuller, 2001, 360.]

[48] [Kettler, 1967, 399; Kettler, Meja and Stehr, 1990]. The persistence of this concern is to be noted as late as 1983: "There is no coherent, self-conscious research tradition in the sociology of knowledge as such. Much of the literature in the field is best described as philosophical anthropology..." [Kuklick, 1987, 288].

[49] "..the process of dissemination and acceptance of the sociology of knowledge in North America involved its *transformation...*" [Stehr and Meja, 1984, 5].

amounting to a complete dismissal of any epistemological cogency to his work.[50] The second was a domestication of Mannheim's sociological approach to the prevailing structural-functionalism of American social science. Here, the key figure was Robert Merton.[51] Merton took up the sociology of knowledge and reorganized it into a "sociology of science."[52] He and his school adopted an approach to science which conformed perfectly to what philosophers of science in the logical positivist/empiricist tradition had articulated to define their own pursuits. This *positivist collusion* revolved around Reichenbach's discrimination of the context of discovery from the context of justification, now reformulated as the discrimination of scientific practices from scientific outcomes.[53] Merton insisted he sought "not the methods of science, but the mores with which they are hedged about," and he disowned any "adventure in polymathy" that would draw him into assessing the methods or substance of science.[54]

In the postwar period a number of others carried the sociology of knowledge forward, making it a respectable if minor specialization within mainstream American sociology.[55] Given the strongly nomothetic, empiricist, and quantitative drift of post-war American social science, however, the "interpretive" approach of Mannheim's original program proved more congenial to intellectual and cultural historians than to sociologists of the mainstream.[56] Mannheim himself recognized that the sociology of knowledge "exceeds the problem area of [sociology] ... in the direction of philosophy and in the direction of a politically active world orientation."[57] Peter Hamilton expressed typically mainstream concerns with his observation: "reduction of sociology of knowledge to a philosophical level merely emasculates it, whilst reduction to a political level perverts it."[58] It was only in certain dissident theoretical traditions that the sociology of knowledge seemed important. Here the phenomenological sociology of Alfred Schütz, especially as it fed into the pragmatist social psychology of G. H. Mead, formed the main channel of the persistence of sociology of knowledge in American sociology. There were also affinities in ethnomethodology.[59]

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[50] "Philosophers have extended considerable effort in order to demonstrate that a sociology of knowledge is neither possible, necessary, nor desirable." [Stehr and Meja, 1984, 2]. The essential critique had been launched already in the German context, in the important work of Grünwald, 1934. See also Child, 1970. Edward Shils, who fully endorsed this critique, commented: "This was one of the reasons the sociology of knowledge came to a halt for some years. It was too embarrassing to go on with it. It was probably one of the reasons Karl Mannheim gave up the sociology of knowledge..." [Shils, 1982, 17].

[51] Above all see [Merton, 1937; Merton, 1941; Merton, 1945]. For a good sense of the shift from Mannheim to Merton in American sociology of knowledge, see [Edward Shils, 1982, 19–24].

[52] See [Merton, 1996, 267–336; Merton, 1977, 3–141].

[53] See my account of this in[Zammito, 2004].

[54] [Merton, 1996, 267].

[55] See especially [Wolff, 1970; Znaniecki, 1970].

[56] "Downplaying the actual and the possible significance as well as the implications of the sociology of knowledge has apparently been one of the strategies of sociologists in attempting to achieve the legitimation of the sociology of knowledge as sociology." [Stehr and Meja, 1984, 7].

[57] Karl Mannheim, in [Wolff, 1971, 263].

[58] [Hamilton, 1974, 150].

[59] On early ethnomethodology see: [Garfinkel, 1967; Garfinkel and Sacks, 1970].

The publication of *The Social Construction of Reality* by Peter Berger and Thomas Luckmann in 1966 appeared to signal a dramatic revival of interest in the sociology of knowledge in American social science, but the title's resonance with later usage makes it seem far more radical than it was.[60] The research program of Berger and Luckmann was significantly different from that of classical sociology of knowledge. Against the "classical" concern for esoteric knowledge and high-intellectual production, it turned to everyday knowledge. Rather than take up the politically abrasive question of *Ideologiekritik*, it concentrated on conventions of the commonplace. Rather than confront the abyssally reflexive issues of epistemology, it "bracketed" these as matters for *another* discipline, philosophy, and proposed to get on with empirical work.[61] In short, Berger and Luckmann took out the *philosophy* and they took out the *politics*. The resulting sociology of knowledge addressed only the "everyday." For the future of the sociology of knowledge it was largely sterile.

Ironically, social thought was about to plunge into tempestuous contextual currents again, not all that unlike the Weimar Culture moment. Beginning in the mid-1960s, the political climate of rebellion against established authority, questioning the complacency of liberal ideologies and the complicity of liberal politicians in repressive regimes and global imperialism, found Parsonian functionalism and its "modernization theory" all too serviceable to oppression. Criticism of the conservative implications of functionalist orthodoxy in sociology generally led to a proliferation of alternative approaches.[62] The late 60s would leave little of orthodox American sociology untouched, and it would totally transform the impulses of the sociology of knowledge. In addition, a reflexive turn paralleling the one Mannheim himself took emerged among American sociologists: they became obsessed with intellectuals like themselves and the warrant for their own judgments of society.[63]

It was not the internal evolution of American sociology, not even in the tradition of Mead, that would trigger the "new" sociology of knowledge. Rather, it drew its intellectual impetus from beyond America (German and French theoretical impulses), and from beyond disciplinary social science (both the humanities and, after Kuhn, the natural sciences). In Germany, the first decisive development was the elaboration of *Hermeneutik* by Hans Georg Gadamer in his epochal *Truth and Method*.[64] That work spawned two debates, first with Emilio Betti, on the tension between hermeneutics as method and hermeneutics as ontology, and second with Jürgen Habermas, over the emancipatory potential of interpretive inquiry. The second development, in which Habermas also played a central role, was the return of the Frankfurt School of "Critical Theory," led by Theodor Adorno, to

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[60][Berger and Luckmann, 1966]. See the early and important essay, [Berger, 1970].

[61]Sociology of knowledge "concerns itself with whatever passes for 'knowledge' in a society, regardless of the ultimate validity or invalidity (by whatever criteria) of such 'knowledge.'" [Berger and Luckman, 1966, 3].

[62][Gouldner, 1970].

[63]See, e.g., [Rieff, 1970].

[64][Gadamer, 1992].

Germany.[65] The key moment in its theoretical ascendancy was the famous "Positivism Dispute," which broke out at the 1961 Congress of German Sociologists.[66] From these debates, Habermas emerged as the major German social philosopher of the epoch.

In the 1960s in France, drawing on the tradition of Durkheim and Mauss and especially the linguistic theories of Saussure, structuralism overthrew the phenomenology and existentialism of Sartre and Merleau-Ponty. "Thinkers like Levi-Strauss, Roland Barthes, and the early Michel Foucault created a revolution in the human sciences by insisting on the textuality of institutions and the discursive nature of social action," Jeffrey Alexander observed.[67] Swiftly poststructuralism radicalized the scene, both problematizing the epistemological frames of structuralist semiotics and introducing a grim reckoning with the problem of domination: "Althusser converted texts into ideological state apparatuses. Foucault conflated discourse with dominating power."[68] Simultaneously or sequentially, the works of Lacan, of Derrida, and of Foucault swept the theoretical world. The "new" sociology of knowledge arose from these impulses, together with one distinctively American contribution: Thomas Kuhn's *The Structure of Scientific Revolutions*.

2 THE NEW SOCIOLOGY OF KNOWLEDGE: FROM KNOWLEDGE TO CULTURE

Since 1970 the field of the sociology of knowledge has witnessed several crucial developments which lay claim to radical novelty.[69] The first form of the "new" sociology of knowledge retained the traditional emphasis on the concept of *knowledge* — indeed, pursued it where the old sociology of knowledge had not thought to go: into the sphere of natural scientific knowledge. This has been the major site for explicit elaboration of sociology of knowledge, and it deserves detailed consideration. At the same time, contemporary sociology of knowledge has drawn upon a far vaster, tacit, interdisciplinary fund of thinking.[70] The development of this more diffuse and interdisciplinary "new" sociology of knowledge carries out a shift "from knowledge to culture." I will discuss three overlapping developments: cultural studies, feminism, and cultural sociology.

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[65]For a collection of primary texts in the Frankfurt School tradition, see, e.g., [Connerton, 1976].

[66]For the opening blast, see [Habermas, 1964]. For the whole dispute see [Adorno, 1976]. For a grander synthesis of the whole question of German Sociology, see Münch, 1993.

[67][Alexander, 1996, 4].

[68][Alexander, 1996, 4]. See pPurvis and Hunt, 1993], for the endless wrangles of Althusserian vs. Foucauldian terminologies.

[69]See, e.g., [Law, 1986; Swidler and Arditi, 1994; McCarthy, 1996; Evers, 2000].

[70]"Contemporary sociology of knowledge includes and draws upon the work of theorists like Foucault, Bourdieu and Elias as well as the varied approaches to the study of scientific knowledge. In addition, it includes the study of texts, as developed in literary studies, cultural studies and cultural theory." [Longhurst, 1989, x].

2.1   The Sociology of Scientific Knowledge

The first compelling formulation of a "new" sociology of knowledge emerged in the early 1970s. The intellectual energies for this "sociology of scientific knowledge" (SSK) gathered primarily in Britain among thinkers inspired by Thomas Kuhn and Ludwig Wittgenstein, not trained in mainstream sociology.[71] They proved willing to propose a most aggressive form of the "social construction of reality" and to challenge the positivist tradition in its most sacred space: the privilege of natural scientific knowledge.[72] They challenged not only the established field of sociology of science as developed by Merton and his followers, but also the Received View of the philosophy of science.

Barry Barnes and David Bloor of the Edinburgh Science Studies Unit provided the decisive elaboration. They promised to provide *causal explanations* of theory choice and change in science, and thus to achieve an *actual* history of science vastly superior to the conjectural history of the Received View. Bloor's essay, "Wittgenstein and Mannheim on the Sociology of Mathematics" [1973], formally announced the "strong programme of the sociology of knowledge."[73] In this seminal article, Bloor enunciated four key methodological principles — causality, impartiality, reflexivity, and symmetry — stressing, as he always would, the fourth and most controversial, symmetry.[74] In his monograph, *Knowledge and Social Imagery* [1976], Bloor elaborated on these four principles. He insisted that sociology, as science, had to offer *causal* explanations.[75] Second, sociology needed to investigate social beliefs without imposing the standards of the investigator upon the subject of investigation.[76] The third principle, reflexivity, suggested that the theory of sociology should not be immune to its own argument; it must be possible to conduct a sociology of sociology.[77] The final principle, symmetry, held that exactly the same causal methodology should apply to true beliefs as to false ones. That is, Bloor insisted that a strong sociology of scientific knowledge would reject the proposition that truth sufficed as an explanation for a belief. Bloor suggested Mannheim's earlier programme "faltered" before the application of sociology of knowledge to the sacrosanct domains of science, mathematics and logic. Recent research suggests that Bloor never fully realized the particular contextual and conceptual concerns which animated Mannheim's original form of the sociology of knowledge, and consequently misunderstood Mannheim's attitude toward natural

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[71]This intersection is caught well by Derek Phillips in 1975: "There are certain strong similarities in the writings of the philosopher, Ludwig Wittgenstein; the historian Thomas Kuhn; and several contributions of two sociologists of knowledge, Karl Mannheim and C. Wright Mills." [Phillips, 1975, 37].

[72]See [Zammito, 2004].

[73][Bloor, 1973, 174].

[74][Bloor, 1973, 173-4].

[75]Robert Nola asks one very pertinent question: "Is what is caused the *act* of x's believing that p, or the very *content* of the proposition that p which x believes?" [Nola, 1991, 108].

[76]More traditional treatments called this "value-free" or "objective" inquiry, and the problems associated with that notion are not unfamiliar. See [Weber, 1949].

[77]Such a "sociology of sociology" was already a reality. For an example, consider [Friedrichs, 1970].

science and mathematics.

For Bloor, knowledge was irretrievably a *social* construct, something "better equated with Culture than Experience."[78] Critics have suggested that it might have been more accurate still to suggest that what the Strong Programme proposed was a sociology of *belief*, for it proposed to efface any distinction between what a group *took* for knowledge and what, by some standard, it was *justified* in taking for knowledge.[79] Bloor proposed a drastic, essentially Durkheimian, theory of social determination. In his important essay "Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge," Bloor attempted to vindicate the long-disputed claim that "the classification of things reproduces the classification of men."[80] In Bloor's view, the "grid-group" model, developed by Mary Douglas on Durkheimian lines, offered the clearest formulation of "mechanisms for linking the social and the cognitive."[81]

Bloor's "Strong Programme" undertook to displace philosophy by sociology: its tenor and its reception cannot otherwise be accounted for.[82] In a 1973 essay, Bloor appropriated Peter Winch in starkly disciplinary terms: "Whereas Winch thinks that much sociology is misbegotten philosophy, the argument of this paper has been that much philosophy is misbegotten sociology ... Rather than philosophy illuminating the social sciences Winch unwittingly shows that the social sciences are required to illuminate philosophical problems."[83] An essay by John Law, "Is Epistemology Redundant? A Sociological View" [1975], explicitly proclaimed the agenda of SSK to be displacing philosophy.[84]

The problems with SSK were severe. Barnes took to heart the point that his Edinburgh colleague Steven Shapin stated so decisively: "The mere assertion that scientific knowledge 'has to do' with the social order or that it is 'not autonomous' is no longer interesting. We must now specify how, precisely, to treat scientific culture as social product."[85] The problem was how to establish a causal linkage. Barnes was candid enough to admit that grounding ideas in social structure had had "only very limited success."[86] "There simply is not, at the present time, any explicit, objective set of rules or procedures by which the influence of concealed interests upon thought and belief can be established."[87] In his second monograph, *Interests and the Growth of Knowledge* [1977], Barnes endeavored a hypothesis:

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[78][Bloor, 1991, 16].

[79]This criticism is made by a series of commentators, from Laudan to Friedman. See [Zammito, 2004].

[80][Bloor, 1982, 267].

[81][Bloor, 1978, 245].

[82]"Bloor has consistently made traditional philosophy and its 'rational' epistemology a particular target. The enterprise of the Strong Programme is conceived specifically as supplanting all traditional epistemology, with the sociology becoming 'heir to the subject that used to be called philosophy.'" [Slezak, 1991, 242] Slezak, in turn, is citing Bloor, [1983, 184].

[83][Bloor, 1973, 191n].

[84][Law, 1975].

[85][Shapin, 1979, 42].

[86][Barnes, 1974, 115]. This candor and skepticism may have distinguished Barnes from Bloor in the early days of the Strong Programme. See: [Manier, 1980, and Zibakalam, 1993].

[87][Barnes, 1977, 35].

"*all* knowledge, 'scientific,' 'hermeneutic' or otherwise, is primarily produced and evaluated in terms of an interest in prediction and control."[88] The decisive theoretical resource upon which he drew was Habermas's *Knowledge and Human Interests.*[89]

In a piece mercilessly entitled "Voodoo Epistemology," Paul Roth made the point that "whatever 'gaps' are evident in orthodox explanations of scientific behavior, the strong programmers have been far too sanguine in their conclusion that only sociology remains to fill the breach."[90] That is, "the problem is a question of the *causal* mechanism at work; . . . knowing where and at what to look in order to discern the engine driving scientific and social changes."[91] Ironically, faced with this basic methodological challenge, the Strong Programme appealed "in principle" to Quine's thesis on the underdetermination of theory by data, the Duhem-Quine Thesis (semantic holism), and the theory-ladenness of data — in short, the whole panoply of problematic post-positivist philosophy of science.[92]

Even as criticism of the "interest" explanation of SSK mounted, the whole structure of sociological explanation was being undermined. By the mid 1980s, despite the enthusiasm of its early successes, SSK shared fully in the shattering of faith in the categories of causal explanation of orthodox sociology. As Bruno Latour put it in *The Pasteurization of France*, "the evolution of our field has made the notion of a 'social explanation' obsolete."[93] Michel Callon explained, "The theoretical difficulty is the following: from the moment one accepts that both social and natural science are equally uncertain, ambiguous, and disputable, it is no longer possible to have them playing different roles in the analysis. Since society is no more obvious or less controversial than nature, sociological explanation can find no solid foundation."[94] Latour was emphatic: "notions like 'context', 'interest', 'religious opinion', 'class position', are ... part of the problem rather than of the solution."[95]

The summary verdict pronounced by Michael Lynch has reverberated widely throughout the social study of science:

> Sociology's general concepts and methodological strategies are simply overwhelmed by the heterogeneity and technical density of the

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[88][Barnes, 1977, 15].

