Science, Technology &
Human Values
http://sth.sagepub.com/

Anthropology and the Cultural Study of Science

Emily Martin

Science Technology Human Values 1998 23: 24
DOI: 10.1177/016224399802300102

Anthropology and the Cultural Study of Science

Emily Martin
Princeton University

This essay explores how the distinctively anthropological concept of culture provides uniquely valuable insights into the workings of science in its cultural context. Recent efforts by anthropologists to dislodge the traditional notion of culture as a homogenous, stable whole have opened up a variety of ways of imagining culture that place power differentials, flux, and contradiction at its center. Including attention to a wide variety of social domains outside the laboratory, attending to the ways nonscientists actively engage with scientific knowledge, and focusing on the complex interactions that flow both into and out of research laboratories are ways the activities of both scientists and nonscientists can be situated in the heterogeneous matrix of culture. Three images—the citadel, the rhizome, and the string figure—allow us to picture the discontinuous ways science both permeates and is permeated by cultural life.

1994 was the first year an anthropologist, Linda Layne, held the position of program chair for the Society for Social Studies of Science (4S) meeting. It was also the first 4S meeting at which two anthropologists had been asked to give keynote addresses, as well as among the first that did not overlap with the meetings of the American Anthropological Association. I regard my address as a wonderful opportunity, a space in which to explore what anthropology might be able to contribute to science studies. But I admit to a certain amount of disquiet in the face of the barrage of recent negative comments about my field. It has been said that studies of science by anthropologists lack "theoretical purchase" (Woolgar 1989) and that it is nearly impossible for anthropologists to carry out useful studies of science (Latour 1990). Alongside the defensiveness naturally aroused by such accu-

---

AUTHOR'S NOTE: This article is a slightly revised version of my keynote address at the October 1994 meetings of the Society for Social Studies of Science, New Orleans, LA. I thank Linda Layne for inviting me to give this lecture, and Olga Amsterdamska and the anonymous reviewers of *Science, Technology, & Human Values* for useful suggestions for revision.

Science, Technology, & Human Values, Vol. 23 No. 1, Winter 1998 24-44

sations, as a recent participant in science studies I also feel considerable humility. The field of social and cultural studies of science is already thickly dotted with the flags of explorers from disciplines in the social sciences and humanities, many wielding selectively some of the analytic categories and practical techniques of anthropology. In a recent review, Sharon Traweek (1993) counts scholars in a least twenty academic disciplines engaged in the study of science, medicine, and technology. To these, one would want to add the critical work of Third World scholars such as J. P. Singh Uberoi, who, in his book *The Other Mind of Europe* (1984), questions whether different conceptions of knowledge might have arisen from a different historical starting place, such as Paracelsus or Leibniz, rather than Newton and Copernicus.

What might a cultural anthropological study of Western science look like?[1] First, it would depend on achieving a profoundly (and tantalizingly) elusive stance. This stance would entail one of those impossible conundrums, like trying to push a bus in which you are riding (in an image from Berger and Luckman) or trying to see, like the fish in Marx's example, the invisible water in which one swims. If science is the ground of nature, as well as the ground of my thought about it, then how can I think about science outside of itself?

A second feature of the anthropology of science as I envision it would involve looking at science simultaneously as a "particular story of how things stand"[2]—that is, interpreting it as meaningful social action—and as an important part of the institutions that are exerting particularly brutal forms of power in the contemporary scene. Scientific institutions are implicated in large-scale political economic forces that can be universal in their scope and that are often damaging in their effects. These forces involve the increasing concentration and mobility of capital, which often lead to greater misery for the poor; concomitant restructuring of the organization of work, both inside corporations and factories and in the spread of "homework." They entail vast alterations in how information is stored and retrieved and the focus on genetics in biological and medical research. Some of their effects include dramatic changes in how scientists and the person on the street conceptualize the components of the human body and the determinants of health, the occurrence of virulent forms of racism, and an intense new biological essentialism.

Even if this elusive stance and simultaneously interpretive and political economic analysis could be achieved, would cultural anthropology have anything new to add to science studies? Of course, my zest for this possibility is only whetted by the recent disparagement of what anthropology can do. I believe the disparagement is based on a failure to see the terrain that cultural anthropology explores. To bring this terrain into relief, I will make use of three images: citadels, rhizomes, and string figures.

Citadels

The natural sciences of the present day are heir to processes that have left most of us thinking they are set apart from the rest of history and society, something like a citadel in Webster's definition: "a fortress that commands a city, both for control and defense." What sets the sciences apart is that they claim to construct reality but not to be themselves constructed. In Traweek's (1988) terms, they appear to be "cultures of no culture." In a challenge to the veracity of this view, some anthropologists and many others in science studies have begun to depict the (in fact) very rich and complex cultures of the natural sciences, ensconced within their citadels apart and above the rest of society. Traweek has described some of the fundamental presuppositions about time, space, matter, and persons that give meaning to the world of a high-energy physicist; she has shown how those fundamental presuppositions take one form in the United States and another, quite different form in Japan, emphatically demonstrating their historically contingent nature.

Traweek's (1988) groundbreaking work opens up intriguing new questions: what would happen if the ethnographer wandered around outside the citadel walls and tried to find out why people do or do not support physics in the form of the supercollider? What if she tried to find out why Steven Hawking's book *A Brief History of Time* (1988) and the video based on it have become bestsellers? Now that we have the benefit of Traweek's work, the way is cleared to connect the lab in which high-energy physicists work with their machines and the living room in which a family watches Hawking talk about the universe, thus calling into question the solidity of the citadel walls.

