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Ethnography of Africa: The Usefulness of the Useless

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The complex epistemological and methodological problems of data-quality control or ethnographer bias in anthropological research as they relate to the use of the native languages and/or the use of native-interpreter informants are critically reexamined. Summarizing the 1939-1940 Mead-Lowie debate, the paper suggests, on the basis of a close review of selected classic ethnographies of Africa, various ways by which the quality of comparative cross-cultural data could be meaningfully improved. [methodology of cross-cultural research, epistemological issues in anthropology, use of native languages in fieldwork, ethnography of Africa, history of anthropology]

TOWARD AN AFRICAN CRITIQUE OF AFRICAN ETHNOGRAPHY

> Hui Tzu said to Chuang Tzu, "Your teachings are of no practical use." Chuang Tzu said, "Only those who already know the value of the useless can be talked to about the useful. This earth we walk upon is of vast extent, yet in order to walk a man uses no more of it than the soles of his two feet will cover. But suppose one cut away the ground round his feet till one reached the Yellow Springs, could his patches of ground still be of any use to him for walking?" Hui Tzu said, "They would be of no use." Chuang Tzu said, "So then the usefulness of the useless is evident." [Fortes 1945:vi.]

SINCE THE EMERGENCE in Euro-America of anthropology as a university discipline in the latter part of the 19th century, the discipline has, in a true scientific, and certainly humanistic, spirit, continued to be characterized by self-conscious efforts to improve its methods of data collection, data analysis, data organization, interpretation, and presentation. That is, it has continued to assess critically its methodological, theoretical, and epistemological foundations.

Especially in the past five years or so, we have seen the appearance of a flood of very vocal and self-critical literature that has succeeded in reopening, albeit under new auspices, the inconclusive debate that Sol Tax once referred to as the "thirty years' war" of 1840-70. This was the war between "Ethnology and Anthropology; a war between those

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who were historians and philosophers on one side and those who were for science, particularly biology (wherever it might lead one), on the other; a war between humanitarians whose science was related to their advocacy of a cause on one side and, on the other, pure scientists who would separate scientific truth from all other human concerns" (Tax 1964:15).

This new body of critical literature seeks among other concerns to evaluate with reference to both comparative cross-cultural and particular case studies the *effects* on ethnographic assumptions, descriptions, comparison, interpretation, and theorizing of the historical connections of anthropology, as basically a European enterprise, to Euro-American colonialism, imperialism, or neocolonialism.

The central arguments here, now all too familiar, revolve around a number of interrelated, age-old questions: (1) Is a value-free social science (i.e., anthropology) possible? (2) What are the limits of objectivity in science? (3) What are or should be the social and moral responsibilities of the white anthropologist as a student of Third World nonwhite peoples, given the fact that, the typical argument goes, anthropology was born and grew out of self-interested Western imperalistic involvement or adventures in non-Western areas?

In the course of this recent "rethinking," "reinventing," "new left or radical critique" of anthropology, serious questions have also been raised about the validity and the practical and theoretical relevance or usefulness of microscopic ethnographic studies, i.e., about traditional ethnographic fieldwork. Critics point to the inherent deficiencies of structural-functional empiricism, with its assumptions of cultural homogeneity, the "tribal" isolate, and tendencies toward equilibrium of the social order; a-, anti-, or nonhistorical biases; normative focus; data-theory tautologies; and, above all, Eurocentric or racist perspectives that have failed to provide a genuine and total critique of colonial society (see particularly the discussion in Asad 1973; Bergmann 1975; Hsu 1973; Hymes 1972; Nash 1975; Owusu 1975, 1976a).

Whatever the real or potential contributions to knowledge of this new reflexive "progressive" critique of anthropology, it is clear that necessary and sufficient weight is yet to be given in current discussions to perhaps the most fundamental problem of sociocultural anthropology, i.e., the problem of data quality control in ethnographic fieldwork. This particular problem is caused in great part by the lack of familiarity with the local vernaculars, which results in serious errors of translation of cultures. Rethinking anthropology should begin with or stress rethinking the role of native languages as it affects the general quality of ethnographic data collection, organization, and presentation. In fact, since the debate in 1939-40 between Mead and Lowie over the question of the use of native languages in ethnographic fieldwork, not surprisingly, no serious and systematic discussion devoted to the subject has appeared.

Traditionally, there have been three related but analytically distinct types of anthropological research—comparative ethnological (theoretical and speculative), applied (practical and policy oriented), and ethnographical.

Whatever the differing aims and particular emphases of the three types of investigations, they are all concerned ultimately with the collection, analysis, interpretation, explanation, and/or application in terms of given theories, methodologies, or philosophies of basic empirical data about non-Western peoples (see Foster 1969, Goodenough 1956, Mair 1975, Naroll 1970b, Owusu 1976b, Rohner 1975b, and Vermeulen and de Ruijter 1975 for some recent discussion of the nature and significance of the distinctions).