[89][Habermas, 1971]. Barnes was influenced in his reading of Habermas particularly by Mary Hesse's review of the work, "In Defense of Objectivity" (1972), reprinted in [Hesse, 1980].

[90][Roth, 1987, 173n].

[91][Roth, 1987, 173n].

[92]"Taking their philosophical cues from Kuhn's account of scientific revolutions (in particular, the claim that paradigm changes are more akin to conversions than to reasoned judgments), Wittgenstein's insistence on the thoroughly social and conventional determinations of language use, and Quine's and Duhem's claims that theories can always be revised to accommodate seemingly adverse evidence, advocates of the strong programme conclude that neither reason nor fact (both of which they view as conventionally determined anyway) serve to explain the choice of one scientific theory over another." [Roth, 1987, 154].

[93][Latour,1988, 256]; see [Schaffer, 1991, 185].

[94][Callon, 1986, 199].

[95][Latour, 1990, 155].

languages, equipment, and skills through which mathematicians, scientists, and practitioners in many other fields of activity make their affairs accountable. It is not that their practices are asocial, but that they are more thoroughly and locally social than sociology is prepared to handle.[96]

By the end of the 80s, Andrew Pickering came to reject the "science as knowledge" agenda of SSK as "thin, idealized, and reductive."[97] More concretely, "SSK simply does not offer us the conceptual apparatus needed to catch up the richness of the doing of science, the dense work of building instruments, planning, running, and interpreting experiments, elaborating theory, negotiating with laboratory managements, journals, grant-giving agencies, and so on."[98] Karin Knorr-Cetina also found the "interests model" inadequate.[99] The "macroscopic congruency claims" involved in the interests model, she averred, "do not specify the causally connected chain of events out of which an object of knowledge emerges congruent with antecedent social interests."[100] To get the requisite concreteness, Knorr-Cetina argued, science studies needed to "adopt a *genetic* and *microlevel* approach."[101]

There was an insistent shift from the macro-level (society and science as wholes) to the micro-level (a laboratory site or a "core set" of disciplinary specialists) and an even more adamant insistence upon considering what scientists *do* (practice), as contrasted with what they *say* (discourse, or in a more traditional vein, theory). Thus the distinctive move in recent science studies has been the shift from conceiving of science as *knowledge* to conceiving of science as *practice*.[102] Finally, the theoretical hallmark of this shift in science studies was the radicalization of the relativism of the original Strong Programme toward a *social constructivism* "all the way down."[103] The new social constructivists pursued the agenda of discrediting the claims of the *natural scientists themselves* that their knowledge somehow found corroboration from nature.[104] For this new and drastic school, science was *only* construction.

The influential laboratory study of Latour and Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* contained in its subtitle what Ian Hacking termed "a manifesto in its own right."[105] Latour and Woolgar insisted: "It is

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[96] [Lynch, 1992, 298].

[97] [A. Pickering, 1992, 5].

[98] [A. Pickering, 1992, 5].

[99] She objected: "interests are not generally obvious to agents themselves ... interests, like other phenomena, appear to be negotiated and accomplished in social action rather than to simply 'exist' ... 'objectively' attributed and 'subjectively' perceived interests do not always coincide ... a question as to who may or may not legitimately identify somebody's interests, and on what grounds." [Knorr-Cetina, 1982, 129 n 32].

[100] [Knorr-Cetina, 1983, 116].

[101] [Knorr-Cetina, 1983, 116-117].

[102] A. Pickering, 1992, 2.

[103] [A. Pickering, 1992, 1].

[104] "The whole rationale of the sociology of science is to challenge scientists' taken-for-granted assumptions." [Delamont, 1987, 167].

[105] [Latour and Woolgar, 1986]. Hacking wrote about it some ten years later because it had

not simply that phenomena *depend on* certain material instrumentation; rather, the phenomena *are thoroughly constituted* by the material setting of the laboratory."[106] They explained: "We do not wish to say that facts do not exist, nor that there is no such thing as reality ... Our point is that 'out-there-ness' is the *consequence* of scientific work rather than its *cause.*"[107] This was the lynchpin of the *constructivism* elaborated in *Laboratory Life*. Hacking makes clear the radical form of the constructivism advocated here by distinguishing between the "anti-realism" of what can be called the instrumentalist tradition in analytic philosophy, most recently exemplified by Bas Van Fraassen, and a radical "irrealism" best represented by Nelson Goodman. Irrealism is the position that the "world" is entirely *made*, i.e., the restriction of all claims to a scheme or framework.[108] "Latour and Woolgar fit together with Goodman quite well."[109] Knorr-Cetina's initial formulations of her constructivist program for science studies were quite close to the instrumentalism of Bas Van Fraassen's "constructive empiricism," but she swiftly moved away toward a more radical posture.[110] "Strong" constructivism, she urged, disdained accommodation with philosophy of science: "The constructivist thesis in the original laboratory studies . . . shifted the question from . . . realist, instrumentalist and such-like doctrines, to an enquiry into the constructive process of world making."[111] The resonances, here, of a departure from the anti-realism of Van Fraassen to the irrealism of Goodman's "world making," as Hacking noted in Latour and Woolgar, should not be missed.

The displacement of the Strong Programme in science studies came at the hands of its own progeny, but proliferation brought with it striking divisiveness. "The 'enemy' is no longer positivist and empiricist philosophy without, but heretical social theories within, the field."[112] This fairly young research specialty has gone through a series of revisionisms, each more radical than the last.[113] In a crucial essay entitled "Epistemological Chicken" [1992], Harry Collins and Steven Yearley tell us that "since the mid-1970s each new variant of SSK has tended to be a

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so deeply stamped the field [Hacking, 1988, 277]. Sergio Sismondo calls it "the best-read of the laboratory ethnographies and a paradigm of constructivism" [Sismondo, 1993, 532]. Jan Golinski, too, comments on "the lasting influence" of the work [Golinski, 1990, 496].)

[106][Latour and Woolgar, 1986, 64].

[107][Latour and Woolgar, 1986, 180n].

[108][Hacking, 1988, 282]. See [van Fraassen, 1980; Van Fraassen and Churchland, 1985] which explores his thought; for Goodman, see [Goodman, 1978].

[109]Hacking, 1988, 282.

[110][Sismondo, 1993, 528].

[111][Knorr-Cetina, 1993, 561].

[112][Pleasants, 1997, 144-145].

[113]In the rhetoric of the "radical" wing, the so-called "reflexivists," a flamboyant avant-gardist "progressivism" is brandished to cudgel all resistors, even as the entire practice is then disavowed as "ironic." This avant-gardism characterizes postmodernism as a rhetorical pose. Always to propose to "go beyond" the current, to "make it new," is every whit as *post*modern as it was "high modern." For a signal instance of this see [Woolgar and Ashmore, 1988, 10]: "I am concerned that readers will miss the irony of our progressive account. They may suspect that (deep down) we actually like the possibility that reflexivity is an advance on previous approaches..." For a blatantly avant-garde posture (or should one say "positionism"?) see [Woolgar, 1992].

little more radical than the one before it. Each new variant has stood longer on the relativist road."[114] "The escalation from relativism through discourse analysis and 'new literary forms' to reflexivity [is] in the end ... [a] regress [which] leads us to have nothing to say."[115] In his crucial intervention, "One More Turn After the Social Turn," Latour gave voice to a sense of crisis in the field: "After years of swift progress, social studies of science are at a standstill."

> A few, who call themselves reflexivists, are delighted at being in a blind alley; for fifteen years they had said that social studies of science could not go anywhere if it did not apply its own tool to itself; now that it goes nowhere and is threatened by sterility, they feel vindicated.[116]

The game of "epistemological chicken" over *symmetry* demystified more than natural science: it brought a general crisis of confidence about *any* claims to knowledge.

This brought to the fore the key notion of *reflexivity*. Steve Woolgar, the main proponent of "reflexivity" in science studies, clearly affiliates this project with French post-structuralism and postmodernism generally:

> The grounds for knowledge have come under increasing challenge within a wide range of disciplines — anthropology, psychology, sociology, philosophy — and more recently in a number of intellectual movements which share a concern for the "problem" of representation and which cut across traditionally defined disciplinary boundaries — poststructuralism, postmodernism, literary theory, and so on.[117]

Under the aegis of this battery of movements, "the word-object relationship, once the paradigm of representation, is displaced by lateral, syntagmatic, and reflexive relations between communicational 'elements' in seemingly anarchistic fields."[118]

From the vantage of ethnomethodology, from the vantage of actor-network theory, or from the vantage of radical reflexivity, the claim of the sociologist to "an epistemic authority and 'ontological reality' (for social phenomena) which s/he denies the natural scientist" for the physical world appeared an insupportable "conceit."[119] John Law penetratingly characterized the situation in 1986 as a "crisis in the sociology of knowledge." It had entered upon a new surge of energy in the postwar era through the ideological analysis of Louis Althusser, the grid-group theory of Mary Douglas, and above all the Strong Programme of SSK, but now "the second phase is in crisis. A third phase is upon us in the form of work that has gone some way to eroding the basis of the sociology of knowledge as this

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[114][Collins and Yearley, 1992, 303]. I take this exchange between Collins and Yearley and Woolgar to be the climactic showdown in the internecine wars within the sociology of scientific knowledge.

[115][Collins and Yearley, 1992, 302].

[116][Latour, 1992, 272].

[117][Woolgar, 1989, xviii].

[118][Lynch and Woolgar, 1990, 2]. Woolgar finds this fully "consistent with the position of the idealist wing of ethnomethodology" which he already endorsed. [Woogar, 1986, 312].

[119][Pleasants, 1997, 149].

has been traditionally conceived."[120] What had come under attack was "the idea
that there is a backcloth of relatively stable social interests which directs knowl-
edge or ideology." Such a stable backcloth had allowed sociological explanation
of beliefs in terms of structure, but now, especially under the impact of Michel
Foucault, "structure has collapsed into knowledge in the form of discourse, and
the sociology of knowledge (if this is still an acceptable title for an inquiry that
has so extensively chopped away at its own foundations) has been refocused" on
"the *technique* of power/knowledge."[121]

   With the crisis of sociological explanation, with the abandonment of empirical
accounts by "discourse analysis," and, most importantly, with the articulation of
"new literary forms" and radical reflexivity, the whole enterprise of a *sociology*
of scientific knowledge reached a fatal impasse.[122] The initiative in the study of
science passed to a different disciplinary cadre — the ethnographers — and to a
different theoretical orientation — *"cultural studies."*[123] David Stump wrote of
a "second round" of revisionism. "This time the skeptical arguments come from
sociologists and anthropologists of science and their allies, the cultural histori-
ans," and the target was not simply philosophy of science but also the sociology
of scientific knowledge.[124] Thus, writing in 1992, Timothy Lenoir conceived of "a
much broader account of science as culture that many of us think science studies
is verging toward," dominated by cultural studies and feminist studies. This revi-
sionist current drew inspiration from Michel Foucault but accentuated, against his
discursivity, the materiality of the body and subject in knowledge and agency.[125]
In particular, feminist criticism played a major role. Donna Haraway wrote that
science studies had not learned from "semiotics, visual culture, and narrative
practice coming specifically from feminist, postcolonial, and multicultural oppo-
sition theory," and that "it is past time to end the failure of mainstream and
oppositional science-studies scholars to engage each others's work."[126] Agreeing
with Haraway, Lenoir asserted that "science studies, at least the tradition of the
sociology of scientific knowledge, has not paid sufficient attention to the work of
feminist historians."[127]

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[120][Law, 1986, 2].

[121][Law, 1986, 18].

[122]Donna Haraway writes of a "crisis of confidence among many 4S scholars that their very
fruitful research programs of the last 10 years are running into dead ends. They are." [Haraway,
1992, 336].

[123]Mario Biagioli, introducing his key anthology, *The Science Studies Reader*, suggested "con-
trasting science studies with ethnography" to highlight the major novelty after 1990. [Biagioli,
1999, xiii].

[124][Stump, 1996, 444].

[125][Lenoir, 1999, 292].

[126][Haraway, 1996, 438]. Elsewhere Haraway pours scorn upon "the abject failure of the social
studies of science as an organized discourse to take account of the last twenty years of feminist
inquiry," or indeed to show *"any* consideration of matters like masculine supremacy or racism
or imperialism or class struggle" [Haraway, 1992, 332]. Haraway sharply critizes Bruno Latour
for these omissions, despite her longstanding recognition of his theoretical achievements for her
own approach to hybridity.

[127][Lenoir, 1999, 292].

The key landmarks of this passage within science studies were the conference held at Stanford in 1991 resulting in the volume, *The Disunity of Science* [1996], and the publication by Routledge, at the end of that decade, of a "state of the art" anthology, *The Science Studies Reader* (1999) edited by Mario Biagioli.[128] Between the conference and the publications fell the media event of the "science wars" — the publication of *Higher Superstition: The Academic Left and Its Quarrels with Science* (1994) by Paul Gross and Norman Levitt, the responding special issue of *Social Text* on the "science wars," and the "Sokal Hoax" which was nested in the latter.[129] This episode, more pertinent to science studies specifically than to the sociology of knowledge, revolved round the allegation of an adversarial relation between the new "cultural studies of science" and practicing natural scientists.[130] For Gross and Levitt, cultural studies of science were flat-out anti-scientific.[131]

Joseph Rouse offered a defense of cultural studies of science which denied this claim and accentuated the fruitfulness of the new approach.[132] In recommending "interdisciplinary cultural studies of science" as an alternative both to philosophy of science and to the sociology of scientific knowledge, Rouse wrote, "I hope to convince readers of the need for different answers to different questions about the sciences."[133] He offered his formulation not "as a strikingly new way to think about the sciences but as an attempt to formulate a more philosophically and politically satisfying interpretation of the more recent achievements of constructivist sociology, contextualist history, and feminist theory of science."[134] His work and the works he cited instituted a shift from the *sociology* of scientific knowledge to a new *culturalism.* "*'Culture'* is deliberately chosen both for its heterogeneity . . . and for its connotations of structure or fields of meaning," Rouse noted.[135] Writing a decade later, Rouse affirmed that "the work of cultural historians, anthropologists, and feminist theorists of science has taken post-constructivist science studies in important new directions."[136]

He identifies as "most central to empirical work in recent science studies . . . the embodiment of scientific understanding in laboratories and material practice, in non-verbal images and models, and in the textual materiality of language."[137] What these have in common is "*denial that nature and society or culture are self-contained components of the world*" in favor of a far more pervasive "*intimacy* of material nature and human social life."[138] As Rouse elaborates, "the basic model of knowledge operative here is that of active but vulnerable bodies seeking

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[128][Galison and Stump, 1996; Biagioli, 1999].
[129][Gross and Levitt, 1994; Ross, 1996; *Lingua Franca*, 2000].
[130]For a discussion of the episode in that context, see [Zammito, 2004, 251-270].
[131]For a wider circle of commentators sharing this view, see: [Gross, Levitt and Lewis, 1997; Koertge, 1998].
[132][Rouse, 1993].
[133][Rouse, 1996, 33, 37].
[134][Rouse, 1996, 39].
[135][Rouse, 1996, 238].
[136][Rouse, 2002, 62].
[137][Rouse, 2002, 68].
[138][Rouse, 2002, 69].

to render their partially shared circumstances more reliable and less threatening, more comprehensible and less alien."[139] That entails recognizing "there is no clear boundary between 'us' and 'not-us'."[140] For Rouse, the new approach entails a radical new temporality in scientific practices: "if one takes seriously the material, institutional, and discursive embodiment of scientific understanding, scientific research runs ahead of what it can already clearly articulate."[141] Rouse contends not only that the ideal of transcendental impartiality is incoherent but also that "knowledge" is not a coherent domain of inquiry. Rouse rejects traditional philosophy of science but also "naturalized epistemology" and, most drastically of all, empirical *explanation* in history or the sociology of scientific knowledge, as well. He embraces a "*post-epistemological* conception of science and scientific knowledge," as part of a general "deflationary approach to knowledge."[142] This is an extremely ambitious reading of cultural studies and its approach to science. To assess it, we must consider cultural studies more widely, then focus in on the key theorists Rouse highlights as cutting edge.