There is another major approach to the walls of the citadel, actor-network theory (ANT), which certainly makes the walls between the activities of scientists and the rest of society permeable. Although I risk treading a well-worn path, I want to focus specifically on Latour's version of ANT because it will allow me to throw into relief what an anthropological concept of culture could add to science studies. In Latour's account, we see how the making of facts, and the resources necessary to make them, depend on gathering allies in many places. Scientists must travel about into government agencies, manufacturing concerns, press offices, publishing houses, and so forth, all the while communicating intensively, to build up supporters for the facts they wish to establish:

> They travel inside narrow and fragile networks, resembling the galleries termites build to link their nests to their feeding sites. Inside these networks, they make traces of all sorts circulate better by increasing their mobility, their speed, their reliability, their ability to combine with one another. (Latour 1987, 160-61)

Although Latour's version of ANT, "science in action," breaks down the walls of the citadel, and I have been inspired and stimulated by this aspect of it, I would claim that this gain comes at too high a price. The Latourian scientist bursts upon the scene as an accumulating, aggressive individual born of capitalism, forming his networks and gathering his allies everywhere, resembling all too closely a Western businessman. We may wonder whether all scientists have, or ever had, such an monadic, agonistic, competitive approach to the world (Birke 1986). In particular, we might wonder whether some women scientists (for a host of reasons more excluded from the citadel than men) may have lived their scientific lives very differently and garnered a measure of success in spite of it (Keller 1983).[3]

Numerous scholars in science studies have already expressed reservations about Latour's (1987) agonistic model; others have made efforts to extend the model in new ways (Amsterdamska 1990; Collins and Yearley 1992; Fujimura 1992). One feature of the Latourian approach, a feature that relates directly to what is not anthropological about it, is that even if Latour's description of the scientist as accumulator is accurate in all its details, the description is never placed in a larger historical or cultural context. The network-building, ally-enrolling scientist is poised in a timeless, universal arena. Much like the cells or particles that scientists are trying to describe in the laboratory, from Latour's perspective, scientists always seem to behave in the same way. Also, like those cells or particles, scientists are stripped down to, reduced to, simple forms—in this case, the competitive, aggressive, accumulating individual.

If we try to put science in a larger context, will we run up against Latour's (1987) claim that you cannot use "society" to explain "nature"? This, he argues, is because what we take to be "society" and "nature" are both produced through the same kind of processes. Similarly, he would argue, you cannot use the state of the economy unproblematically to explain science because the economy is the outcome of another science, economics. The same goes for society, the outcome of sociology: "The very definition of a 'society' is the final outcome, in Sociology Departments, in statistical institutions, in journals, of scientists busy at work gathering surveys, questionnaires, archives, records of all sorts, arguing together, publishing papers, organizing other meetings" (Latour 1987, 257). For Latour, all kinds of knowledge are produced by exactly the same processes: "calibrating inscription devices, focusing the controversies on the final visual display, obtaining the resources necessary for the upkeep of the instruments, building *nth* order theories on the archived records" (p. 256). All kinds of knowledge also are (by definition) produced in the same manner, by scientific experts.

Here is where anthropology can make its radical critique. What if network building and resource accumulation are not the only way knowledge is established? What if many other kinds of processes proceeding from fundamentally different assumptions about the world profoundly affect experts and scientists even as they accumulate resources and build networks? What if important, forceful processes flow into science as well as out of it? What if "nature" is not simply what natural scientists tell us it is, and what if "society" is not simply what sociologists tell us it is? What if instead people who call themselves scientists are continuously interacting with, and being profoundly affected by, people who do not call themselves scientists? What if, in complex historical circumstances, both scientists and nonscientists are forging ways of acting, being, and thinking in the world, or in other words, forging what anthropologists call cultures?

Introducing the anthropological concept of culture is not a simple matter, because of the current ferment within anthropology about how "culture," historically a key concept of the discipline, should be understood. Speaking directly to colleagues in science studies who operate with a different set of meanings attached to "culture," Traweek explains the anthropological sense:

> To anthropologists, "culture" is not all about vestigial values, "society" is not all about agonistic encounters, and "self" is not about autonomy and initiative. A community is a group of people with a shared past, with ways of recognizing and displaying their differences from other groups, and expectations for a shared future. Their culture is the *ways*, the strategies they recognize and use and invent for making sense, from common sense to disputes, from teaching to learning; it is also their ways of making things and making use of them and the ways they make over their world. (1992, 437-38)

Faced with the task of educating those outside anthropology about the dynamic quality of the anthropological concept of culture, Traweek stresses culture's role in any community with a shared past. But speaking to other anthropologists, Lila Abu-Lughod makes the point that use of the concept of culture has a tendency to "overemphasize coherence." "If 'culture,' shadowed by coherence, timelessness, and discreteness, is the prime anthropological tool for making 'other,' and difference . . . tends to be a relationship of power, then perhaps anthropologists should consider strategies for writing against culture" (1991, 147). "Writing against culture" would entail writing about similarities that crosscut difference and processes that introduce flux and contradiction into otherwise apparently stable wholes.

With this cautionary note, an anthropological approach to science as culture would depend on our ability to describe how scientific knowledge is produced across a large array of domains. This task would be greatly aided by the work of numerous science studies scholars who have examined important aspects of the ways science is embedded in society: how scientists interact with nonscientists (Callon 1986; Latour 1988); how science can be seen as culture and contains many different "cultures" (Collins 1975; Dupré 1993; Galison and Stump 1996; Jasanoff et al. 1995; Knorr Cetina 1991, 1992; Pickering 1992); how scientific knowledge is as socially constituted as other forms of knowledge production (Amsterdamska 1990; Ashmore 1989; Woolgar 1988).

Building on this considerable terrain, an anthropological approach would press for a much more inclusive notion of what counts as pertinent to the objects of scientific knowledge and practice. Instead of allowing the particular interests of the scientists to determine which persons, settings, and materials are relevant, one could grant nonscientists coming from many different cultural settings the ability to alter the agendas of scientific research or the uses of scientific materials. Exploration of this issue has been begun by anthropologists like Rayna Rapp and Deborah Heath. As acute observers of the landscape, they notice what should have been obvious to us all along—that many powerful collectives and interested groups dot the landscape all around the citadel of science. These groups interact with the world inside the citadel of science frequently and in powerful ways. It is as if we thought of science as a medieval walled town, and it turns out it is more like a bustling center of nineteenth-century commerce, porous and open in every direction. So Rapp (1988) describes how genetic counselors (they are not research scientists but professionals whose specific job is to translate between the science of genetics and the public) communicate the meaning and implications of genetics and genetic testing to pregnant women. The women to whom they translate science are not passive recipients. Rapp's vivid material makes plain the complexity of fitting new knowledge to diverse lives: a working-class single mother chose to keep an XXY fetus (with Klinefelter's syndrome), declaring him at the age of four, "normal as far as I am concerned . . . and if anything happens later, I'll be there for him, as long as he's normal looking" (Rapp 1988, 152). On the other hand, a professional couple chose to abort a fetus with this same Klinefelter's syndrome, saying, "If he can't grow up to have a shot at becoming the President, we don't want him" (p. 152). The same "facts" are given a very different kind of life.