Ethnographic fieldwork, perhaps the most interesting and certainly the most challenging and fundamental, has two principal aspects: the survey and intensive participant observation. Ethnographic research conventionally has had as its main objective the descriptive account of native cultures. That is, the provision for primarily a Western

European audience of new and basic or additional and *reliable* information about non-Western—the so-called "primitive," "barbarous," "savage," or "backward" peoples—"the millions whose welfare" according to Fortes "is in the trust of Western civilization" (1953:46) and of whose cultures nothing or little was (is) known to *Europeans*.

Over the years, white anthropologists have effectively and successfully persuaded (seduced? convinced? reassured?) scholars the world over and intelligent laymen alike to believe, at times against their better judgment, that their ethnographies of "primitive" people are trustworthy because they are the result of painstaking, and *intensive* fieldwork, which implies *fluency* in the languages of the peoples studied (see, for instance, Staniland's recent [1975:x]well-meaning but misplaced apologetic in his work on the Dagomba).

Yet a careful reading of the typical "tribal" monograph ingeniously protected by an "ethnographic present" and written in obscure "scientific" and esoteric language demonstrates one thing: it is virtually impossible, particularly for the native anthropologist, to falsify, replicate, or evaluate it *objectively*. For, frequently, it is not clear whether the accounts so brilliantly presented are about native realities at all, or whether they are about informants, about "scientific" models and imaginative speculations, or about the anthropologists themselves and their fantasies.

Whatever the message and intellectual contribution of these ethnographies, they represent a clear measure of the general distorting intellectual impact of the extension of Western politicoeconomic frontiers, of Western "discovery" of the non-Western world, which has since led, unabated, to the systematic and often forcible restructuring and transformation by Europeans of the "new" and "primitive" world in the image of Europe.

The main purpose of this article, then, is to reexamine, with particular reference to selected, highly representative ethnographic (and historical) accounts of Africa, the implications for past, current, and future research of the perennial problem of the use (lack of use, misuse, or abuse) of native languages in fieldwork. The issue of native languages as fieldwork tools was raised in the now almost forgotten or ignored 1939-40 debate, summarized and commented upon below, between Margaret Mead and Robert H. Lowie. There is ample epistemological, substantive, and methodological evidence of the urgent need to reopen this debate. I show, for instance, on the basis of the analysis of representative selected textual references in two classic, very popular, and influential ethnographies—Evans-Pritchard's *The Nuer* (1940), already in its eighth reprinting, and Meyer Fortes' *The Dynamics of Clanship among the Tallensi* (1945), usually cited with *The Nuer*—that a great majority of the very authoritative and over-quoted ethnographies of Africa by distinguished Western scholars produced particularly during the colonial period have been successfully put together without the serious and systematic benefit of the relevant local vernaculars.[1] Most of the ethnographers did not and could not have had an adequate command of the relevant indigenous languages and a ready comprehension of the natives' speech among themselves. The resulting inevitable reliance of ethnographers on semiliterate and literate native interpreter-informants who communicated in various lingua francas or the so-called contact languages, e.g., Pidgin English, Swahili, etc., did not, as we shall soon see, provide sufficient or reliable insurance against working misunderstanding between ethnographers and the people they studied and, inevitably, data quality contamination. I further demonstrate with reference to a recent fairly representative case study of local politics in Ghana (Dunn and Robertson 1974) that the use of literate native interpreter-informants may very well compound the problem of ethnographer bias in field reports and, hence, of intercultural translation. I present also some evidence from cross-cultural survey methodology to show that one critical factor that greatly contributes to systematic errors in ethnographic accounts is the lack of language familiarity or fluency.

Today, when "heavy acculturation" is the rule rather than the exception in African societies, a *prior* ability to speak and understand *several* relevant local vernaculars is essential if the ethnographer is to avoid serious factual errors and misleading theoretical conclusions. Command of several local vernaculars is necessary because of the increasing tendency of Africans to shift from language to language within a single interaction context or social field as a result of the mixing of different speech communities. Unfortunately there is a growing tendency among Africanists (and anthropologists working in other geographic areas as well) to assume rather naïvely, even as they pay lip service to the importance of the use of native languages, that since European languages are now widely used throughout Africa, satisfactory scholarly ethnographies based on fieldwork can be written without mastery of the relevant vernaculars.[2]

According to the School of Oriental and African Studies' linguistic map of Africa there are, and we need to be constantly reminded, some 1,500 living indigenous languages in very active daily use among Africa's 300 million or so inhabitants, in addition, of course, to the major European languages in use, especially by the new elites, throughout the continent. Africa's self-identity is to a significant extent defined in terms of this linguistic reality. It is this cultural reality, which has over the years successfully defied the otherwise aggressive European culture penetration, which makes ethnographic research in Africa a formidable task, even for native scholars. I then put forward what may be a radical solution to the epistemological and methodological dilemmas of the foreign anthropologist who still dominates the study of African societies and cultures. It is my firm belief that the continued *professionalism* in the field of African studies, the field's contribution to the science of society, and the extent to which ethnographic knowledge could be of real service to the host community and government all depend critically on the seriousness and determination with which the problem of data quality control as it relates particularly to linguistic competence is successfully tackled.