2.2   *Cultural Studies*

For much of its early trajectory, the sociology of scientific knowledge remained strikingly distinct from and largely indifferent to a wider form of the "new" sociology of knowledge, cultural critique associated with poststructuralist "theory."[143] "Cultural Studies" seems the most plausible rubric for this other, "interdisciplinary" current of "new" sociology of knowledge, from knowledge to "culture."[144] As Victoria Bonnell and Lynn Hunt put it, "almost anything can fall under the rubric of cultural studies."[145] One recent self-reflection captures this well: "The work that presents itself as cultural studies — a loose affiliation of interdisciplinary research initiatives and political projects that rarely claim a history much earlier than Gramsci — spans a wide variety of subjects and concerns," and is best understood as an "intervention," not a "conversation."[146] What gives cultural studies a modicum of coherence is that it strives to "deal fundamentally with issues of domination, that is, contestations of power." It is always concerned with "demystification and deconstruction of power."[147] There are two other commonalities: first, an effort to situate structural changes in contemporary societies under the

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[139][Rouse, 2002, 71].
[140][Rouse, 2002, 71].
[141][Rouse, 2002, 75]. Here, Rouse draws on H[ans-Jörg Rheinberger, 1999].
[142][Rouse, 1996, 211].
[143]"Science studies, focused on the edifying object of 'modern' scientific practice, has seemed immune from the polluting infections of cultural studies — but surely no more." [Haraway, 1992, 330].
[144]Alexander offers a critical appraisal of this cultural turn in "General Theory in the Postpositivist Mode: The 'Epistemological Dilemma' and the Search for Present Reason," ch. 3 of [Alexander, 1995].
[145][Bonnell & Hunt, 1999, 10].
[146][Dean, 2000, 2].
[147][Bonnell & Hunt, 1999, 10-11].

rubric "postmodernity," and, second, recourse to a pervasive "theory" of language and textuality drawn from poststructuralism.[148]

Just as with SSK, the point of origin of cultural studies lies in the upheavals of the 1960s, but the impetus is very different. Cultural studies responded directly to the rise of the so-called "new social movements" and the restive political dissent associated with the New Left.[149] In part because the proletarian redemption embraced by the old Marxism had lost its luster, the quest for alternative sites of resistance to oppression and for articulation of alternative political futures proved coterminous, especially for dissident, if mostly academically-ensconced intellectuals. The key issues of race, class, and gender, as these have become canonized in contemporary discourse, followed out of the rise of the "new social movements," but with the ebbing of their wider social efficacy, they have tended to become increasingly academicized and persist as a radical agenda within (or better, against) the disciplinary structures of academia as "cultural studies."

The establishment of the Center for Cultural Studies at Birmingham in 1964, to be led for a decisive decade starting in 1969 by Stuart Hall, connected not only to a "crisis of the humanities" over the democratization of "culture" in Britain, but also to the crisis of Marxism: the decay of the old Left and the search for alternatives by the New Left. British cultural studies endeavored to shift the focus of inquiry from elite to popular culture, then to the problematics of "mass culture" in the line of Adorno and Horkheimer's *Dialectic of Enlightenment* and above all Antonio Gramsci's theory of hegemony.[150] The thrust of the major works of the movement was to highlight the theme of subcultural resistance.

The painful process of adjusting the local tradition of British cultural studies to the powerful insights of "structuralism" forms the central narrative of Hall's reconstruction of British cultural studies. The challenge started with structuralism and semiotics. Lévi-Strauss first upset the thinking of British cultural studies, to be followed more famously by theoretical confrontation with Althusserian Marxism.[151] Richard Johnson confirms this narrative in his own account, "What is Cultural Studies Anyway?" [1986].[152] Hall and Johnson recognize that the structuralist insights needed to be incorporated, but make clear this entailed losses as well as gains. The prospect of a coherent "culture," even in a determinate empirical instance, proved an evident casualty of this learning process.[153] Deep structures "behind the back" of the subject — above all, language — constituted subjectivity: this much British cultural studies accepted from structural-

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[148][Chaney, 1994, 42-88].
[149][Laraña, Johnston and Gusfield, 1994; Pichardo, 1997].
[150][Adorno and Horkheimer, 1972; Gramsci, 1978].
[151][S. Hall, 1980, 529]. The famous quarrel over Althusser and theory in British Marxism thus played a central role in the development of Cultural Studies. See [Johnson, 1979].
[152][Johnson, 1986/87, 50].
[153]Hall writes that it became impossible to bring "closure" to the conceptual sphere of culture [S. Hall, 1992, 284], and Johnson argued not only that culture could not be made a precise category but that no single discipline could encompass its various crucial aspects [Johnson, 1986/87, 42]. Thus, Cultural Studies became "interdisciplinary" by default. That is became *anti*-disciplinary as well is another matter.

ism. But it welcomed the post-structuralist supplement that this constitution was "always already" internally contested and thus dynamic, not static. Moreover, the British never forgot that subjectivity was affected by external forces beyond language (i.e., materiality). Ultimately, as Johnson put it, British cultural studies wanted a "post-post-structuralist" account of subjectivity: "not to deny the major structuralist or post-structuralist insights: subjects *are* contradictory, 'in process', fragmented, produced. But human beings and social movements also strive to produce some coherence and continuity, and through this, exercise some control over feelings, conditions and destinies."[154] In short, *agency* needed to be retrieved. Balancing structure with agency proves the master theme of contemporary social theory, of the "new" sociology of knowledge.

Richard Johnson observed, with a reflexive candor, "recognition of the forms of power associated with knowledge may turn out to be one of the leading insights of the 1970s."[155] Academic situatedness entailed political constraints, as Michel Foucault and Pierre Bourdieu theorized disturbingly. It accounted, in short, for a double marginality of British cultural studies: *vis à vis* the elite academic establishment, and also, more painfully, *vis à vis* elements in society with which it sought solidarity. Despite its achievements, then, there is a wistfulness about the retrospective accounts of British cultural studies by its leading exponents. The most important theoretical recourse for Hall and the Birmingham Center was Gramsci, not least because he also had been someone in the Marxist tradition struggling to overcome its obvious inadequacies while remaining within its political ranks.[156] What drew Hall was Gramsci's idea of the "organic intellectual".[157] For Hall, Gramsci's inspiration was direct and poignant: "we were trying to find an institutional practice in cultural studies that might produce an organic intellectual."[158] Hall noted that just as "movements provoke theoretical moments," intellectuals, to become organic, had to identify with and inform actual historical movements. The "historical conjuncture" never emerged.[159] "We never connected with that rising historic movement," Hall admitted.[160] "We couldn't tell . . . where [such a movement] could be found."[161] Thus, "we were organic intellectuals without any organic point of reference; organic intellectuals with a nostalgia or will or hope . . ."[162] In fact, he admitted, "we never produced organic intellectuals."[163] It was "a metaphorical exercise," though of course "metaphors [still]

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[154][Johnson, 1986/87, 69].
[155][Johnson, 1986/87, 40].
[156][S. Hall, 1992, 280].
[157]See [Harris, 1992].
[158][S. Hall, 1992, 281].
[159][S. Hall, 1992, 283].
[160][S. Hall, 1992, 282].
[161]S. Hall, 1992, 281. "In my view, a key problem was the absence of political contexts — parties or movements — that would like academic radicals and popular experiences and develop what Gramsci called a 'popular,' 'collective,' or 'national-popular collective will'..." [Johnson, 1997, 457].
[162][S. Hall, 1992, 281].
[163][S. Hall, 1992, 282]. "The aspiration, among cultural studies intellectuals to be 'organic' to

affect one's practice."[164] In reality, Foucault's model of the "specific intellectual," situated in a mode of expertise, appeared to describe the British situation more accurately.[165] "It's the organic intellectual, metaphorically, as the hope, and it's the specific intellectual as the mode of operation."[166]

Like British cultural studies, the American variant arose within the traditional humanities, specifically in university English departments.[167] This new discourse, which has become hegemonic within literary studies, sees itself coterminous with many other "micro" analytic practices across the disciplines, all of which appear to pursue "culture" in the anthropological sense, as "situated knowledge(s)."[168] Its "interdisciplinarity" often does not involve learning techniques from these other disciplinary practices, however, but rather settles for aggressively disputing disciplinarity as such, as if the denunciation of disciplinary closure makes up for the absence of disciplinary rigor.[169] Hall was quite open in expressing bafflement: "In the United States . . . 'cultural studies' has become an umbrella for just about anything."[170] And again: "I'm not sure that cultural studies in the United States has actually been through that moment of self-clarification..."[171] Hall registered a "moment of danger" in the triumphal institutionalization of American cultural studies, which he associated strikingly with its extraordinary "theoretical fluency": "There is no moment now, in American cultural studies, where we are *not* able, extensively and without end, to theorize power — politics, race, class, and gender, subjugations, domination, exclusion, marginality, Otherness, etc. ... [T]here is the nagging doubt that this overwhelming textualization of cultural studies' own discourses somehow constitutes power and politics as exclusively a matter of language and textuality itself."[172] As one rueful reflection concluded, the result has often been very "thin" description.[173] The "new historicism" strikes this "old" historicist, with rare exceptions, as a case in point.[174]

There is a very strong connection between "cultural studies," as it has evolved in the United States, and adamant advocacy of French poststructuralism. In his con-

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the experience and everyday needs of popular groups was always to some degree unrequited." [Johnson, 1997, 458].

[164] [S. Hall, 1992, 282].

[165] [Morris, 1991, 17-18].

[166] [S. Hall, 1992, 292]. For a thoughtful consideration of this phenomenon from an Australian perspective, see [Morris, 1991, 10-11].

[167] "Literary theorists have decisively shaped the transplantation of British cultural studies in the United States." [Seidman, 1997, 41].

[168] "Cultural Studies often look like work done in micro-sociology, in discourse analysis or conversation analysis, in semiotics, in legal studies, in social history or in social anthropology." [Frow, 1991, 27].

[169] "Athough most scholars see cultural studies as antidisciplinary, they should not for a minute think it is nondisciplinary," Ellen Messer-Davidow asserts [Messer-Davidow, 1997, 492]. On the contrary, we should think about that long and hard.

[170] [S. Hall, 1992, 22].

[171] [S. Hall, 1990, 292].

[172] [S. Hall, 1992, 286].

[173] M. Pickering, 1997, 3.

[174] [Zammito, 1993].

tribution to a volume notably entitled *From Sociology to Cultural Studies* [1995], Steven Seidman notes this: "cultural studies seems to parallel French 'postmodern' theory in viewing the new role of the mass media, the saturation of daily life by commerce and commodification, the new technologies of information, and the foregrounding of cultural politics as signaling perhaps a second 'great transformation' in post-Renaissance Western societies."[175] Seidman wishes to draw upon cultural studies to "challenge" disciplinary sociology to accept the fundamental poststructuralist epistemological and theoretical positions — the semiotics of Barthes and Foucault, the psychoanalysis of Lacan, etc. — as more socially-politically emancipatory.[176] Seidman abjures any claims to ultimate warrant or even empirical adequacy and pitches his case entirely on "pragmatic" grounds.[177] On similarly *pragmatic* grounds, however, one may well dispute the benefit of buying into Foucault, Lacan, and their brethren at anything like the rate of (cultural) exchange at which American postmodernists have valued them. It seems to me that we are well underway to a decisive *deflation* of their intellectual value.

The recognition that American cultural studies drifted too far towards this cultural-structural determinism ("social constructionism" all the way down) has begun to surface within the movement itself. Brian Doyle, for one, argues that the "culture" of cultural studies needs to be transformed to bring agency back in.[178] Meaghan Morris observes: "If a cultural dopism is being enunciatively performed (and valorized) in a discourse that tries to contest it, then the argument in fact *cannot* move on, but can only retrieve its point of departure as 'banality' (a word pop theorists don't normally use) in the negative sense."[179] Another recent collection calls for cultural studies to get "back to reality," by which is meant concrete political engagement.[180] The "moment of danger" Hall discerned in American cultural studies has proven quite real, as a leading American representative avows: "Cultural studies has entered the fast track of academic success

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[175] [Seidman, 1997, 45].

[176] "A Foucauldian perspective shifts the ground of social analysis to a focus on the making of bodies, desires, and identities, to power/knowledge regimes and to dynamics of moralizaton, discipline, and surveillance." (Ibid., 46) "Psychoanalysis offers a language of an intricate, dense, psychic, and intersubjective life, a life of fantasies, wishes, fears, shames, desires, idealizations, identification, that cannot be comprehended by a vocabulary of interests, means-ends rationality, cost-benefit calculations, need dispositions, or values, or by the surface psychologies of behaviorism, cognitivism, or symbolic interactionism" [Seidman, 1997, 49]. Thus, "cultural studies departs from these assumptions by imagining an individual as socially produced; as occupying multiple, contradictory psychic and social positions or identities; and by figuring the self as influenced by unconscious processes." (Ibid., 46) And "cultural studies places struggles over meanings, identities, knowledges, and the control of discursive production and authorization on an equal footing with struggles over the distribution of material resources." (Ibid., 55)

[177] "Deconstructing 'the empirical' means that we recognize that discursive conflicts, even ones about the social, can never be resolved by appeals to the empirical alone, and that such discourses should acknowledge their own entanglement in power and therefore, at times, when pragmatically useful, bring ethical reflection and political considerations into social discourse" [Seidman, 1997, 58].

[178] [Doyle, 1995].

[179] [Morris, 1988, 19].

[180] [McRobbie, 1997].

in the United States. But the cost may be too high . . ."[181] American cultural studies often seems happiest just *theorizing* "social construction" in general.[182] Even more self-defeatingly, in its politics, "too often ... cultural studies has either romanticized marginality or at least ethicised it as a new standard of political or even intellectual judgment."[183] Todd Gitlin minced no words, terming this "a grim and hermetic bravado which takes the ideological form of paranoid, jargon-clotted, post-modernist groupthink, cult celebrations of victimization and stylized marginality."[184] In less charged language, Patrick Brantlinger reaches the same conclusion, that cultural studies labors under the illusion "that highly specialized, in-house expressions of presumably radical ideas are somehow equivalent to political acts, with power to influence the real (that is, the nonacademic) world."[185]

Cultural studies in the United States, as in Britain, aimed to be an intervention, not just a research initiative. The ambition was that "resisting intellectuals can provide the moral, political and pedagogical leadership for those groups which take as their starting point the transformative critique of the conditions of oppression."[186] Like the British, these academic leftists sought to "forge allegiances around new historical blocs," to participate in "oppositional public spheres."[187] But in the United States, cultural studies has become merely an academic pursuit, in all the senses of that phrase.[188] As Brantlinger puts it, "the 'left academy' is . . . marginalized even within the academy and virtually invisible to the general public."[189] The one sphere of cultural studies where its intellectual articulation has not lost contact with a wider social movement and a real political-cultural impact has been feminism. "Academic feminism has a clear sense of a public beyond the academy which it both addresses and represents."[190]

Though cultural studies generally appear "both diverse and contested," what may be of most interest for the "new" sociology of knowledge is the trend Joseph Rouse identified as "cultural studies of science." To appraise it, it seems appropriate to consider not only Rouse's own work, but samplings from the body of

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[181][Grossberg, 1997, 7]. Elizabeth Long notes, as well, that the integration of cultural studies "as a strictly *academic* endeavor in the United States may be reshaping cultural studies as another narrow specialty, detaching it from the broader realm of political relevance and social responsibility." (Long, 1997, 14)

[182][S. Hall, 1992, 286; M. Pickering, 1997, 9].