Positioning herself in a similar way, Heath (1993) has done ethnographic work on the interface between a genetics lab and people with Marfan syndrome, a connective tissue disorder caused by a genetic abnormality, the one that probably affected Abraham Lincoln. In this interface zone, she finds the National Marfan Foundation, the U.S. lay organization for affected individuals and their advocates, holding meetings attended by genetics researchers as well as people affected by Marfan syndrome. Here, the interface between the world of science and the public becomes membrane thin. Researchers are "hurt" when people with Marfan syndrome do not like the evidence presented to them by scientists working inside the citadel. In particular, they did not like to look at a slide of an electron micrograph of the molecule altered by the genetic abnormality that affects them (someone in the audience hissed at the villain responsible for Marfan syndrome). Looking outward from their citadel, researchers are "amazed" to see the phenotypic variability in the people who share Marfan's.

In an emotional consequence of this close contact between the inside and outside of science, one researcher became angry at Marfan patients after a National Marfan Foundation conference. She was unsettled because the patients thought she was responsible to them. This expectation clashed with her belief that pure science should determine its own course even while she acknowledged that with a little readjustment she could push her research in directions that might provide more therapeutic findings (Heath 1993). These events *could* be described as "scientists accumulating resources and acquiring allies," but only at the cost of stripping them entirely of the complex cultural details that confer meaning.

In sum, ethnographic research suggests that the strict, fixed borders between the citadel of science and the "untutored" public do not hold up to scrutiny. The walls of the citadel are porous and leaky. Action and initiative go in both directions. It is less "science in action" than "knowledge in action" in a multitude of contexts, both scientific and nonscientific. Note that I am not attempting to explain science by society asymmetrically. Rather, I am claiming that both "science" and "society" as categories are produced inside the heterogeneous matrix of culture, the missing term in ANT. Culture, meaning fundamental understandings and practices involving such terms as the person, action, time, space, work, value, agency, and so on, is produced by a far wider range of processes than those deployed by experts producing science. It is that terrain, rendered invisible in ANT, and regarded as irrelevant to the understanding of scientific cultures in some parts of science studies, on which anthropologists move around.

Rhizomes

To see this terrain, we need to ask whether the layout and design of the citadel itself, the logic of the actions of the scientists within it, may not be as deeply embedded in the same countryside as the hamlets and villages surrounding it. This question would lead us to wonder how knowledge in the citadel, and its manner of production, might be linked with processes and events outside. To avoid assuming a *one-way* linkage in which scientific knowledge flows from the citadel out, or scientists choose autonomously the resources outside the citadel they need, we can rely on an image from Deleuze (1993)—the rhizome. A rhizome, like crabgrass or bamboo, has an underground rootlike stem that sends up leafy shoots from the upper surface and roots from the lower. Unlike other plants, such as pine trees, which depend on all their major parts (roots, trunk, and leaves) to live and which propagate by seeds, rhizomes can be broken entirely apart into segments and still grow up again as complete organisms:

> A rhizome as a subterranean stem is absolutely different from roots and radicles. Bulbs and tubers are rhizomes. Rats are rhizomes. Burrows are too, in all of their functions of shelter, supply, movement, evasion, and breakout. The rhizome itself assumes very diverse forms, from ramified surface extension in all directions to concretion into bulbs and tubers. . . . Any point of a rhizome can be connected to anything other, and must be. This is very different from the tree or root, which plots a point, fixes an order. . . . A rhizome may be broken, shattered at a given spot, but it will start up again on one of its old lines, or on new lines. You can never get rid of ants because they form an animal rhizome that can rebound time and again after most of it has been destroyed. . . . [Rhizomic things] evolve by subterranean stems and flows, along river valleys or train tracks; [they] spread like a patch of oil. (Deleuze 1993, 29, 32, 30)

This image might do well to capture the kind of discontinuous, fractured and nonlinear relationships between science and the rest of culture that Donna Haraway has traced, for example, in the convoluted lines between primatology and movies about primates:

> The women and men who have contributed to primate studies have carried with them the marks of their own histories and cultures. These marks are written into the texts of the lives of monkeys and apes, but often in subtle and unexpected ways. . . . Monkeys and apes—and the people who construct scientific and popular knowledge about them—are part of cultures in contention. (1989, 2)

Are there ways of studying ethnographically the discontinuous, nonlinear ways that might link the citadel to the rest of the world? In my own recent work, I have been making efforts to do that—but legitimacy for such efforts may not come easily. When I described the range of sites at which my research was taking place (an immunology lab, various clinical HIV settings, AIDS activist volunteer organizations, several urban neighborhoods, and corporate workplaces), a science studies colleague was dismayed. Coming from a tradition of studying science in great detail inside the laboratory, she asked me, "Don't you know how to stay put?"

But with the image of a rhizome in mind, anthropologists of science need not be confined. Some people can peer over the castle walls; some can look through the holes in its walls. And others can trace the convoluted, discontinuous linkages between what grows inside the castle walls and what grows outside. Some objects or concepts become concrete inside the castle and extend outward, as what Latour calls "immutable and combinable mobiles" (1987, 227). These may be written documents like maps, objects like a telephone, techniques like vaccination, or concepts like "the germ." Once at large, these entities may be used in ways that know no limit. Scientists may use them to enhance their own social position, as Latour has demonstrated in the case of Pasteur's use of vaccination.

But equally, mobiles may fail to have any effect, fail to be taken up, or adequately taken up, or taken up beyond the intentions of their inventors. Any of these processes is likely to involve power relations, large or small scale. Any of them is also likely to entail a lively engagement on the part of people outside science. Perhaps engagement is especially likely when scientific developments have direct impact on the body, the material form of a person, in the Euro-American view. Consider, for example, the case of vaccines for children in the United States, a case that has caused an outcry because the number of children receiving vaccines by the age of two is so low. It is generally assumed that low vaccination rates correlate with lack of education, lack of economic resources, and lack of political weight. Although such factors undoubtedly play an important role, we can gain a more complex understanding of this phenomenon by looking deeply into the language, metaphor, and imagery that are used in the United States to build up ways of thinking about the body. Such ways of thinking about the body inevitably make use of social distinctions based on power differences—race, class, gender, ethnicity, and so on. For example, in medical and popular language, the body's immune system is said to be like an "educational system" in which immune system cells are trained. The thymus (an organ that plays a crucial role in the immune system) is metaphorically compared to a "school" in which immune cells (T cells) "learn" how to distinguish good cells in the body that they should leave alone from harmful ones that they should destroy. Vaccinations come into play after T cells have "graduated" from the thymus.

Vaccinations are, in this imagery, a kind of "crash course" for the immune system, a kind of "public education" for the immune system that is often required by the state. Vaccines teach our immune systems to respond to disease organisms that threaten our collective health, such as small pox, diphtheria, or polio (Martin 1994).

There is a puzzle here. Given that educating the immune system through vaccination seems like an obviously good thing to do, why don't more people do it? Press reports frequently decry the embarrassingly low rates of vaccination received by American children. According to the California Department of Health, only 34.5 percent of children in that state have their vaccinations up-to-date at twenty-four months. As I mentioned, the reason for such low rates is usually taken to be lack of resources and knowledge in poorly educated and otherwise disadvantaged portions of the population. This would seem to imply that those who are poorly educated in general will have poorly educated immune systems, because they will not know enough or have the money to obtain the benefits of the "crash courses" provided by vaccination. But this reason can at best only explain part of the puzzle: strikingly low rates of vaccination (and vigorous antivaccination movements) can be found also in highly educated yuppie bastions such as Santa Cruz, California, or Santa Fe, New Mexico.

This is where an understanding of the imagery, language, and metaphor operating in our contemporary culture of the body can enlighten us. People who decide that they want to avoid vaccination, the system of "public education" for their immune systems offered by the state, may be intending to develop their own high-quality "private schooling" instead. Such people may have been engaging in a lifetime of preparation, training, and nurturing of their (and their children's) immune systems through diet, exercise, avoiding stress, and cultivating healthy practices. They may quite reasonably believe that they and their immune systems are already able to handle any germs that may come their way, flexibly changing and adapting, rapidly responding as needed to a continuously changing environment. In such a view, a vaccine, crudely bludgeoning the delicate adjustment of the finely tuned immune system at a time when there is no actual threat, could easily be seen as undermining health.

This example is a small part of developments in the culture of health in the United States by which the very bodies of people are being categorized into two types: those that can survive the present intensely competitive environment and those that cannot. In domains as diverse as business training programs and biology classrooms, people with superior immune systems, superbly trained and continuously reeducated to respond flexibly to any new circumstances in the environment, are set apart from those with slow, rigid, and inflexible immune systems. This new politicization of the body sometimes runs along the lines of familiar kinds of discrimination, based on class, race, gender, or sexuality: the immune systems of women and gay men, for example, are often found wanting, as when attention is called to the high percentage of women among those afflicted by autoimmune disorders or the high numbers of gay men initially afflicted by AIDS. By the terms of the new ideal flexible body, certain categories of people (women, gays) may yet again be found wanting. What is at stake is which of us is thought fit to be part of an emerging stratum of elite worker-citizens, who, with their agile, flexible, creative "superimmune" systems, bodies, and minds, can take their places in the new empowered, creative, corporate workforce.

In cases like these, I would argue, the scientist and the layperson must be seen as coparticipants in the processes by which "mobiles" do or do not become part of our lives. As Gary Downey, Joe Dumit, and Sharon Traweek put it in their definition of the field of cyborg anthropology, "Cyborg anthropology is interested in how people construct discourse about science and technology in order to make these meaningful in their lives. Thus, cyborg anthropology helps us to realize that we are all scientists" (1997, 23). To return to the image of the rhizome, an ethnographer inquiring into the "ramified surface extensions" of the immune system would be as likely to trace connections between propensities or disinclinations in the "public" and what is thought a desirable project in science, as to trace connections in the other direction.

For example, I found that most biological researchers still operate with a mechanistic view of the body; the body is divided up into compartments, arranged hierarchically under the head of ruling organs like the brain, clearly separated from the outside environment. Cause and effect are linear. In contrast, most nonscientists are operating with a very different notion of how the body works and how it relates to its environment. Often, the body is seen as a complexly interacting system embedded in other complex systems, all in constant change. No one part is ever always in charge. Change is nonlinear in the sense that small initial perturbations can lead to massive end results. One woman, whose pseudonym is Vera Michaels, rejected the image of the immune system on the cover of *Time* (a boxing match between the vicious virus and the T cell), because, she said, "it depicts such violence going on in our bodies." She insisted that such violence is "not in there." She claimed her own representation would be "less dramatic":

> My visualization would be much more like a piece of almost tides or something . . . the forces, you know, the ebbs and flows.
>
> [Asked if she could draw anything like that, she replied,]

> I could. I don't think anybody would perceive it as a portrayal of the battle within.
>
> [What is it that ebbs and flows?]
>
> The two forces, I mean, the forces . . . imbalance and balance.

As she spoke, she drew an elegant outline of ocean swells, labeling it "the waves" and capturing her picture of the body in turbulent, constant change.

Often people interviewed in my study talk at great length about the impossibility of separating such an ever-changing body from its environment; health is affected by diet, water, air, mood, stress, relationships, the past, colors, work, and so on. Often, people turn to alternative medicine—acupuncture, homeopathy, chiropracty, herbs, natural foods—to address these concerns.