[183][Grossberg, 1997, 15].

[184][Gitlin, 1993, 21]. "After the late 1960s, as race, gender (and sometimes class) became the organizing categories by which critical temperaments have addressed the world in the humanities and social sciences, faculty working this territory came to display the confidence of an ascending class speaking predictably of 'disruption,' 'subversion,' 'rupture,' 'contestation,' and the 'struggle for meaning.' The more their political life is confined to the library, the more aggressive their language." (Ibid., 19.)

[185][Brantlinger,1990, 17].

[186][Giroux et al, 1985, 480].

[187][Giroux et al, 1985, 481].

[188]See the harsh complaint by a political intellectual: [McChesney, 2002].

[189]Brantlinger, 1990, 128.

[190][Brantlinger, 1990, 129].

recent work which he takes to exemplify this new approach.[191] Two scholars who
feature prominently in Rouse's accounts are Donna Haraway and Karen Barad.
Another key theorist along these lines is N. Katherine Hayles. It is not insignifi-
cant that these are all feminist theorists, for the question arises whether — *apart
from feminism* — there is a significant theoretical achievement to be associated
with cultural studies — especially for the sociology of knowledge.

While Rouse has endeavored to construe the developments of the last decade
and more in terms of a cultural studies of science that is wider than feminist
science studies, he has also been attentive to the prominence of feminism in the
new wave. Thus, in his essay "Feminism and the Social Construction of Scientific
Knowledge," Rouse sets out systematically to juxtapose feminist science studies
to the sociological approach in which he affirms "that gender and the sex/gender
distinction play[ed] almost no role."[192] Feminist science studies share three im-
portant premises with the sociology of scientific knowledge — that science is a
social achievement, that traditional epistemologies are "not merely false, but ide-
ological" in privileging the authority of science, and that, consequently, science
studies should be politically engaged. Nevertheless, Rouse sees feminist science
studies "opposing a framework *shared* by traditional philosophers of science and
the new sociology of scientific knowledge," namely, the "*epistemological* conception
of their project."[193] For Rouse, feminism shares with postcolonial anthropology a
consciousness that "scientific knowledge [is] itself a form of action" with material
consequences.[194] Not only is this political critique, it entails as well a "concern to
understand knowledge as embedded within specific ways of engaging the world."[195]
Feminist science studies shift from a concern with "content" of scientific knowledge
to a "concern with *relationships* among knowers and known." According to Rouse,
feminists "have generally eschewed detachment, and the explanatory project, in
favor of a participatory stance."[196] That is, "what is *at issue* in feminist accounts
is not scientific knowledge as a totality, but particular scientific practices, projects,
and claims, that are understood as ongoing interactions among knowers and the
world known."[197] They "raise questions about how knowledge claims and practices
of inquiry become *significant* and authoritative."[198] Instead of a concern with how
science got to be what it is now, they are concerned with its future possibilities.
They "dissolve any sharp distinction between epistemic and political criticism."
And they adopt a "more adequate conception of reflexity." Accordingly, Rouse sees
"feminist scholars working toward *postepistemological* conceptions of knowledge,

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[191]Rouse specifically mentions "the work of Donna Haraway, Sharon Traweek, Mario Biagioli,
Robert Marc Friedman, Vassily Betty Smocovitis, or Paula Treichler" [Rouse, 1996b, 178].
[192][Rouse, 1996b, 196].
[193][Rouse, 1996b, 202, 198].
[194][Rouse, 1996b, 203].
[195][Rouse, 1996b, 204].
[196][Rouse, 1996b, 205].
[197][Rouse, 1996b, 211].
[198][Rouse, 1996b, 209].

evidence, justification, and objectivity."[199]

Donna Haraway's essay, "The Promise of Monsters: A Regenerative Politics for Inappropriate/d Others," was contributed to a benchmark anthology that set out to showcase the achievements of cultural studies in America, and she emphatically aligned her own work with cultural studies.[200] As much as her famous "Cyborg Manifesto" a decade earlier, this essay is a manifesto: a programmatic and polemical stance-taking, highly critical not only of an established order she finds "impossible but all-too-real," but of earlier approaches to science studies as well. Her attack goes against both those who believe there is a nature "out there" apart from human experience as well as those who think nature is only a text. For Haraway, nature needs to be reinterpreted as a *topos* and a *tropos*: as a question of places, "locations that are widely shared, inescapably local, worldly, enspirited," but also as a question of tropes: "figure, construction, artifact, movement, displacement."

She insists upon "artifactualism," a synthetic approach that entails "effects of connection, of embodiment, and of responsibility." Rejecting "sun-worshipping stories about the history of science and technology as paradigms of rationalism," she advocates under artifactualism "a co-construction among humans and non-humans." For this she invokes as well "the term 'material-semiotic actor' to highlight the object of knowledge as an active part of the apparatus of bodily production. . . [B]odies as objects of knowledge are material-semiotic generative nodes. Their boundaries materialize in social interaction among humans and non-humans."[201] Haraway accordingly insists upon "the corporeality of theory." Moreover, since "ways of life are at stake in the culture of science," there must be "accountability and responsibility for translations and solidarities."[202] Above all, she expresses a hope for change, hence her emphasis on "the generation of novel forms." Putting this all together, she writes: "Perhaps our hopes for accountability for techno-biopolitics in the belly of the monster turn on revisioning the world as coding trickster with whom we must learn to converse."[203] That is, "the artifactual 'collective' includes a witty actor that I have sometimes called coyote." "Coyote nature," the "world of non-machine non-humans," is "a collective, cosmopolitan artifact crafted in stories with heterogeneous actants."[204]

Typical of all the key representatives of the shift to "cultural studies of science," Haraway repudiates "representationalism." She formulates it explicitly as "a political semiotics of representation" in which the natural scientist serves as the "perfect representative of nature, that is, of the permanently and constitutively speechless objective world," such that, in this ventriloquistic and "depoliticizing

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[199][Rouse, 1996b, 202].

[200]"I take as a self-evident premise that 'science is culture.' Rooted in that premise, this essay is a contribution to the heterogeneous and very lively contemporary discourse of science studies *as* cultural studies." [Haraway, 1992, 296].

[201][Haraway, 1992, 298].

[202]Ibid., 299.

[203]Ibid., 298.

[204]Ibid., 332.

expert discourse" of "passionless distance," the "*only* actor left is the spokesperson" and "the represented is reduced to the permanent status of the recipient of action, never to be a co-actor..."[205] Against this domineering representation, Haraway counterposes the idea of "articulation." There, "boundaries take provisional, never-finished shape." This creates the "potential for generation," the "potential for the unexpected from unstripped human and non-human actants enrolled in articulations." She affirms: "the empty space, the undecidability, the wiliness of other actors, the 'negativity,' ...give me confidence in the *reality* and therefore ultimate *unrepresentability* of social nature..."[206]

Haraway makes clear that articulation is more than discourse: "Discourse is only one process of articulation. An articulated world has an undecidable number of modes and sites where connections can be made."[207] Thus, "language is the effect of articulation, and so are bodies."[208] "It will not do to approach science as cultural or social construction, as if culture and society were transcendent categories, any more than nature or the object is."[209] In artifactualism, "both members of the binary pairs collapse into each other as into a black hole."[210]

What Haraway seeks, she writes in "Modest Witness," is "questions of pattern, not of ontological difference ... shifting sedimentations of the one fundamental thing about the world — relationality."[211] Adverting to the essential engagement that characterizes feminism, she insists: "The point is to make a difference in the world ... To do that, one must be in the action, be finite and dirty, not transcendent and clean."[212] Thus, "situated knowledges" are engaged with, yet "friendly to science," without being naive about its "political semiotics."[213]

Like Haraway, N. Katherine Hayles affirms from the outset that "scientific inquiries are social and ideological constructions."[214] Like Haraway as well, Hayles is critical of representationalism, because it "encourages the fallacy that perception passively mirrors the external world." Hayles wants to "make representation a dynamic process rather than a static mirroring." She fears "representation may be too passive a concept to account for the complexities involved" in what she claims is "an active process of self-organization that depends on prior learning and specific contexts."[215] For Hayles, this is where the first point comes in: "so-called observables are permeated at every level by assumptions located specifically in how humans process information from their environments," i.e., they are "observations made by humans located at specific times and places and living in specific

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[205]Ibid., 312. In "Modest Witness," Haraway writes of the "epistemological and social power" of the scientist as "authorized ventriloquist for the objective world." [Haraway, 1996, 429].

[206][Haraway, 1992, 313].

[207]Ibid., 324.

[208]Ibid., 324.

[209]Ibid., 330.

[210]Ibid., 330.

[211]pHaraway, 1996, 439[.

[212]Ibid., 439.

[213][Haraway, 1992, 313].

[214][Hayles, 1993, 27].

[215]Ibid., 29.

cultures."[216]

Again like Haraway, Hayles is not content with a Derridean "prison house of language." That is, "possibilities for articulation exist that can elude the reflexive mirroring that would encapsulate us within textuality and nothing but textuality." Hayles postulates that science "retain[s] its distinctive characteristic as an inquiry into the nature of the physical world." But this nature and world are not the concepts of classical physics and philosophy but rather the artifacts of "complexities that unite subject and object in a dynamic, interactive, ongoing process of perception and social construction" which is "species-specific, culturally determined, and context-dependent." The central features of this conception, which Hayles calls "constrained constructivism," are contexts, consistency and constraints. The crucial notion is that of *constraints*: "constraints enable scientific inquiry to tell us something about reality and not only about ourselves."[217] It is not that constraints "tell us what reality is. This they cannot do. But they can tell us which representations are consistent with reality, and which are not."[218] Hayles sharply distinguishes this position from Popperian falsificationism by noting that Popper still clings to the idea of ultimate "congruence" between concept and reality in a "one-to-one correspondence." Hayles denies that very possibility: "congruence cannot be achieved because it implies perception without a perceiver."[219] That is why *consistency* is the best that can be hoped for: it is local, it is limited, but it can be "good enough." The concrete "interactions with the flux are always richer and more ambiguous than language can represent," and yet "a synergy between physical and semiotic constraints ... brings language in touch with the world. Physical constraints, by their consistency, allude to a reality beyond themselves that they cannot speak; semiotic constraints, by generating excess negativity, encode this allusion in language."[220] This excess negativity in semiotic constraints Hayles identifies with metaphoricity, which, "far from being a special subset of language usage, is fundamental to how language works."[221]

The gain in a theory of constrained constructivism, for Hayles, is that it permits the retrieval of a form of objectivity without retreating into "correspondence" with a "referent" in the old representationalist sense. To be sure, "every perspective is partial, all knowledges situated," and yet "the possibility of distinguishing a theory consistent with reality from one that is not can also be liberating." It is this that Hayles, following George Levine and Haraway, terms "the central issue of the contemporary sociology of knowledge" and "the central problem that a feminist sociology faces."[222]

While Hayles stresses the species determinacies of humans and thus the subject polarity in the situatedness, i.e., more the human and less the non-human,

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[216]Ibid., 28.
[217]Ibid., 32.
[218]Ibid., 33.
[219]Ibid., 35.
[220]Ibid., 38.
[221]Ibid., 40.
[222]Ibid., 41.

her exposition of physical constraints is fully compatible with the argument of Haraway for a material-semiotic non-duality. This is all the more emphatically the case with the theory of "agential realism" developed by Karen Barad. Barad asserts flatly: "physical and conceptual constraints are co-constitutive."[223] The "'material-discursive' practices [of science are] constrained by particular material-discursive factors and not arbitrarily construed... [T]here is a sense in which 'the world kicks back'."[224] Barad focuses precisely on "the question of the relationship between the material and the discursive," the "role of human *and* nonhuman factors in the production of knowledge," and hence "a more robust understanding of materiality."[225] Invoking Niels Bohr's philosophical-physical critique of Newtonian physics, Barad makes clear what *representationalism* has meant for conventional thought: "that observation-independent objects have well-defined intrinsic properties that are representable as abstract universal concepts."[226] Bohr insisted that the object could not be conceived apart from the "agencies of observation," and that these formed an irreducible "non-dualistic whole," a *phenomenon* or *event* behind which it was impossible to reach. Reality is the referent of physics, but "the referent is not an observation-independent object, but a phenomenon." Thus the objects of physics must always be encountered with the "physical and conceptual apparatuses" in and through which they occur. "Phenomena are the effects of power-knowledge systems, of boundary drawing projects. . . 'Subjects' and 'objects' do not preexist as such, but are constituted through and within particular practices."[227] Reality is "continually reconstituted through our material-discursive intra-actions."[228] That is, "what is being described by our theories is not nature itself, but our participation *within* nature."[229] Materialization is "an iteratively intra-active process whereby material-discursive bodies are sedimented out of the intra-action of multiple material-discursive apparatuses through which these phenomena (bodies) become intelligible."[230] "Subsequent iterations of particular situated practices constitute important shifts in the particular apparatus."[231] Because "intra-actions are constraining but not determining" there is a "space for material-discursive forms of agency," and "agency is about the possibilities and accountability entailed in refiguring material-discursive apparatuses of bodily production, including the boundary articulations and exclusions that are marked by those practices."[232] Even the non-human can be conceived as agential: this is the sense of the idea that "the world kicks back."

"The world in which we live . . . is sedimented out of particular practices that

[223][Barad, 1999, 4].

[224]Ibid., 2.

[225]Barad, 1998, 89.

[226]Ibid., 94.

[227]Ibid., 106.

[228]Ibid., 104.

[229]Ibid., 106.

[230]Ibid., 108.

[231]Ibid., 102.

[232]Ibid., 112.

we have a role in shaping," Barad argues, and this is not only an epistemological but an ontological point, in her view.[233] "Reality is sedimented out of the process of making the world intelligible through certain practices and not others. There-fore, we are not only responsible for the knowledge that we seek, but, in part, for what exists."[234] Such accountability, Barad argues, is constitutive of feminist science studies: a "normative commitment to a responsible and democratic future for science."[235] "We need knowledge systems that are both reliable and account-able guides to action," yet at the same time science must "incorporate a reflexive critical discourse," and "agential realism insists that mutually exclusive, shifting, multiple positionings are necessary if the complexity of our intra-actions are [sic] to be appreciated."[236] Thus, while "boundaries are necessary for making mean-ings," it is also the case that "boundaries are interested instances of power, specific constructions, with real material consequences."[237] Because there are real conse-quences, scientific practices entail "direct accountability and responsibility."[238]

Drawing on these theorists and others, Joseph Rouse discerns six "significant common themes" which constitute the specific new approach of these cultural stud-ies of science.[239] First, their key target is failure to acknowledge the *disunity* of science. Science, Rouse insists, is not a "natural kind."[240] It is no *one* thing, nor indeed a *thing* at all but a congeries of diverse practices which persist only via con-tinual reassertion and even in this reassertion mutate. "Cultural studies of science do not attempt to survey scientific practice as a whole and pronounce on its aim and legitimation. Cultural studies instead address particular discursive practices and the specific interactions with other practices and things through which those practices become significant."[241] Second, cultural studies abjure offering *explana-tions* of science, whether "internal" or "external." Instead, they concern them-selves with "the articulation and significance of meanings."[242] For Rouse, what distinguishes cultural studies is "the question of significance."[243] That is, "cul-tural studies focus on the emergence of meaning within human practices."[244] More extensively, "cultural studies emphasize a critical engagement with discursive prac-tices [in which] meaning is best understood as an open and dynamic engagement with the world."[245] Thus, cultural studies "explore the heterogeneous interconnec-tions among words, images, actions, and other events and things. . ."[246] Third,

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[233] Ibid., 102.

[234] Ibid., 105.

[235] [Barad, 1999, 3].

[236] [Barad, 1996, 186-187.

[237] Ibid., 182.

[238] Ibid., 183.

[239] [Rouse, 1996, 242].

[240] Here, Rouse is citing with approval [Rorty, 1988]. But see the reservations, which I share, expressed by [Hacking, 1996].

[241] [Rouse, 1996, 236].

[242] Ibid., 245.

[243] Ibid., 25.

[244] Ibid., 33.

[245] Ibid., 221.