A recent study published in *The New England Journal of Medicine* showed the extent to which Americans use alternative therapies. Visits to alternative providers exceeded the number of visits to all U.S. primary care physicians in 1990:

> Extrapolation to the U.S. population suggests that in 1990 Americans made an estimated 425 million visits to providers of unconventional therapy. This number exceeds the number of visits to all U.S. primary care physicians (388 million). Expenditures associated with use of unconventional therapy in 1990 amounted to approximately $13.7 billion, three quarters of which ($10.3 billion) was paid out of pocket. This figure is comparable to the $12.8 billion spent out of pocket annually for all hospitalizations in the United States. (Eisenberg et al. 1993, 246)

Here, we can see the resurgence from among the general population of a complex of practices that was dealt a staggering—but it turns out not a lethal—blow by organized scientific medicine earlier in the century.

Moving like a rhizome back inside the citadel of science, there is a group of scientists who are claiming that the body is made up of complex nonlinear systems inseparable from their environment. They argue that the immune system is a self-organizing network, a complex system of the sort Vera Michaels meant to evoke by drawing turbulent waves (Varela and Coutinho 1991). But today, these scientists are considered unconventional and their views controversial. If this currently controversial view were eventually to prevail within biological science (and there are many signs that it will), surely we would want to incorporate in any account of that development how such a view of health and the body had first already been at large in the general population. Developments in science would be participating in broader cultural developments, not simply reflecting them, but not leading them necessarily either. They might be participating in the proliferation of one part of a rhizome in another place.

String Figures

As Max Black (1962) said long ago, metaphors both enlighten and blind at the same time. I would not want anyone to take away the impression that I am looking for an actual thing out there in the world that is the equivalent of the rhizome of knowledge. We are not looking for a thing; we are seeking to understand processes by which things, persons, concepts, and events become invested with meaning. Perhaps in the end the best metaphor of all—my third and last—is one that consists almost only of process: Haraway's evocation of string figures used in the game of cat's cradle:

> The cat's cradle figures can be passed back and forth on the hands of several players, who add new moves in the building of complex patterns. Cat's cradle invites a sense of collective work, of one person not being able to make all the patterns alone. . . . It is not always possible to repeat interesting patterns, and figuring out what happened to result in intriguing patterns is an embodied analytical skill. (1994, 69-70)

In Haraway's terms, in critical studies of science, we would be playing a kind of cat's cradle, a serious game "about complex, collaborative practices for making and passing on culturally interesting patterns" (p. 70). I would like to adapt the image to characterize the movement between expert scientists and the rest of the world. This space in which science and culture are coconstituted is discontinuous, fractured, convoluted, and in constant change. To traverse such a space, we need an image of process that allows strange bedfellows, odd combinations, and discontinuous junctures. An example of strange bedfellows would be people with Marfan syndrome making geneticists feel hurt. An example of odd combinations and discontinuous junctures would be the general public abandoning the mechanistic, linear view of the body before most of the biological sciences have. The discontinuity in this case is that a conception of health based on the immune system, which only took the form of a scientific specialty (immunology) in the sciences in the 1970s, has already reached general currency "on the street" before it has spread to other, closely related sciences like molecular biology or genetic engineering. One of the scientists in my fieldwork expressed his sense of the oddness of a closer affinity between immunology and popular views of the body than between immunology and other biological sciences this way: "hard-core molecular biologists can't stand the soft and evocative way immunologists talk about systems."

There is another use for string figures: to help us think about what kind of activities we count as "science." Notice first that we call string figures a "game." But what kind of a game is it? It has some rules, but they are few and flexible. Any number of people can play, for any length of time. The string can be made from any material. Old figures can be repeated or new ones invented. It can be played competitively (who will mess up first) or cooperatively (how long can we keep the figures moving). It can be played in any location, by anyone invited to join. How could such a loose, casual game with so few rules work as a model for science? Interestingly, one of the forefathers of science studies, Ludwik Fleck, used analogies between the thought collective of science and games, in which he stressed the aspects of games that are not governed by formal rules. For example,

> If the individual may be compared to a soccer player and the thought collective to the soccer team trained for cooperation, then cognition would be the progress of the game. Can an adequate report of this progress be made by examining the individual kicks one by one? The whole game would lose its meaning completely. (Fleck 1979, 46)

His emphasis is on the manner of play (cooperation among team members) and the history of the moves in the game in the context of the activity as a whole. Nowhere does he lay stress on the formal rules of the game. If he had, given the extent and precision of the rules of soccer, the metaphor would have yielded a very different impression of how science is "played" and of how readily contradictory elements can be layered in it.

In contrast, consider the reference to rules of games made by a theorist influenced by Fleck, Thomas Kuhn. In Kuhn, the rules of the game are central:

> [There are] fundamental novelties of fact and theory. . . . [When they are] produced inadvertently by a game played under one set of rules, their assimilation requires the elaboration of another set. After they have become parts of science, the enterprise, at least of those specialists in whose particular field the novelties lie, is never quite the same again. (1970, 52)

For Kuhn, change in science proceeds by changing from one set of rules to another, as if we were playing soccer and then began to play another incommensurable game, lacrosse. These games, unlike string figures, have an elaborate set of constitutive rules, rules that govern how many players must be on a team, what space the game may be played in, what winning by definition consists in, what materials the balls and rackets may be made of, how long the game is played, and much more besides. In sum, what kind of game science is said to be like (string figures with only a few open-ended rules on one hand or soccer with many exact rules on the other) and how much importance is given to the *rules* of the activity (with Fleck largely ignoring them and Kuhn making them central) are both crucial to what we include in our accounts of scientific activity.[4]

It is the openness and casualness of the activity of making string figures—its tolerance of the exigencies of events as they happen—that lets more cultural stuff into a model for science in society. It is the closedness and formality of the rules of formal games—the extent to which the game is governed and defined by the rules—that as a model for science in society tends to keep cultural stuff out. Perhaps the most useful model for science is not any one game but the whole loosely related family of games. If you will grant me the license to make a few substitutions in Ludwig Wittgenstein's paragraph 66 of *Philosophical Investigations*,

> Consider the proceedings that we call "natural science." I mean biology, physics, mechanics, geology, ecology, astronomy, archaeology, mathematics, botany, primatology and so on. What is common to them all? Don't say: "There must be something common or they would not be called 'natural sciences' "—but look and see whether there is anything common to all. —For if you look at them you will not see something that is common to all, but similarities, relationships, and a whole series of them at that. To repeat: don't think, but look! Look for example at biology with the multifarious relationships among its parts. Now pass to theoretical physics; here you find many correspondences with the first group, but many common features drop out, and others appear. When we pass next to ecology, much that is common is retained, but much is lost—are they all experimental? All based in laboratories? Or is there always computation or observation? Think of specimen collecting, whether insects or seashells or micrographs. In molecular biology there are experiments in laboratories, but when a botanist collects plant specimens or a primatologist discovers a new species, this feature has disappeared. Look at the parts played by craft and technique; and at the difference between craft in biology and in theoretical physics. The result of this examination is: we see a complicated network of similarities overlapping and crisscrossing; sometimes overall similarities, sometimes similarities of detail. (adapted from Wittgenstein1968, 31-32)

The existence of different cultures in science has been extensively addressed within science studies. My point in raising the issue here is different: I would argue that the complicated network of overlapping and crisscrossing characteristics in different sciences, many of which also occur in a multitude of activities outside of science, allows us to see the openness our concept of the natural sciences actually has. It is in part this openness that allows us to see that scientific activity widely interdigitates with the rest of culture. To repeat: whenever we are tempted to say *something* must set all the sciences apart from life outside science, we might remember Wittgenstein's admonition: "don't think, but look!" To *look*, of course, to an anthropologist means go out and do fieldwork.

A view of science as a loosely bounded conglomeration of practices, some with relatively hard and fast defining rules like the game of soccer and some with flexible and minimal rules like string figures, creates not just variation among scientific practices but openness to the world outside science. In *Leviathan and the Air-Pump*, Steven Shapin and Simon Schaffer (1985) describe the seventeenth-century experimental "form of life" developed by Boyle as bounded by clear defining rules. Those who would enter the newly emerging experimental space had to assent to the legitimacy of these rules. In a review, Ian Hacking takes exception to their game analogy: after being "larded over with Wittgenstein scholarship, [he says] it is now useless" (1991, 240-41). But a metaphor larded over is not necessarily a metaphor without effect. I-n *Leviathan and the Air-Pump* and other writings, the metaphor can have forceful effects. Far from a dead metaphor, it is a sleeping metaphor that needs to be woken up so we can examine the work it is doing. Characterizing science as an activity with rules that define the conditions of play (even granting the nature of the rules can vary from science to science) emphasizes sharp and discontinuous boundaries between science and everything else. The incident from Gabriel Márquez's *One Hundred Years of Solitude* (1970), which Shapin and Schaffer use to begin their book, involves the two adversaries Father Nicanor and José Arcadio Buendía: "On a certain occasion when Father Nicanor brought a checker set to the chestnut tree and invited him to a game, José Arcadio Buendía would not accept, because according to him he could never understand the sense of a contest in which the two adversaries have agreed upon the rules" (Márquez 1970, 86). Leaving the matter here leaves us with the impression that what matters about the natural sciences is the formal, elaborate, imposing nature of their rules, rules that close in and define a form of life. I want to ask: might not José Arcadio Buendía have accepted with alacrity if Father Nicanor had offered him a string to play with instead of checkers?

The ethnographic fieldwork projects I have described are beginning to give us a picture of the natural and medical sciences as complex, in constant, turbulent interaction with many parts of the cultural landscape. There is a formal resemblance between this picture and the picture of the world painted by Vera Michaels, the informant who likened herself in the world to a wave in the ocean. This leads me to a related point. My initial worry was that I would not be able to get outside science in order to talk about it. I found that indeed I could not, but that is because science is not located where we thought it was. Rather than being produced in an isolated, privileged realm and trickling out to inform the rest of us about what is "true," science is made throughout—bubbles up from many places within—historically constituted human culture. Culture is also made throughout—bubbles up from many places within—science. Perhaps this is what Fleck meant when he said that the image from popular culture of the evil bacilli in the shape of little devils flying from the open mouth of a sick person "haunt[s] the scientific specialty to its very depths" (1979, 117). There are other images, Fleck would say, than the devil-destroying imagery that is still to this day used in bacteriology and immunology. For example, we could emphasize that most of the bacteria we live with are commensals, part of our bodily community, and they only cause problems when the community gets out of balance.

That the musings of anthropologists about "how things are" should echo the musings of their informants is no more and no less a product of the same permeation. Anthropology, no less than natural science, is "haunted to its very depths" by ideas that are salient in the cultures in which it lives.

Before I put my figures of speech away, I want them to do one last job. I chose them to enable a picture of culture that can stand up to or at least alongside of ANT's vision of science. In sum, ANT sees science, from physics to sociology, deploying the same set of features to produce its effects: clarity of signs, simplicity of explanation, visibility of inscriptions, continuity and linearity of links in its networks, and so on. One reason what anthropologists mean by culture cannot be reduced to this list of characteristics is that so many of its features work in opposite ways. Like a rhizome, culture is discontinuous. Some links are invisible and disappear from time to time below the surface of what we can know into dreams, memory, or the account books of multinational corporations. Like string figures, culture is nonlinear, alternately complex and simple, convoluted and contradictory. As often as not, its processes celebrate mystery and opacity.

At this point, I have backed into a position I do not like, using culture as a noun, hypostatizing it, making it sound like a thing instead of a complex of processes. So, to give the kind of thing I mean flesh and bones, let me cite two examples.