[246] Ibid., 232.

cultural studies accentuate the localism and materiality of scientific practices as against "conceptions of the effortless and immaterial universality of scientific reasoning and knowledge."[247] Fourth, cultural studies explore the "constant traffic across boundaries" between scientific and other practices in the world.[248] As he puts it, "we make sense of what we do by enacting narratives in which what we do has an intelligible place."[249] Boundaries are always tentative and fluid. There is "traffic in all directions across whatever boundaries . . . too heavy for any significant autonomy of a domain of scientific practices" from others.[250] In particular, this emphasis on openness of boundaries highlights linkages of knowledge and power, as in the fraught relation between scientific knowledge and constructions of sex and gender. "Cultural studies often emphasize the importance of difference, power, ideology, and the proliferation of incommensurabilities, distortions, silencings, and other failures of understanding and communication."[251] Fifth, according to Rouse, cultural studies of science adopt "a subversive rather than an antagonistic stance" to scientific practices; they "challenge the formulation of the question rather than proposing an alternative to its traditional answers."[252] Over against the sociology of scientific knowledge the new project of cultural studies of science emphasizes that "indeterminacy, instability, opacity, and difference must play a more prominent role," and that a critical engagement with the practices of the sciences must replace the "normative anemia" shared ironically by both "value-free" science and "relativist" social constructionism.[253] Cultural studies engage science energetically but self-reflexively: "the question of political reflexivity is perhaps the most important and demanding challenge to be met."[254] For Rouse, the blurring of boundaries and the subversion of meanings imply that "cultural studies' interpretive readings are thus part of the culture of science and not an explanation or interpretation of it from 'outside'."[255] He denies that cultural studies of science endeavor what could only be "an ironic disciplinary imperialism" because cultural studies make no claim to "epistemic sovereignty," and instead always situate themselves concretely in the alignments of both power and knowledge as "reflexive attempts to strengthen, transform, or reconstitute existing alignments or counteralignments by resituating them historically and geographically."[256]

There is much that is persuasive about Rouse's synthesis, but it occasions some reservations, as well. Certainly, accentuating the disunity of science is an essential element in a new approach to science studies, but the question remains whether there is still some commonality that distinguishes in concrete contexts between a

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245. Ibid., 221.
[247] 247.
[248] Ibid., 250.
[249] [Rouse, 1999, 452].
[250] Ibid., 445.
[251] [Rouse, 1996, 233].
[252] Ibid., 254.
[253] Ibid., 25, 21.
[254] Ibid., 25.
[255] Ibid. 255.
[256] Ibid., 34, 258.

science and other practices. Indeed, the whole problem of boundaries, to which Rouse refers in his fourth point, obviously requires this. What is important, of course, is resisting reification of such boundaries, yet concrete accounts will entail *both* the articulation of specific differences among sciences and some frame of scientific practices and knowledge-claims which differentiates these from other practices against which these can be situated.

It is more difficult to assent to the dismissal of explanation in Rouse's new program. In stressing meaning and understanding as against explanation, my first impression is that Rouse is recurring to the longstanding quarrel between interpretive or hermeneutic approaches and those which pursue nomothetic universalism along more positivistic lines.[257] Dismissing *any* sort of explanation, including the kind of specific accounting which is a key part of contextual-historical practice, seems to me to extend a legitimate philosophical quarrel with one species of explanatory thinking to a general rule that does not sit well with a lot of concrete practice. To be sure, such concrete practices of interpretation and explanation aim at a localism which takes the materiality of situation very seriously — whether this be a laboratory apparatus or a channel of distribution for claims, i.e., whether physical or "textual." Yet many of those who seek to carry out a "naturalized" form of inquiry could subscribe to this, and thus Rouse's view seems less plausible as a disputation with "naturalized epistemology" than as one with a universalism that certainly characterized the "received view" of philosophy of science and the early, Strong Programme view of a sociology of scientific knowledge. There, to be sure, both science and society were handled as unproblematic universalities. Rouse's fourth point about the porosity of boundaries is surely well-taken, as is the concern with the "power/knowledge" configuration. This concern about and intervention against the unacknowledged power relations in signifying practices is at the core of cultural studies in the widest conspectus, and an element clearly central to the particular concern with science. Rouse believes that Foucault offers a good point of departure for conceptualizing this problem in the "new" sociology of knowledge.[258] As we shall see, the emergent field of "cultural sociology" has evolved in significant measure in dialogue with Foucault's theory.

The last two claims Rouse proffers occasion the most doubt. First, he argues that cultural studies of science take a "subversive" rather than "antagonistic" stance toward scientific practices. While it is surely fruitful to dispute the drastic posture of the "science wars" that *all* cultural studies are hostile to science, there is a substantial body of evidence that antagonism is not a paranoiac fantasy of natural scientists but characterizes attitudes expressed by *some* prominent exponents of cultural studies and postmodernism regarding science, disciplinarity and questions of warrant and evidence.[259] *Subversion* itself is an interesting notion, es-

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[257]For a classic juxtaposition, see [Wright, 1971]. For recent, far more sympathetic rendering of the interpretive approach to understanding, see [Hiley, Bohman and Shusterman, 1991].

[258][Rouse, 1987; 1994; 1993b; 1996c].

[259]In short, viewing the "science wars" as a historian, I find the matter more ambivalent than Rouse did and does as a participant. See [Zammito 2004] and soon, Rouse's review essay on that work, expressing his criticisms of it.

pecially if one asks how this coincides with the "participatory" construction Rouse places on the whole of cultural studies of science. How does subversion reconstruct the practice of sciences from the inside, and is that really what it has been about? I am not so confident as Rouse about all this.

Just this makes problematic Rouse's argument that cultural studies of science play a participatory rather than external role in the practices of science. I think that the ethnographers, from Latour to Traweek, have kept the stress on the *observer* in their practice of "participant-observer."[260] To be sure, a central concern of feminist studies of science is to enable and to recognize the participation of women in scientific practices, and some of the most penetrating criticism of the abuses of scientific power come from feminists situated in (or, more often, just within the margins of) a "science as usual" indifferent to its abusive practices. On the other hand, it is not so clear that political critique and discursive critiques are coterminous, and that recourse to poststructuralist theories of discourse is a necessary or a sufficient means to bring about political or epistemic criticism of ongoing scientific practices. Rouse has offered too benign an account of the post-structuralist obsession with language and textuality, and too bland a view of the "theoretical" impact of that intervention. He dismisses too glibly the hegemonic impulse in cultural studies. Seen from a wider disciplinary-historical vantage, cultural studies has shown a quite hegemonic disposition — especially in its American academic deployment within literary studies and the human sciences. While that imperialism has not been so pervasive in science studies, that may say more about the resistance of natural scientists than about the motives or impetus of cultural studies as a movement.

Altogether, I find Rouse's affirmations of cultural studies to be a powerful but an interested account, one that goes too lightly over its internal contestations and its external aggressions. While he discerns important impulses for the future of any effective "new" sociology of knowledge — a term with which he would very likely remain unhappy — these are not, in my view, unique to what he calls "cultural studies of science," nor is the particular meta-philosophy he associates with that movement entirely convincing. First, concrete achievements in interpretation, as situated as these must be, nevertheless entail problems of adjudication and evidence that require articulation. I do not believe that in dismissing "epistemic sovereignty" one forecloses "epistemology" altogether, though, of course, it will require respecification. Second, the question of cumulation and stabilization as *positive* features of scientific practices gets too short a shrift in Rouse's account. The historical-contextual fluidity of practices and of the narratives that embody and interpret them sometimes becomes so pervasive in Rouse's reconstructions that one loses track of the situatedness in a concrete sense without which it is impossible to undertake scientific practices or to understand them. Ultimately,

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[260][Latour and Woolgar, 1986; Latour, 1987; Traweek, 1988]. Latour and Traweek have, of course, sharply dissonant views of the ethnographic endeavor. See their exchange in Pickering, ed., 1992 and the very sharp rebuke of Latour by Haraway for his criticisms of Traweek, in [Haraway, 1992].

the ambivalence that Rouse's synthesis arouses in me has to do with concrete practice of interpretation: offering a (contingent and fallible) account of a particular instance to and within a particular community of interested readers. Looking from *within* a situation, I believe that context, consistency, and above all the constraints of cumulation and stabilization need to be taken more richly into view, alongside both the radical novelty of practical inquiry and the problematics of power/knowledge that Rouse rightly stresses. Not only do I hold out for more concrete determinacies in the material dimension of the practice non-duality, I hold out similarly for more determinate linguistic-discursive articulations — always, to be sure, in a concrete, situated context. In considering Haraway, Hayles and Barad, I think this theme of determinacy and constraint is very prominent.

Evelyn Fox Keller, reflecting on the intersection of science studies with feminist studies, draws an essential point from these new impulses, distinguishing them from the linguistic obsession of postmodernism: "where advocates of difference within science depart from and effectively counter the tendency in postmodernism towards an indefinite proliferation of difference is in their reminder of the constraints imposed by the recalcitrance of nature."[261] The idea of constraint is one that is shared widely in science studies, as is clearest in the exchange between Peter Galison and Andrew Pickering.[262] What constraint and the emphasis on the non-dualistic reconstruction of concrete practices accomplish is an escape from the impasses not only of the realism/anti-realism controversies in philosophy of science, or the realism/relativism controversies between sociology of knowledge and philosophy of science, but of even more pervasive "sticking points" in the history of Western thought — between language and world, culture and nature, subject and object.[263] What is sought is not an indiscriminate monism, but a highly contingent, situated practice which entails at once materialization and intelligibility, yet without any guarantee of persistence and without claim to anything more than a pragmatic fitness. In this, I believe myself fundamentally in sympathy with Rouse and the feminist theorists he celebrates (Haraway and Barad) as well as others (Hayles, Keller and especially Helen Longino) who play a more peripheral role in his account. Where we agree emphatically in that feminism has played a crucial role in reformulating the problematic of a sociology of knowledge.

2.3   Feminism and the Sociology of Knowledge

Feminism's contribution to postmodern contestation and epistemological criticism has been crucial. One might go so far as to say that without feminism, cultural studies would have been a far less effective initiative. More specifically, feminism, in my view, has played a very important role in the development of the "new sociology of knowledge." Yet it has done so under a peculiar double tension. On the one hand, feminism has pressed the demand to be taken seriously *as* disciplinary

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[261][Keller, 1999, 241].
[262]See my discussion of this exchange in [Zammito, 2004.
[263][Hacking, 1999].

sociology. On the other, it has felt more comfortable in an interdisciplinary disposition. Mainstream sociology has considered feminist theory too partisan and particular to qualify for its universalistic domain of theory.[264] Conversely, feminist theory spearheaded the *anti*-disciplinary onslaught of "cultural studies." The very idea of disciplinary structure, for feminism, smacks of the "abstract masculinity" it is contesting. Over the recent period, feminism has advanced from the clarification of a theory of "gender" in juxtaposition to "sex" (the repudiation of physiological reductionism) to a clarification of "sex" (and other elements in biology) as a gendered projection upon the natural order. With that, wider questions of gender bias in the epistemological and methodological presuppositions of natural science have assumed centrality.[265] Feminists now theorize the influence of gender on the construction of knowledge and reason generally, creating a feminist epistemology and sociology of knowledge.[266]

The relation of women to knowledge is fundamentally problematic in at least Western thought. "The concepts of women and of knowledge — socially legitimated knowledge — [were] constituted in opposition to each other in modern Western societies," Sandra Harding proclaimed in *Whose Science? Whose Knowledge?*[267] That is, "the particular methods and norms of the special sciences are themselves sexist and androcentric . . . constructed primarily to produce answers to the kinds of questions an androcentric society has about nature and social life."[268] A way of conceptualizing *order* (gendered symbolism) compelled and preyed upon a *division of labor* ("women's work" vs. prediction and control) and authorized male psychological *identity* ("abstract masculinity" — the "man of reason").[269] This not only excluded women from the position of the knower; it defined them as/with the object of inquiry/control, "nature."[270] This has been the point of departure for the most recognized feminist sociologist of knowledge,

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[264] As Charles Lemert asks regarding Dorothy Smith, "can her sociology of women's experience lead to a sociology of the subjective experience of others and thus to a sociology as such?" [Lemert, 1992, 68]

[265] Mario Biagioli offers a similar trajectory for feminist studies of science in [Biagioli, 1999].

[266] Keller, 2001, 98. Keller was one of the pioneers in this domain of "gender and science." Her essay, "Gender and Science" [Keller, 1983] was one of the first contributions to the field, and her book [Keller, 1985] won acclaim for its articulation of a feminist vantage on doing science. With Helen Longino, Keller edited one of the most effective anthologies of feminist theory of science [Longino and Keller, 1992]. Later still she chronicled the field in [Keller, 1995].

[267] [Harding, 1991, 106].

[268] Ibid., 117. Harding credited Evelyn Fox Keller for the core argument "that it is in the association of competence with mastery and power, of mastery and power with masculinity, and of this constellation with science that the intellectual structures, ethics, and politics of science take on their distinctive androcentrism." [Harding, 1986, 121], referring to Keller, 1983.

[269] Decisive for the interpretation of the first issue was Merchant, 1990. On the question of gendered divison of labor, important work was done by Rose, [1987] and Hartsock, [1983]. On the question of psychological identity, the most important source was "object relation theory" in psychoanalysis [Chodorow, 1978; Dinnerstein, 1976] — and the application of this to the problem of masculinity in science by [Keller, 1983]. The "man of reason" is, of course, Genevieve Lloyd's construction: Lloyd, 1984.

[270] [Ortner, 1974]; see also [MacCormack and Strathern, 1980].

Dorothy Smith.[271] Her decisive intervention, "Women's Experience as a Radical Critique of Sociology," first presented in 1974, makes clear from its title onward the fundamentally adversarial relation she posits between what feminism unearths and what sociology imposes.[272] As Smith puts it, there is "a disjunction between how women experience the world and the concepts and theoretical schemas by which society's self-consciousness is inscribed."[273] That is, "as women we have been living in an intellectual, cultural, and political world, from whose making we had been almost entirely excluded."[274]

The question that feminists face is whether a radical critique of *all* knowledge claims as power plays can leave any possibility for a *feminist* claim to knowledge? Can situatedness entail *knowledge*, and how can this be adjudicated *across* situations? These are, of course, the essential challenges within the tradition of discourse known as "sociology of knowledge." In a very important review/intervention in 1981, Donna Haraway discerned a dilemma in the feminist stance. Feminist critique all too swiftly "glides into a radical doctrine that all scientific statements are historical fictions made facts through the exercise of power." But "showing the fictive character of all science, and then proposing the real facts results in repeated unexamined contradictions."[275] Feminist epistemology had to advance from a critical to a constructive program: "Feminists want some theory of representation to avoid the problem of epistemological anarchism. An epistemology that justifies not taking a stand on the nature of things is of little use to women trying to build a shared politics."[276] Much later, in a very important reflection on this longstanding problem, Susan Hekman reasserted the same key point: "feminist politics demand a justification for the truth claims of feminist theory."[277] Dorothy Smith raised this concern as well, ironically against postmodernist feminist critics of her work who appeared to dispute the possibility of asserting actual knowledge.[278] Similarly, Joan Alway observes: "feminism cannot afford to renounce efforts to describe, explain, and understand the regularities, the underlying tendencies, and the generalized meanings of the social world," and accordingly it must "be informed by a conviction that knowledge of the social world is possible and that this knowledge can serve as a basis for at least improving social conditions."[279]

Sandra Harding proposed "strong objectivity" as her personal answer to Har-

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[271] Smith not only categorizes her work as sociology of knowledge, but she has the distinction of being the only feminist theorist to attract attention in the wider sphere of sociological theory, as Joan Alway notes, via such contexts as a symposium in *Sociological Theory* and a session at the American Sociological Association annual conference — both in 1992, a "recognition of Smith's contributions to social theory ... long overdue," in Alway's judgment. [Alway, 1999, 212].

[272] [D. Smith, 1974].

[273] [D. Smith, 1974, 13].

[274] [D. Smith, 1987, 1].

[275] [Haraway, 1986, 78].

[276] Ibid., 79.

[277] [Hekman, 1997, 342].

[278] [D. Smith, 1990, 79].