1. How can we understand the recent exponential growth in the category of autoimmune diseases? This is certainly fueled by activities in science, but it has been avidly taken up by nonscientists. I suggest this is not because they were enrolled or recruited by scientists or anyone else associated with scientists. At least in part, the eagerness with which autoimmunity has been adopted by nonscientists is because at the present time the condition of autoimmunity is culturally gripping. A mixture of the mundane and the cosmic, it is a complex

system turned against itself, the immune system gone insane, treating self as enemy, dying from within. The horror and the fascination of this kind of thing—a drama of life and death—cannot be reduced to passively being recruited to the agendas of science without losing what we were trying to understand in the first place.

2. How can we understand the appearance of a book in the business category called *JobShift*, which is about "dejobbing"—the end of jobs as such in the American workplace and how to successfully make the transition to the post-job era in which each person will convert him- or herself into a self-contained economic entity, a business called "You & Co." that builds up assets, diversifies investments, cultivates partners, and strives for flexible fluidity (Bridges 1994)? This book appeared in the same month as an article in *The Economist* called "The Anorexic Corporation" (1994) about a worrisome development in which delayering and downsizing have become "dangerously habit-forming." It seems some corporations start getting lean and flexible but then become addicted to these processes and cannot stop until they become "serial downsizers," weak, emaciated, and rigid.

The appeal of a fluid, flexible system is emanating from many domains—among them the science of immunology, with its exquisite, specific, and flexible antibodies, and the profession of human resource managers, with its fondness for the metaphor of the immune system as a tool to teach workers how to become flexible and responsive team members. I doubt if "recruitment" by immunologists to their view of the world or "translation" by human resource managers into theirs would be the best description of what is going on here. That would imply too much rational intent, purposeful action, and deliberate control. But setting this issue aside, the horrible fascination of a flexible system becoming so anorexic is surely also coming from a very different direction: the real-life desperation and fear of delayered employees living on the edge of a precipice, frightened, hungry, and growing ever more invisible. Perhaps I can encapsulate my main point by rewriting a famous title of Latour's (1983)—"Give Me a Laboratory and I Will Raise the World." I am trying to say, "Give anthropologists a culture, and we will show how utterly science and its laboratories are entangled in it."

Notes

1. This lecture was intended to give my perspective as a newcomer to the field on what the cultural study of science could be, rather than provide a review of the relevant existing literature. For readers who are interested in exploring further how anthropologists have studied Western science, the following provides a partial list of additional sources, some of which were published after the 1994 4S meetings: Clarke and Fujimura (1992); Downey, Dumit, and Traweek (1997); Dubinskas (1988); Edwards et al. (1993); Escobar (1994); Gray (1995); Gusterson (1996); Hess (1995); Pfaffenberger (1992); Rabinow (1996).

2. Clifford Geertz used this phrase in a lecture about studying Western science as a cultural system. (The lecture was given in 1992 at the Institute for Advanced Study, Princeton, NJ.)

3. We might wonder whether the growth of science such as high-energy physics, in which research is conducted by large collaborating groups with cooperative links to other such groups, would mitigate any tendency toward individual competition (Traweek 1988).

4. Lynch (1992) and Collins (1985) have discussed the non-rule-like character of much scientific work.

References

Abu-Lughod, Lila. 1991. Writing against culture. In *Recapturing anthropology: Working in the present*, edited by Richard G. Fox, 137-62. Santa Fe, NM: School of American Research Press.

Amsterdamska, Olga. 1990. Surely you are joking, Monsieur Latour! *Science, Technology, & Human Values* 15 (4): 495-504.

The anorexic corporation: American companies are realizing that downsizing on its own is not enough to restore them to health [Editorial]. 1994. *The Economist* 332:19-20.

Ashmore, Malcolm. 1989. *The reflexive thesis: Wrighting sociology of scientific knowledge*. Chicago: University of Chicago Press.

Berger, Peter L., and Thomas Luckmann. 1967. *The social construction of reality*. New York: Doubleday.

Birke, Linda. 1986. *Women, feminism, and biology: The feminist challenge*. New York: Methuen.

Black, Max. 1962. *Models and metaphors*. Ithaca, NY: Cornell University Press.

Bridges, William. 1994. *JobShift: How to prosper in a workplace without jobs*. Reading, MA: Addison-Wesley.

Callon, Michel. 1986. Some elements of a sociology of translation: Domestication of the scallops and the fishermen of St. Brieuc Bay. In *Power, action, and belief*, edited by John Law, 196-233. London: Routledge.

Clarke, Adele E., and Joan H. Fujimura, eds. 1992. *The right tools for the right job: At work in twentieth-century life sciences*. Princeton, NJ: Princeton University Press.

Collins, H. M. 1975. The seven sexes: A study in the sociology of a phenomenon, or the replication of experiments in physics. *Sociology* 9:205-24.

———. 1985. *Changing order: Replication and induction in scientific practice*. London: Sage.

Collins, Harry, and Steven Yearley. 1992. Epistemological chicken. In *Science as practice and culture*, edited by Andrew Pickering, 301-26. Chicago: University of Chicago Press.

Deleuze, Gilles. 1993. Rhizome versus trees. In *The Deleuze reader*, edited by Constantin V. Boundas, 27-36. New York: Columbia University Press.

Downey, Gary, Joseph Dumit, and Sharon Traweek. 1997. Cyborg anthropology: A proposed seminar at the School of American Research. Unpublished manuscript.

Dubinskas, Frank, ed. 1988. *Making time: Ethnographic studies of high-technology organization*. Philadelphia, PA: Temple University Press.

Dupré, John. 1993. *The disorder of things: Metaphysical foundations of the disunity of science*. Cambridge, MA: Harvard University Press.

Edwards, Jeanette, Sarah Franklin, Eric Hirsch, Frances Price, and Marilyn Strathern. 1993. *Technologies of procreation: Kinship in the age of assisted conception*. Manchester, UK: Manchester University Press.

Eisenberg, David M., and R. C. Kessler, C. Foster, F. E. Norlock, D. R. Calkins, and T. L. Delbanco. 1993. Unconventional medicine in the United states: Prevalence, costs, and patterns of use. *New England Journal of Medicine* 328 (4): 246-52.