[279] [Alway, 1999, 224].

away's challenge.[280]

> A strong notion of objectivity requires a commitment to acknowledge
> the historical character of every belief or set of beliefs — a commitment
> to cultural, sociological, historical relativism. But it also requires that
> judgmental or epistemological relativism be rejected.[281]

While recognizing the inevitability of historical and cultural contingency, there are, for Harding, "rational or scientific grounds for making judgments between various patterns of belief."[282] She rejects the disjunctive formulation of judgmental relativism that "if one gives up the goal of telling one true story about reality, one must also give up trying to tell less false stories."[283] Harding believes one can "apply rational standards to sorting less from more partial and distorted belief."[284]

From within the frame of sociology of knowledge, feminist "standpoint" theory offers some very pertinent insights. The key innovators of this approach were Nancy Hartsock and Dorothy Smith. Hartsock set forth the argument with admirable lucidity in her essay, "The Feminist Standpoint," first published in the early 1970s. Hartsock drew an explicit analogy to Marxist historical materialism and its two claims that differential positions in society result in different perceptions of social relations, and, more controversially, that "correct vision ... is available from only one."[285] The essential features of standpoint theory, accordingly, are that "in systems of domination the vision available to the rulers will be both partial and perverse," that it will nevertheless structure everyone's lives and must therefore be understood and deconstructed against the grain of its domination, and hence, finally, that "the vision available to the oppressed group must be struggled for and represents an achievement" that is emancipatory for society generally.[286] Central to this argument is the notion that "the ability to go beneath the surface of appearances to reveal the real but concealed social relations requires both theoretical and political activity."[287] Hartsock argues, for this reason, that she is characterizing a *feminist* standpoint, not a female one, because it is both achieved, not given, and political, actively emancipatory in its intent and practice, not disinterested.[288] In stressing the "sexual" as opposed to the "gender" division of labor and stressing "object relations" psychology, Hartsock affirms a strong continuity between the embodied and the constructed elements of women's experience. She urges that women do have a different social experience from men which can have epistemic consequences. Thus they can achieve "a particular and

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[280][Harding, 1991, 149]. Developing her idea of "strong objectivity" in response to Haraway's challenge represents the major intellectual move from Harding's first book to her second.

[281]Ibid., 156.

[282]Ibid., 152.

[283]Ibid., 187.

[284]Ibid., 159.

[285][Hartsock, 1983, 284].

[286]Ibid., 285.

[287]Ibid., 304.

[288]Ibid., 289.

privileged vantage point" from which to expose the "fragility and fundamental falseness of the masculinist ideology."[289]

Dorothy Smith emphatically insists upon starting from the actuality of women's experience.[290] Her concern is simply "how a sociology might look if it began from women's standpoint."[291] She suggests that when (some) women discover that "the sociological subject as an actual person in an actual concrete setting has been canceled in the procedures that objectify and separate her from her knowledge," this creates a "line of fault," a "bifurcated consciousness." Feminism seizes this opportunity for reflexive critique: "an opening in a discursive fabric through which a range of experience hitherto denied, repressed, subordinated, and absent to and lacking language, can break out."[292] Women, "lacking means of expression, lacking symbolic forms, images, concepts, conceptual frameworks, methods of analysis, . . . lacking self-information and self-knowledge," have had to invent their own linguistic forms to express their insight. "In their work at the point of rupture between experience and the ideological mode of integrating and reading it, women have had to resort to their experience unmade, because there has been no alternative."[293]

Smith advocates "making our direct embodied experience of the everyday world the primary ground of our knowledge."[294] But she denies that this is a subjectivist approach. "The actualities of our everyday world are already socially organized ... prior to the moment at which we enter and at which inquiry begins."[295] Thus, "the everyday world is not fully understandable within its own scope. It is organized by social relations not fully apparent in it nor contained in it."[296] If Smith argues that such a feminist sociology should "begin from where we are located bodily," that is only a point of departure from which to seek out "the relations underlying and generating the characteristics of our own directly experience world."[297] She insists "there is no other way to know than humanly, from our historical and cultural situation." Situatedness does not discredit knowledge-claims: "to disclose the interests and perspectives of sociological knowers does not as such invalidate a knowledge that is grounded in actualities."[298] Engagement does not annul judgment: "to begin from the standpoint of women is to insist on the validity of an inquiry that *is* interested and that begins from a particular site in the world."[299] More, "a sociology for women preserves the presence of subjects as knowers and actors" against an "established sociology [which] is preoccupied with suppressing

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[289]Ibid., 284, 297.
[290][D. Smith, 1992].
[291][D. Smith, 1974, 12].
[292]Ibid., 20, 12.
[293][D. Smith, 1987, 58-59].
[294][D. Smith, 1974, 22].
[295]Ibid., 23.
[296]D. Smith, 1987, 92.
[297]D. Smith, 1974, 23, 27.
[298][D. Smith, 1990, 32].
[299]Ibid., 33.

the presence of the sociological subject."[300] Thus, "the standpoint of women . . . directs us to an 'embodied' subject located in a particular actual local historical setting . . . . Though discourse, bureaucracy, and the exchange of money for commodities create forms of social relations that transcend the local and the particular, they are constituted, created, and practiced always *within* the local and particular."[301] For Smith, sociological theory is suspect in its systematic abstraction, which she attributes to shifts in the "relations of ruling" that have established themselves in modernity: "the objectification of knowledge is a general feature of contemporary relations of ruling."[302] For Smith, this objectification is associated with an abstract form of knowledge she terms, generally, *textually mediated discourse*: "our knowledge of contemporary society is to a large extent mediated to us by texts of various kinds."[303] Their "virtual reality" supercedes the actuality of lived experience: "knowledge as a specialized form of social organization appears to be independent of the presence and activities of subjects," and "the account comes to stand in for the actuality it claims to represent."[304]

Thus Smith appears to insist upon the actuality of the particular subject against any conceptual or abstract notion of structure or culture: "Constituting these phenomena as *culture* or *meaning* converts the practices of actual people, integral to everyday courses of action, into a timeless, dislocated space of mere language, mere thought."[305] She insists, "there is an actual subject prior to the subject constituted in the text."[306] In adopting this stance, Smith simultaneously rejects orthodox sociology and also postmodernist strands in feminism, in particular those which embrace Foucault. "[Orthodox] sociology created and creates a construct of society that is specifically discontinuous with the world known, lived, experienced, and acted in."[307] Foucault's notion of discursive regimes she finds similarly alienating: "power and knowledge are not linked in some mystical conjunction such as that enunciated by Michel Foucault."[308]

Sandra Harding's widely cited *Science Question in Feminism* offered a summary account of "standpoint" theory: "Knowledge emerges for the oppressed through the struggles they wage against their oppressors."[309] The argument derives from Hegel's master-slave dialectic and its Marxist, especially Neo-Marxist elaboration.[310] In general, "more complete and less distorting categories [become] available from the standpoint of historically locatable subjugated experiences."[311] "In a socially stratified society the objectivity of the results of research is increased

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[300][D. Smith, 1987, 105, 117].
[301]Ibid., 108.
[302][D. Smith, 1990, 67].
[303]Ibid., 61.
[304]Ibid., 66, 74.
[305]Ibid., 202.
[306][D. Smith, 1990a, 5].
[307]Ibid., 2.
[308][D. Smith, 1990, 79].
[309][Harding, 1986, 126].
[310]Ibid., 59. See [Hegel, 1977, 111-119; Lukacs, 1971].
[311][Harding, 1991, 158].

by political activism by and on behalf of oppressed, exploited, and dominated groups."[312] That is, "only through political struggle could women get the chance to observe the depth and extent of male privilege."[313] The struggle of women in their situation of subjugation opened up critical (objective and incremental) knowledge which could be transformative for science (and society). "Women's subjugated position provides the possibility of more complete and less perverse understandings . . . a morally and scientifically preferable grounding for our interpretations and explanations of nature and social life."[314]

Standpoint theory lost a good deal of its elan in subsequent feminist discourse.[315] In the 1980s, two highly divisive controversies broke out within feminism.[316] The result was what Linda Alcoff called a crisis for feminist theory. First, the category *woman* shattered into that of many different *women*.[317] This fracturing occurred primarily via sustained critique of the ethnocentrism of white Western feminism by "women of color."[318] This essentially *political* critique, while it fragmented feminism into a set of potentially conflicting ("hyphenated") feminisms, remained committed to a fundamentally liberatory agenda in which claims to knowledge, grounded in *experience* (*inter alia,* of subjugation), remained possible and necessary.[319]

The second, *epistemological* controversy, launched from within Western feminism by assimilation of high European post-structuralist theory, challenged the very idea that there was any nature, however diffracted, to be found in the category *women.* Postmodernist feminists disputed the "women's way of knowing" advocated by "standpoint" theory as a dangerous and insupportable "essentialism."[320] They invoked a radical relativism, insisting that the proper role of feminism should be strictly *critique*: disruption of authoritative discourses.[321] Postmodern feminism thus raised Haraway's dilemma to a dogma.[322] In the words of Jane Flax: "We cannot simultaneously claim (1) that the mind, the self, and knowledge are socially constituted and that what we can know depends upon our social practices

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[312]Ibid., 127.

[313]Ibid., 71.

[314][Harding, 1986, 26].

[315][Hekman, 1997, 341].

[316][Alcoff, 1988].

[317]Haraway discusses this in Haraway, [1991a, 155-161]. Keller notes: "as we have learned, it makes dubious sense to ask about women's experience without first asking, and quickly, Which women?" [Keller, 2001, 100].

[318][Moraga and Anzaldúa, 1981; Hull, Scott and Smith, 1982; Hooks, 1984; Mohanty, 1988].

[319]For a recent and complex assessment of these issues, see [Moya and Hames-Garcia, 2000].

[320]"The essentialism — anti-essentialism debates define '80s feminism..." [Schor, 1994, vii]. See also [Fuss, 1989].

[321]Harding ambivalently offers this characterization of postmodern feminism in each of her two books. The leading feminist postmodernists, according to Harding, were Jane Flax and Donna Haraway. Harding, [1986] upholds the standpoint position against postmodern feminism. Harding, [1991] seeks to find an accommodation between standpoint theory and postmodernism.

[322]The decisive confrontation of feminism with post-structuralist theory is to be found in the essays collected in [Nicholson, 1990]. For unequivocal advocacy of postmodern feminism, see [Hekman, 1990].

and contexts and (2) that feminist theory can uncover the truth of the whole once and for all."[323]

Yet feminist standpoint theory has seen an even more recent revival of consideration.[324] Susan Hekman, a foremost postmodern feminist, has revisited thoughtfully the feminist standpoint arguments. She finds no problem with the notion of situatedness of knowledge, but she cannot accept the idea that any perspective is privileged, not even the feminist one.[325] That is the essential move of postmodernist feminism, and she argues that it was implicit in the very idea of situated knowledges all along. Feminist discourse, as Foucault would argue, is but one discourse among others.[326] Standpoints are always already "discursively constituted," including the feminist standpoint, Hekman argues, though "the material reality of women's lives on which it is based is not."[327] Hekman argues that Dorothy Smith "refuses to acknowledge . . . that [women's] reality is also discursively constituted."[328] Hekman finds that Smith "posits an absolute dichotomy between abstract concepts on the one hand and lived reality on the other," and this is deeply problematic. Moreover, "Smith does not offer any argument for why the located knowledge of women is superior to the abstract knowledge of the sociologist; this is assumed to be obvious."[329] But, if all standpoints are equally partial and partisan, how is one to adjudicate among them, how is feminism to make actual claims that warrant attention? For Hekman, Donna Haraway articulates "the central problem facing feminist theory today: given multiple standpoints, the social construction of 'reality,' the necessity of an engaged political position, how do we talk about 'better accounts of the world,' 'less false stories'?"[330]

Haraway's essay, "Situated Knowledges: The Science Question in Feminism and the Privilege of the Partial Perspective" [1988], composed as a commentary on Harding's *The Science Question in Feminism*, offered a meditation on the "inescapable term 'objectivity.'"[331] Haraway described her own intellectual development in terms of the absorption of two powerful theoretical-critical perspectives: social constructivism in science studies and post-structuralism in the human sciences. The first taught that "all knowledge is a condensed node in an agonistic power field," that "all drawings of inside-outside boundaries in knowledge are theorized as power moves, not moves towards truth." The second presented "always already absent referents, deferred signifieds, split subjects, and the endless play of signifiers," engendering a "hyper-real space of simulations." In sum, "the strong

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[323][Flax, 1990, 48]. Flax skews the case with the absolutist notion of "the truth of the whole once and for all," for the issue is really about concrete knowledge claims in concrete local-historical contexts.

[324][Wylie, 2003; Winant, 1987].

[325][Hekman, 1997, 349].

[326]Ibid., 345.

[327]Ibid., 346.

[328]Ibid., 348.

[329]Ibid., 347, 352.

[330]Ibid., 358.

[331][Haraway, 1991b, 183-201]. Haraway explains the context of the essay in a footnote, ibid., 248 n 1.

programme in the sociology of scientific knowledge joins with the lovely and nasty tools of semiology and deconstruction to insist on the rhetorical nature of truth, including scientific truth." But, she now acknowledged, "the further I get with the description of the radical social constructivist programme and a particular version of postmodernism, coupled to the acid tools of critical discourse in the human sciences, the more nervous I get." Pragmatically, politically — and therefore cognitively — "we would like to think our appeals to real worlds are more than a desperate lurch away from cynicism." Accordingly, Haraway proposed a new sobriety: "We cannot afford these particular plays on words — the projects of crafting reliable knowledge cannot be given over to the genre of paranoid or cynical science fiction." Taking up the dilemma she had earlier enunciated herself, she wrote: "no matter how much space we generously give to all the rich and always historically specific mediations through which we and everyone else must know the world," it now appeared that "feminists have to insist on a better account of the world; it is not enough to show radical historical contingency and modes of construction for everything." She called for "some enforceable, reliable accounts of things not reducible to power moves and agonistic, high status games of rhetoric or to scientistic, positivist arrogance." More concretely, the "problem is how to have *simultaneously* an account of radical historical contingency for all knowledge claims and knowing subjects, a critical practice for recognizing our own 'semiotic technologies' for making meaning, *and* a no-nonsense commitment to faithful accounts of a 'real' world."[332]

Haraway proposed a solution: "feminist objectivity means quite simply *situated knowledges*."[333] Renouncing the all-or-nothing "god-trick" of a view from nowhere, Haraway at the same time rejected relativism. For Haraway, "relativism is the perfect mirror twin of totalization in ideologies of objectivity." "The alternative to relativism is partial, locatable, critical knowledges, sustaining the possibility of webs of connections called solidarity in politics and shared conversations in epistemology."[334] "Only partial perspective promises objective vision . . . Partial perspective can be held accountable for both its promising and its destructive monsters."[335] "The knowing self is partial in all its guises . . . it is always constructed and situated together imperfectly, and *therefore* able to join with another, to see together without claiming to be another."[336] For Haraway, "production of universal, totalizing theory is a major mistake."[337] Instead, one should opt for "partial realities that value serious difference."

In the context of these epistemological debates, Ruth Bloch urged that feminist theory turn towards a substantive element that had somehow fallen out of sight. Concerned that much of feminist theory had developed reductivist impulses, whether to textual codes or to material force, she suggested that feminists

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[332]Ibid., 184-187.
[333]Ibid., 188.
[334]Ibid., 191.
[335]Ibid., 190.
[336]Ibid., 193.
[337]Ibid., 181.

consider the autonomy of the cultural.[338] "Drawing from Marxism and poststructuralism alike, feminists tend to conceive of culture as the reflection of dominant interests."[339] "Lost from such an understanding is the way that gender becomes socially meaningful and articulates with other common structures of meaning."[340] Retrieving a non-reductive notion of culture would allow feminist theory a way to recuperate the valid parts of the "standpoint" theorizing of an earlier phase of thought, like object relation psychology or the idea of women's experience: "The more voluntaristic insights of 'women's culture' need to be incorporated into the more recent discursive analysis of gender."[341] Bloch's argument met only lukewarm response from her feminist commentators, but her argument for the autonomy of culture opens out on the newest and most exciting sociology of knowledge.[342]

2.4   Cultural Sociology

In their explicit characterization of the "new" sociology of knowledge, Swidler and Arditi note "scholars in history, philosophy, anthropology, and the history of science" have played as important a role as sociologists.[343] Craig Calhoun observes that "recent work in literary criticism, philosophy and history which ought to be seen as central to social studies of culture" is only very lately coming within the ken of sociological theorists, but "the reformulation of sociological theory depends in large part on a rethinking of the place of culture within it," that is to say, on "how sociologists relate to other disciplines and to interdisciplinary discourses."[344] *Cultural sociology* is that response. Surveying the development of recent cultural sociology, Michèle Lamont and Robert Wuthnow observe that "a growing number of American cultural sociologists are increasingly reading outside their discipline, and are becoming more influenced by the interdisciplinary current in which European cultural theorists play a central role."[345] More specifically, "as American cultural sociology has begun to flourish again in recent years, it has been increasingly influenced by the work of scholars such as Foucault, Habermas, Douglas, and Bourdieu."[346]

     While there has always been "sociology of culture," Jeffrey Alexander claims,

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[338][Bloch, 1993].