Escobar, Arturo. 1994. Welcome to Cyberia: Notes on the anthropology of cyberculture. *Current Anthropology* 35 (3): 1-30.

Fleck, Ludwik. 1979. *Genesis and development of a scientific fact*. Chicago: University of Chicago Press.

Fujimura, Joan H. 1992. Crafting science: Standardized packages, boundary objects, and "translation." In *Science as practice and culture*, edited by Andrew Pickering, 168-211. Chicago: University of Chicago Press.

Galison, Peter Louis, and David J. Stump. 1996. *The disunity of science: Boundaries, contexts, and power*. Writing Science. Stanford, CA: Stanford University Press.

Gray, Chris Hable. 1995. *The cyborg handbook*. New York: Routledge.

Gusterson, Hugh. 1996. *Nuclear rights: A weapons laboratory at the end of the Cold War*. Berkeley: University of California Press.

Hacking, Ian. 1991. Review of *Leviathan and the Air-Pump*, by Steven Shapin and Simon Schaffer. *British Journal for the History of Science* 24:235-41.

Haraway, Donna. 1989. *Primate visions: Gender, race, and nature in the world of modern science*. New York: Routledge.

———. 1994. A game of cat's cradle: Science studies, feminist theory, cultural studies. *Configurations* 2 (1): 59-71.

Hawking, Stephen. 1988. *A brief history of time: From the big bang to black holes*. New York: Bantam.

Heath, Deborah. 1993. Bodies, antibodies, and partial connections. Paper presented at "Cyborg Anthropology," seminar at the School of American Research, Santa Fe, NM.

Hess, David J. 1995. *Science and technology in a multicultural world: The cultural politics of facts and artifacts*. New York: Columbia University Press.

Jasanoff, Sheila, Gerald E. Markle, James C. Petersen, and Trevor Pinch, eds. 1995. *Handbook of science and technology studies*. Thousand Oaks, CA: Sage.

Keller, Evelyn Fox. 1983. *A feeling for the organism: The life and work of Barbara McClintock*. New York: Freeman.

Knorr Cetina, Karin. 1991. Epistemic cultures: Forms of reason in science. *History of Political Economy* 23:105-22.

———. 1992. The couch, the cathedral, and the laboratory: On the relationship between experiment and laboratory in science. In *Science as practice and culture*, edited by Andrew Pickering, 113-38. Chicago: University of Chicago Press.

Kuhn, Thomas S. 1970. *The structure of scientific revolutions*. Chicago: University of Chicago Press.

Latour, Bruno. 1983. Give me a laboratory and I will raise the world. In *Science Observed: Perspective on the social study of science*, edited by Karen D. Knorr-Cetina and Michael Mulkay, 143-70. London, Beverly Hills, New Delhi: Sage.

———. 1987. *Science in action: How to follow scientists and engineers through society*. Cambridge, MA: Harvard University Press.

———. 1988. The pasteurization of France. Cambridge, MA: Harvard University Press.

———. 1990. Postmodern? No, simply AMODERN! Steps towards an anthropology of science. *Studies in the History and Philosophy of Science* 21 (1): 145-71.

Lynch, Michael. 1992. Extending Wittgenstein: The pivotal move from epistemology to the sociology of science. In *Science as practice and culture*, edited by Andrew Pickering, 215-65. Chicago: University of Chicago.

Márquez, Gabriel García. 1970. *One hundred years of solitude*. New York: Harper & Row.
Martin, Emily. 1994. *Flexible bodies: Tracking immunity in America from the days of polio to the age of AIDS*. Boston, MA: Beacon.
Marx, Karl. 1967. *Capital: A critique of political economy*. New York: International.
Pfaffenberger, Bryan. 1992. The social anthropology of technology. *Annual Review of Anthropology* 21:491-516.
Pickering, Andrew, ed. 1992. *Science as practice and culture*. Chicago: University of Chicago Press.
Rabinow, Paul. 1996. *Making PCR: A story of biotechnology*. Chicago: University of Chicago Press.
Rapp, Rayna. 1988. Chromosomes and communication: The discourse of genetic counseling. *Medical Anthropology Quarterly* 2 (2): 143-57.
Shapin, Steven, and Simon Schaffer. 1985. *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life*. Princeton, NJ: Princeton University Press.
Traweek, Sharon. 1988. *Beamtimes and lifetimes: The world of high energy physics*. Cambridge, MA: Harvard University Press.
———. 1992. Border crossings: Narrative strategies in science studies and among physicists in Tsukuba Science City, Japan. In *Science as practice and culture*, edited by Andrew Pickering, 429-65. Chicago: University of Chicago Press.
———. 1993. An introduction to cultural and social studies of sciences and technologies. *Culture, Medicine, and Psychiatry* 17:3-25.
Uberoi, J.P.S. 1984. *The other mind of Europe*. Delhi: Oxford University Press.
Varela, Francisco J., and Antonio Coutinho. 1991. Immunoknowledge: The immune system as a learning process of somatic individuation. In *Doing science: The reality club*, edited by John Brockman, 158-66. New York: Prentice Hall.
Wittgenstein, Ludwig. 1968. *Philosophical investigations*. Translated by G.E.M. Anscombe. Oxford: Basil Blackwell.
Woolgar, Steve. 1988. *Science: The very idea*. New York: Tavistock.
———. 1989. Review of *Life among the scientists: An anthropological study of an Australian scientific community*, by Max Charlesworth, Lindsay Farrall, Terry Stokes, and David Turnbull. *Current Anthropology* 32:79-81.

Emily Martin is Professor of Anthropology at Princeton University (Princeton, NJ 08544). Her work on ideology and power in Chinese society was published in The Cult of the Dead in a Chinese Village *(Stanford University Press) and* Chinese Ritual and Politics *(Cambridge University Press). Beginning with* The Woman in the Body: A Cultural Analysis of Reproduction *(Beacon Press), she has been working on the anthropology of science in the United States. Her research is described in* Flexible Bodies: Tracking Immunity in America from the Days of Polio to the Age of AIDS *(Beacon Press). Currently, she is doing an ethnographic study of normalization and cultural concepts of the mental.*