[339]Ibid., 83.

[340]Ibid., 80.

[341]Ibid., 95.

[342]See [Laslett, 1993; Harding, 1993]. Bloch astutely replied that these commentators missed her point. (See [Bloch, 1993a].) At the core was their confusion over what Bloch meant by the autonomy of culture: they took it to mean the empirical actuality of a given web of beliefs and practices (the ethnographic notion of cultures). Bloch meant it as *one* structural factor in the constitution of social reality, but one with its own character and impact. In this, as we shall see, she is in step with the new "cultural sociology."

[343][Swidler and Arditi, 1994, 306].

[344][Calhoun, 1989, 1].

[345][Lamont and Wuthnow, 1990, 306].

[346]Ibid., 301.

what is new is the emergence of a "cultural sociology."[347] Alexander goes so far as to pirate the slogan of a "strong program" from science studies to characterize the radical novelty of the agenda of this "new" sociology of knowledge/culture.[348] For Alexander, "commitment to ... cultural autonomy is the single most important quality of a strong program."[349] The second principle is commitments to interpretive or "hermeneutic" reconstruction, "a Geertzian 'thick description' of the codes, narratives, and symbols that create the textured webs of social meaning."[350] The third defining principle is the effort "to anchor causality in proximate actors and agencies, specifying in detail just how culture interferes with and directs what really happens."[351] Alexander is not entirely comfortable with Geertz's total aversion to generalization: "As the text replaces the tribe as the object of analysis, cultural theory begins to look more and more like critical narcissism and less and less like the explanatory discipline" that Alexander still believes sociology should be.[352] Thus Alexander welcomes the structuralist move in anthropological and interpretive thought: "If one takes a structuralist approach to narrative . . ., textual forms are seen as interwoven repertoires of characters, plot lines, and moral evaluations whose relationships can be specified in terms of formal models ... thus operat[ing] as a bridge between the kind of hermeneutic inquiry advocated by Geertz and the impulse toward general cultural theory."[353] Alexander urges that we believe "structuralism and hermeneutics can be made into fine bedfellows."[354] That would make cultural sociology quite a theoretical achievement!

Even as Alexander and a battery of sociologists have taken up the concept of culture in a decisive manner, anthropologists, the traditional purveyors of the idea, ironically seem to have grown deeply disenchanted with it.[355] Thus we must assess what repelled the anthropologists and what attracted the sociologists, and how all this bears upon the "new" sociology of knowledge. Robert Brightman identified no fewer than fifteen different aspersions against the "culture construct" within current cultural anthropology, observing wryly thereupon: "When we encounter arguments today that *the* culture construct should be abandoned, we must naturally wonder which of its formulations from among all the possible ones we should be rid of."[356] The most common objection in the anthropological literature has been to the notion of culture as *coherent*, as organic unity.[357] "Contestation, entropy, and chaos have long since displaced coherence and integration as the

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[347][Alexander, 1996].
[348]Ibid.
[349]Alexander elaborates this notion in [Alexander, 2003, 13].
[350]Ibid.
[351]Ibid., 14.
[352]Ibid., 22.
[353]Ibid., 25.
[354]Ibid., 26.
[355][Sewell, Jr., 1999].
[356][Brightman, 1995, 527]. See also [Abu-Lughod, 1991]. For dismay over this apostasy, see [Yengoyan, 1986].
[357]The blame for this heritage is ascribed heavily to Herder and German romanticism and historicism.

privileged disciplinary themes."[358] That is, "the claim is not only that cultures are internally diverse (vs. homogeneous) but that they are disordered, contradictory, and sometimes disputed."[359] Discovering the contradictory or conflictual elements within cultures accentuated what Sherry Ortner, in an influential theoretical overview of disciplinary trends in 1984, dubbed the "practice orientation," i.e., the shift from the "abstraction" of culture as a whole to the "strategies" and "interests" in individuated practice or "agency."[360]

The objection to the *coherence* of the culture construct is complemented by a rejection of its *closure.* "The concept of a fixed, unitary, and bounded culture must give way to a sense of the fluidity and permeability of cultural sets," Eric Wolf proclaimed.[361] Cultures should not be viewed as "natural kinds," critics agreed, since they were neither primordial, nor local, nor discrete.[362] Instead, "criteria of delimitation are multiple, redundant, incongruent, and overlapping."[363] Indeed, Arjun Appadurai resorted to the notion of *fractals* to claim that there were "no Euclidean boundaries, structures, or regularities" to culture.[364] All these objections to the culture construct have an element in common: they reflect a dramatically enhanced awareness of the role of culture in relations of power, and consequently the need to bring these features — intracultural or intercultural — into the spotlight. This has clearly been the most important feature of recent theoretical developments in culture theory.

If anthropologists have been desperately seeking to get out from under the culture construct, the situation in sociology was just the opposite. Culture had seen "the weakest analytical development of any key concept in sociology," according to Margaret Archer.[365] Craig Calhoun agrees that "the study of culture has been strikingly marginalized in sociology, especially in the United States."[366] Jeffrey Alexander offers a telling anecdote about chatting with sociology colleagues at UCLA in the 1970s who found the very idea of "cultural sociology" laughable.[367] That it nonetheless became "a prominent subfield" by 1988 and "a major growth industry in the sociological portfolio" for the 1990s therefore requires some explication.[368] A variety of impulses led to this surge; it was part of what William Sewell, Jr., has dubbed "a kind of academic culture mania" in the 1980s and 1990s.[369] One of its domestic American origins lay in the "production-of-culture" approach led by Diana Crane and Richard Peterson, i.e., attention to the concrete

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[358][Brightman, 1995, 517].
[359]Ibid.
[360][Ortner, 1984].
[361][Wolf, 1982, 387]. See also [Clifford, 1992; Gupta and Ferguson, 1992].
[362]These are three of the fifteen faults that Brightman found among the criticisms of the culture construct.
[363][Brightman, 1995, 519].
[364][Appadurai, 1990, 20], cited in [Brightman, 1995, 521].
[365][Archer, 1985, 333].
[366][Calhoun, 1989, 1].
[367][Alexander, 2003, 4-5].
[368][Wuthnow and Witten, 1988, 49].
[369][Sewell, Jr., 1999, 36].

institutional context of actual cultural objects.[370] But decisive for the "culture mania" of the 1980s and 1990s, of course, was the impact of structuralism and poststructuralism, and with these, the rise of "cultural studies." It saw its task as overcoming "a general failure in work on cultural production to analyse historically the sign-systems, codes and styles which are available for authorial and audience groups to make meanings with."[371] Within the discipline of sociology, Sewell observes, "by the late 1980s, the work of cultural sociologists had broken out of the study of culture-producing institutions and moved toward studying the place of meaning in social life more generally."[372]

The new cultural sociologists were no more disposed to accept the holistic notion of culture than their anthropological colleagues, for all the variance of their disciplinary trajectories. Indeed, the dawning of theoretical interest in culture within sociology came with the abandonment of what Margaret Archer called the "myth of cultural integration."[373] Ann Swidler similarly characterized and dismissed "the older definition of culture as the entire way of life of a people." Instead, she opted for the "image of culture as a 'tool kit' of symbols, stories, rituals, and world-views, which people may use in varying configurations."[374] As with the anthropologists, the concern that drove the new field was to grasp concrete practices, to reject at one and the same time "structural" determinism and cultural determinism, to create a more sophisticated theoretical space for agency. But the endeavor was also to grasp the specific contribution — enabling and constraining — that cultural factors made to practices. That entailed a more differentiated notion of culture: breaking it out into more determinate structures of its own, and then finding how, concretely, these affected practices. The core issue was cultural "causality," and, as its logical prerequisite, cultural "autonomy."[375] Culture, the new cultural sociology sought to establish, was at once an autonomous factor (not an outcome or epiphenomenon of social structure) and a component (not a self-sufficient determinant) of the concrete practices of agents, who possessed, accordingly, their own element of autonomy. Sociology, such thinking suggests, needs to move more toward "cultural studies," ethnomethodology, hermeneutics. "Thick description" and "local knowledge(s)" seem more achievable and credible than a "nomothetic" universalism.[376] That is, the balance between "interpretive" and "positivist" social science has shifted substantially in favor of the former.[377] Hence all the talk of "turns" — rhetorical, interpretive, and, of course, cultural.

For Swidler and Arditi, the "new" sociology of knowledge as "cultural studies" entails a shift from "formal systems of ideas" to the everyday. "In cultural studies,

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[370]See the programmatic statement of [Peterson, 1979]. And see [Crane, 1994] and Peterson's status report of the field: [Peterson, 1994].

[371][Barrett, Corrigan, Kuhn and Wolff, 1979, 23].

[372][Sewell, Jr., 1999, 37].

[373][Archer, 1985].

[374][Swidler, 1986, 273].

[375][Kane, 1991; Alexander, 1992].

[376][Wright, 1971].

[377][Hayes, 1985].

culture denotes symbolic systems that are deeply embedded, taken-for-granted
... [whereas] sociology of knowledge ... directs attention to cultural elements
that are more conscious..."[378]   In some tension with this, however, there is a
notable concentration on knowledge producers, which at its extreme generates a
new "sociology of intellectuals" that repeats with difference the preoccupations of
the "old" sociology of knowledge.[379] The difference lies in the "attention to the
specific organizational contexts in which knowledge producers work."[380] Hence
the proliferating new field of inquiry into "knowledge societies."[381] Above all,
Foucault's "disciplinary regimes" and his contention that "techniques of power
are also, simultaneously, forms of knowledge" seem to be central to the "new"
sociology of knowledge.[382] Theorizing reflexively upon university intellectuals and
especially social theorists, Pierre Bourdieu emerges as a second decisive force for
this "new" sociology of knowledge.[383]

The most salient difference between the European and the American develop-
ments Lamont and Wuthnow discerned was that from the outset the Europeans
showed a pervasive concern with the relation between culture and power, whereas
the Americans came to this only very belatedly. European thought, following
Gramsci, called for analysis of "the subjective process of consolidation of class
domination." Hence Foucault's concern with "discursive regimes," and Bourdieu's
conception of "symbolic violence." "For Foucault and Bourdieu, cultural codes
also structure social relations by defining boundaries between groups, therefore
excluding groups from access to resources and positions."[384] American sociolo-
gists received these European influences via the mediation of "popular cultural
studies, symbolic anthropology, cultural history, literary criticism and women's
studies."[385] The key difference American cultural sociologists claim between their
version of "cultural studies" and the Europeans is that the latter lapsed into de-
terminism, making subjects over into "cultural dopes," even if in the sophisticated
formulations of Bourdieu or Foucault, whereas the new American cultural sociol-
ogists seek a better balance between structure and agency.[386]

American cultural sociology of the late 1980s and the1990s set itself the task of
working through the theoretical resources developed in continental structuralism
and poststructuralism, above all Foucault and Bourdieu, as well as in the work
of theorists in the "interpretive" tradition of social theory, like Clifford Geertz,
Anthony Giddens, and Jürgen Habermas, to make clearer sense of the relations
among structure, agency and culture. A recent anthology entitled *The New Amer-
ican Cultural Sociology* [1998] brings together some of the seminal works in the

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[378][Swidler and Arditi, 1994, 306].
[379][Kurzman and Owens, 2002].
[380]pSwidler and Arditi, 1994, 312[.
[381]pStehr and Ericson, 1992; Stehr, 1994].
[382][Swidler and Arditi, 1994, 314].
[383]Ibid., 317-318.
[384][Lamont and Wuthnow, 1990, 295-297].
[385]Ibid., 294.
[386][Sherwood, et al, 1993; Alexander, et. al., 1993, 10-11].

new field.[387] In his introduction to the volume, Philip Smith both recognizes the diversity of approaches and seeks to construe their commonality.[388] It was and remains, as all its exponents agree, a congeries without an identifiable core of consensus.[389] Cultural sociologists do share issues, above all the structure/agency problem. Crucial to the American theorists is the effort to elaborate a more effective notion of agency, avoiding the two extremes of structuralist determinism and voluntarist freedom, neither of which seem adequate to the theoretical or empirical task. Thus they endeavor to situate the concept of culture effectively in this force-field between structure and agency.[390]

Two essays by Sewell offer significant clarification of the crucial relations of culture, structure, and agency. In "The Concept(s) of Culture," Sewell discriminates between two "fundamentally different meanings" of the term *culture*: as an abstract category for one form of (social) structure, and as a concrete, bounded unit of beliefs and practices, as in the ethnographic sense of culture(s). Sewell makes the convincing point that the anthropologists' disillusionment with the second notion cannot be allowed to carry over to an abandonment of the first. He is prepared to jettison the holistic idea of (ethnographic) culture(s), in line with the general trend: "It now appears that we should think of worlds of meaning [his phrase for ethnographic units] as normally being contradictory, loosely integrated, contested, mutable, and highly permeable."[391] But he holds out for at least a "thin" notion of coherence, not only at the level of system, but also at the concrete ethnographic level, a coherence which is itself "variable, contested, ever-changing, and incomplete," yet, notwithstanding, empirically interpretable.[392]

Picking up on a left-handed avowal from James Clifford, Sewell insists "we cannot do without a concept of culture," and therefore "we need to modify, rearticulate, and revivify the concept."[393] Two aspects that he seeks to rearticulate are culture as system and culture as practice. While each had a separate tradition of advocacy within anthropology, as Ortner outlined, Sewell urges that these are "complementary concepts." Thus, "system and practice constitute an indissoluble duality or dialectic."[394] The real challenge is to cash out this idea of duality and dialectic: to show how structure (in this case, *cultural* structure) and practice form a mutuality without obfuscating the distinction. That is what he turns to in the second essay, "A Theory of Structure: Duality, Agency, and Transformation," in which he sets out from the "most promising existing formulations" by Anthony

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[387][Philip Smith, 1998].

[388][Philip Smith, 1998].

[389]Cultural sociology is not "a unified subdiscipline with shared theoretical emphases or a common set of empirical problems" [Wuthnow and Witten, 1988, 65]. Given the "absence of any strong core of conceptual agreement" in the field, there is "still no clear and solid ground." [Rambo and Chan, 1990, 636].

[390]Particularly discerning in that regard is the essay by Hays, [1994].

[391][Sewell, Jr., 1999, 53].

[392]Ibid., 57.

[393]Ibid., 38. He is picking up on Clifford's avowal: "culture is a deeply compromised idea I cannot yet do without," in [Clifford, 1988, 10].

[394]Ibid., 47.

Giddens and Pierre Bourdieu to elaborate and refine the dialectic.[395]

Sewell begins with Giddens's theory of structuration, arguing that Giddens had undertheorized his key components of structure, namely *rules* and *resources.* Rules, Sewell notes, seemed too fixed and static. Giddens intended that these rules be "generalizable," but Sewell suggests that, too, had deterministic overtones. In place of Giddens's terms, he proposes the notion of a *schema,* which "can be actualized in a potentially broad and unpredetermined range of situations," and the notion of *transposability*, which allows greater latitude for application.[396] He is even less satisfied with Giddens's treatment of resources. These must already be materially available to be construed by a schema for the actualization of structures. Resources develop unpredictably in response to many factors beyond discursive structures, but they are also "polysemic" — available to multiple construals within and across cultural schemas. Conversely, structural schemas, to be enacted, require resources. Sewell creates space both for agency and for social change: "because structures are multiple and intersecting, because schemas are transposable, and because resources are polysemic and accumulate unpredictably," in every enactment, structure is at risk.[397]

Turning to Bourdieu, Sewell observes: "Bourdieu has not overcome the lack of agency inherent in the concept of habitus elaborated in *Outline of a Theory of Practice.*"[398] There is a strong danger, in Bourdieu's sociology, of "a single direction of causality" which reduces agency to instantiation of preprogrammed structures.[399] Thus Bourdieu did not escape the structuralism against which he saw himself rebelling, but offered still "an impossibly objectified and overtotalized conception of society."[400] At least Giddens insisted upon "knowledgeable" actors, and Sewell holds they are "far more versatile than Bourdieu's account ... would imply."[401] The notion of structure must be loosened to make variability — and hence social change — theoretically conceivable. There must be "many distinct structures . . . at different levels ... in different modalities... based on widely varying types and quantities of resources" to make place for a theory of change. "A theory of change cannot be built into a theory of structure unless we adopt a far more multiple, contingent, and fractured conception of society — and of structure."[402]

I would like to close this overview with a consideration of one striking exemplar of this complex new sociology of knowledge: the work of Margaret Somers, who identifies her work explicitly with "a 'cultural turn' in the history and sociology of knowledge."[403] As against the "classical" form of Marx and Mannheim, she

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[395][Sewell, Jr., 1992, 4].
[396]Ibid., 8, 17.
[397]Ibid., 19.
[398]Ibid., 14.
[399]Ibid., 12.
[400]Ibid., 15.
[401]Ibid., 17.
[402]Ibid., 16.
[403][Somers, 1999, 124].

advocates "a vigorous retrieval and embrace of a new kind of sociology of knowledge" not about external interests which determine knowledge but rather about its "conditions of possibility" as a function of "cultural autonomy."[404] This new, cultural form of sociology of knowledge asks *how* concepts work, in terms of the network of their relations and its systemic logic, not *why,* in terms of material determinations.

Somers constructs her approach in response to intense theoretical contestation of the legitimacy of her field of "historical sociology."[405] First, its essential concepts — particularly agency and structure — encountered theoretical impasses, such that every attempt to theorize agency sociologically seemed to reduce it to an epiphenomenon of some (material) structure.[406] But there was an even deeper level of dispute in which historical sociologists were challenged by general sociologists with having given up the authentic nomothetic ambitions of theory for a merely historical descriptiveness.[407] That is, not only substantive concepts but also epistemological standards were at stake. To break free of these impasses required a move of radical reflexivity. "The real challenge is to problematize our problems . . . by deconstructing and historicizing our conceptual frameworks, categories, and vocabularies."[408]

This task of "unthinking" proves inordinately difficult, Somers establishes, because empirical counterinstances seem to have no real impact upon the entrenched structures of the discipline.[409] This leads her to conceptualize what she calls "knowledge cultures," and, in particular, a "metanarrative" in which certain ways of seeing the world become "naturalized" into presuppositions which cannot be displaced singly, but must be dealt with systematically.[410] In particular, Somers identifies as the knowledge culture dominating current social science the metanarrative she terms "social naturalism."[411] To "unthink" this constraining metanarrative of social naturalism, its *own* artifice needed to be unearthed: it needed to be deconstructed as itself "merely" cultural. That is, what was required was a radical historicization of current knowledge culture. "Linking epistemology to the historicity of its production enables us to question the 'primordial' distinctions between nature and culture and to demonstrate the contingency of the epistemological framework."[412]

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[404] Ibid., 135; See also [Somers, 1996, 55; 1995, 115].

[405] [Somers, 1996] and the literature cited there.

[406] Ibid., 78.

[407] Ibid., 58.

[408] Ibid., 73.

[409] The notion of "unthinking" was taken from [Wallerstein, 1991]; [Somers, 1999, 132].

[410] "A metanarrative not only provides the range of acceptable answers but also defines both the questions to be asked and the rules of procedure by which they can *rationally* be answered. No alternative empirical approach ... can be considered seriously until the 'gatekeeping' power of the dominant metanarrative is challenged historically." [Somers, 1995, 115-6n]. "Metanarratives ... are accountable only to 'ideal-typical' historical accuracy — thus making them immune from serious empirical challenge and conducive to problematic explanations." [Somers, 1995a, 256]

[411] [Somers, 1995, 115].

[412] [Somers, 1999, 156].

"The theoretical task is thus a historical one," Somers writes.[413] It is essential to establish that all knowledge cultures are "history-laden."[414] Hence she proposes a "historical epistemology," a "historical sociology of concept formation." Somers urges us to understand "knowledge culture" as "a conceptual network . . . dedicated specifically to epistemological concepts and categories of validity."[415] As she explicates this notion, *knowledge* betokens what any way of thinking in a particular time and place *credits* with truth.[416] (She is less concerned with the *validity* of this ascription of validity than with its *efficacy* in any given cultural moment.) She defines *culture* explicitly as "intersubjective symbolic systems and networks of meaning-driven schemas organized by their own internal rules and structures."[417] Thus a "knowledge culture" is an autonomous structure organizing knowledge into a system or network, "buttressed by an epistemological infrastructure that verifies its truth claims."[418] The three features that Somers accentuates in her theory of knowledge cultures are *reflexivity*, bringing the conceptual structures of theory themselves under scrutiny; *relationality*, interpreting concepts always in terms of the network in which they are embedded; and *historicity*, i.e., that "successful truth claims are products of their time and thus change accordingly."[419] Above all, "concepts are 'history-laden'."[420] They "have histories of contestation, transformation, and social relationships," and it is the task of a historical sociology of concept formation to unearth "how concepts gain and lose their currency ... reconstructing their making, resonance, and contestedness in time."[421]

To establish her notion of relationality, Somers draws explicitly on the legacy of structural linguistics, seeking the "internal logics" of the "web or . . . structured configuration" in which alone concepts have meaning.[422] She invokes Ian Hacking's insistence upon understanding concepts as "words in their sites."[423] It is as such webs or networks that culture deploys itself as "a form of structure in its own right, constituted autonomously."[424] She conceives of "numerous, often competing conceptual networks, mediated by a multiplicity of power relations."[425] She identifies these with Sewell's "cultural schemas." These may be "narrative structures, binary codings, patterned metaphors or sets of metaphors, symbolic dualities, or practices of distinction."[426] The crucial point of relationality is that one must

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[413][Somers, 1996, 74].
[414][Somers, 1999, 135].
[415]Ibid., 134.
[416]Ibid., 124.
[417]Ibid.
[418]Ibid., 125.
[419]Ibid., 134. See also [Somers, 1995a, 232].
[420]Ibid., 135.
[421][Somers, 1999, 135].
[422][Somers, 1995, 135].
[423]See her epigraph to [Somers, 1995, 113]. See [Hacking, 1990].
[424][Somers, 1995, 131].
[425]Ibid., 136.
[426][Somers, 1999, 125].

"explore the internal dynamics of a cultural schema on its own terms."[427] But knowledge cultures are simultaneously political, because they constitute power relations in and through codifying meanings.[428] "The power/culture link is built into the very nature of the symbolic logics, boundaries, differences, and demarcations."[429] Here the influence of Foucault and Bourdieu on her theorizing is paramount.

Somers derives from the work in history and sociology of science that the Received View's distinction between the context of discovery and the context of justification cannot be upheld.[430] Accordingly, the context of discovery becomes a much more fruitful domain for theorizing.[431] It points especially to the critical role of problem-formulation, of question-posing, in knowledge cultures. As Collingwood argued long ago, without knowing the questions posed, it is hard to grasp what the answers signify. But precisely what a "knowledge culture" approach directs us to ask is "where do we get the questions?"[432] We must "explore how, when, and why questions change over time."[433]

To grasp the historicity of knowledge, Somers offers a radically revisionist notion of "causal narrativity."[434] First, she disputes energetically the dominant theory of explanation in analytic philosophy, the covering-law model of Hempel.[435] She argues that in this model causality has in fact been displaced by a notion of a priori logical entailment.[436] Because the model has been so hegemonic, hermeneutic theorists have rejected any notion of causality whatsoever, wanting nothing to do with the covering-law model. Both, as a consequence, miss an alternative approach to causality, which is to take it resolutely as the historical reconstruction of contingency.[437] This is the idea of causal narrativity that Somers develops in great detail.

There has arisen, Somers argues, a new vein of theorizing about narrative: narrative as *social epistemology and ontology*, a body of interpretation that suggests that such stories are the very stuff of social action. "Social identities are constituted through narrativity, social action is guided by narrativity, and social processes and interaction — both institutional and interpersonal — are narratively mediated."[438] "Narrative is an *ontological condition of social life*."[439] "It is through narrativity that we come to know, understand, and make sense of the social world . . . We come to *be* who we *are* (however ephemeral, multiple, and

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[427]Ibid.
[428][Somers, 1995a, 236].
[429][Somers, 1995, 133].
[430]For more on the unravelling of this distinction, see [Zammito 2004].
[431]For parallel work which substantially supports her view, see Thomas [Nickles, 1980].
[432][Somers, 1996, 71].
[433]Ibid., 72.
[434]Ibid., 79.
[435][Hempel, 1965].
[436][Somers, 1996, 59].
[437]Ibid., 79.
[438][Somers, 1992, 606].
[439][Somers, 1994, 614].

changing) by our location (usually unconsciously) in social narratives and networks
of relations that are rarely of our own making."[440] The new theory of narrativity
stresses four features: relationality, causal emplotment, selective appropriation,
and historicity.[441] That is, narratives are *networks* ordering elements which have
no sense separately.[442] The ordering is *causal* in precisely the sense that Somers
invokes against the Received View, i.e, "inherently narrative and historical."[443]
"To make something understandable in the context of what has happened is to
give it historicity and relationality."[444] And in order to construe the emplotment
in this causal manner, "evaluative criteria" are intrinsic to the narrative, which
is what she means by "selective appropriation," the prioritizing of themes and
meanings. Finally, these narratives postulate that things change with time and
space. They embody historicity as they construct it. Somers concludes that "the
range of considerations about ontologies, philosophical anthropologies, and even
our views of reality are historically contingent."[445] This "does not mean we may
have no good reasons for having our own standards of truth and reason," but it
does suggest "a certain degree of agnosticism concerning foundations."[446]

3   CONCLUSION: "THEORY" IN SEARCH OF THE "SOCIAL"

In the introduction to their very important volume, *Beyond the Cultural Turn*,
Bonnell and Hunt offer a most discerning account not only of where the human
sciences have been over the last thirty years but also of where they might most
profitably move now.[447] I find myself in general sympathy with both their diag-
nostic history and their prospective proposals. What is clear in their account is
that while "during the 1980s and 1990s, cultural theories, especially those with
a postmodernist inflection, challenged the very possibility or desirability of social
explanation," this postmodernist view is now subject to a powerful revisionism.[448]
They note that the social "began to lose its automatic explanatory power" largely
because of the collapse of positivist and orthodox Marxist explanatory paradigms
of their own (dead) weight. Yet, with poststructuralism and postmodernism, "the
cultural turn threatened to efface all reference to social context or causes and of-
fered no particular standard of judgment."[449] Accordingly, today the human sci-
ences might well "find common ground again in a redefinition or revitalization of

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[440][Somers, 1992, 600].
[441]Ibid., 601-602; [Somers, 1994, 616-617].
[442]"I draw a clear connection between narrative and networks ... the logic of any single element
carries within it embedded fragments of the entire causal plot." [Somers, 1995a, 242].
[443][Somers, 1996, 79].
[444][Somers, 1992, 602].
[445][Somers, 1996, 64].
[446]Ibid., 66.
[447][Bonnell and Hunt, 1999].
[448]Ibid., 3.
[449]Ibid., 9.

the social."[450] Similarly, after the massive invective against "disciplinarity," they suggest that "'redisciplinization' seems to be in order," since "interdisciplinarity can only work if there are in fact disciplinary differences."[451] That does not mean that the challenges of poststructuralism to epistemology and methodology can be forgotten; "epistemological and even ontological issues are invariably raised by the cultural turn," and reflexivity is inescapable.[452] But by the same token, the fatal impasses in radical reflexivity that postmodernism saw at every turn are coming to seem more hallucinatory than necessary, or, as one commentator put it with admirable obliqueness, they seem now "philosophically undermotivated."[453]

"Social construction" represents the theoretical effort to discern that and how the experience of individuals is prefigured by collective structures, material or cultural, which both enable and constrain identity and action. The realization of difference, pronounced in the encounter with historical or cultural otherness (historicism; cultural relativism), combines with a recognition of the degree to which what appears natural in one's own social-cultural context is "always already" instituted by linguistic and social forms whose pervasive order informs and perhaps even *determines* perception and agency (the hermeneutic of suspicion). The result can be politically contestatory, in that it disputes any "naturalness" to the ascription of roles, statuses, or identities to subordinated groups within a society or across societies. But it can also be epistemologically and even politically disabling, because it puts under question every judgment individuals make, on the suspicion that it has been preemptively structured by an alien impulse over which the individual has neither cognitive nor active control. This notion of the "cultural dope" has been one of the most important complications in the turn to the "social construction" of reality. While it is one thing to dispute the naturalness of the findings of natural science and *a fortiori* of social science so as to create the space for alternative possibilities, it is quite another to put the efficacy of human choices and the actuality of human knowledge under theoretical erasure (as in the "death of the subject"). This is not simply to appeal to the old logical gambit of the "liar's paradox" that the claim to know that knowledge is preemptively "constructed" is a knowledge-claim which falls under its own ban. It is rather to appeal to the political motive that has ostensibly animated the turn to "social construction," namely the emancipatory ambition of critique. And, I would add, it is to insist upon a far more energetic defense of pragmatic and empirical — non-foundationalist — notions of epistemology, judgment, and warrant. It is simply hyperbolic on any sound account of human judgment to adopt the positions which radical versions of poststructuralism and postmodernism have proffered about the preemptive and disseminative character of language, ideology, discursive regimes or the unconscious, just as it is simply crude and empirically bogus to proffer claims about the socio-economic determination of human choice along the lines of orthodox Marx-

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[450]Ibid., 11.
[451]Ibid., 14.
[452]Ibid., 13.
[453][Brightman, 1995, 525]. For a thoughtful postmodernist stance, see [Seidman, 1991].

ism. Each of these influences calls for empirical scrutiny and appraisal, not *a priori*
postulation. If the hyperbolic element in "social construction" is abandoned, we
are thrown back to concrete cases and actual resources for judgment and choice,
and the terms of that kind of enterprise, its promise and its limits, follow a different
script altogether, abjuring absolutes whether positive or negative, and settling for
partiality, for dispute, and yet for determinate adjudication concerning structure,
agency, materiality and culture within bounded discursive communities. The only
way forward, as Helen Longino insists, is to "understand the cognitive process
of scientific inquiry not as opposed to the social, but as thoroughly social."[454]
The crucial point is that objectivity is a collective/communal, not an individual
achievement.[455] "It is not the individual's observations and reasoning that matter
in scientific inquiry, but the community's."[456] That is, objectivity is "a process of
critical emendation and modification of . . . individual products by the rest of
the scientific community."[457]

One of the leading proponents of American cultural studies recognizes the need
"to disarticulate cultural studies from the modern 'discovery' of the social con-
struction of reality."[458] Indeed, postmodernism's epistemological extremity and
hermetic foreclosure of human action within deep, encarcerating codes ("social con-
structionism" all the way down) have been good for neither theory, nor research,
nor emancipatory politics. Positivism was, indeed, a very powerful anaesthetic.
Sometimes coming out of anesthesia there are hallucinatory bouts. Perhaps that
is what postmodernism was.

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[454][Longino, 1992, 201].

[455]"The objectivity of scientific inquiry is a consequence of that inquiry's being a social and
not an individual enterprise." (Ibid., 205.)

[456]Ibid., 207.

[457][Longino, 1990, 68].

[458][Grossberg, 1997, 31].

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