FUNCTIONS OF LANGUAGE IN ETHNOGRAPHY: MEAD VS. LOWIE

In a classic assessment of "native languages as fieldwork tools," Mead (1939) observes that "there is much misunderstanding of what is meant by *using the native language*, a phrasing which I prefer to *speaking the native language*. The latter. . . arouses the suspicion of linguistic purists, terrifies students who have not yet tried fieldwork, and puts an *undue* premium on virtuosity at the expense of emphasizing that a language is a tool, not a feather in one's cap" (1939:196; emphasis added).

She goes on to note characteristically that "we may consider the use of the native language in relation to the problems that confront the fieldworker and divide them into the need to *speak* and the need to *understand*, always bearing in mind that the fieldworker is not in the field to *talk* but to *listen*, not there to express complicated ideas of his own that will muddle and distort the natives' accounts. The demands upon him for active linguistic participation are lower than they are in any *normal* period of his life" (1939:196; emphasis added). Mead proceeds to identify the *three* functions of language in the field as (1) the need to ask questions correctly, (2) the need to establish rapport, and (3) the need to give accurate instructions. For Mead, 20 to 30 locutions at most, with allowance for inflection (1939:197) and "one piece of scrupulously accurate habitual formal comment," (1939:199) are usually adequate for the linguistic needs.

The stress is on the command of a modicum amount of *strategic* native utterances. For if the "ethnologist [ethnographer] cannot give quick and accurate instructions to his native servants, informants and assistants . . . he will waste an enormous amount of time and energy doing mechanical tasks which he could have delegated if his tongue had been just a little better schooled" (1939:199). According to Mead, "it is also essential

to know whether the natives can digest *complex* instructions or whether the instructions must be given them piecemeal sometimes permitting them to answer and repeat between each item in a series" (1939:199; emphasis added). If the fieldworker can learn to handle these three situations, Mead concludes, "he will be able to *use* the native language . . . insofar as speaking is concerned," since he naturally wishes to limit himself to the minimum in conversation, for "he is there to observe and listen" (1939:200). For Mead then "using the native language" for active participation and for obtaining "ethnological information" does not mean, as Lowie indicates in his effective and noteworthy rebuttal of Mead's *mutatis mutandis*, "what it means for a would-be authority on any *advanced contemporary civilization,* viz., a *fluent* command of the vernacular, coupled with ready comprehension of the natives' speech among themselves. Such control . . . Dr. Mead vehemently deprecates—almost contemptuously—as 'linguistic virtuosity' " (Lowie 1940:81; emphasis added).[3] Mead concludes on the basis of the above considerations that since the publication of Malinowski's *Argonauts of the Western Pacific* in 1922, which marks a significant revolution in ethnographic fieldwork methods, more than 25 investigators of both sexes from England and the U.S. "have done *authentic* fieldwork *using* native tongues" (1939:191-192; emphasis added).

If it is true that Malinowski's fieldwork methods mark a significant advance in ethnographic field techniques as Mead readily admits (and no serious ethnographer would dispute), it is also the case, as Malinowski himself points out in relation to both survey research (statistical documentation and delineation of the anatomy of culture) and the aspect of fieldwork concerned with the "imponderabilia of actual life and typical behavior" (1961:1-25), that the goal of fieldwork with its emphasis on the use of the native language is "to grasp the *natives'* point of view, his relation to life, to realise *his* vision of *his* world" (1961:25; emphasis in original). This certainly implies fluency in the local vernacular.

The *indispensable* role of the proper *contextual* use of the native language in fieldwork is justified on *scientific,* practical, and humanistic grounds.

DATA QUALITY CONTROL AND NATIVE LANGUAGE FAMILIARITY

One of the most neglected issues in social/cultural anthropology in general and in cross-cultural survey methodology in particular is, according to Rohner (see Rohner et al. 1973:275-276), the complex problem of ethnographer bias or "data quality control"—that is, the problem of systematic errors occurring in the process of ethnographic data collecting and reporting.

Data quality control technique first proposed by Naroll (1962) deals not merely with the general trustworthiness of isolated social/cultural facts but also—and more importantly for hologeistic researchers—with the statistical testing of the reliability of hypotheses about transcultural relationships among two or more social, cultural, psychological, etc., variables based on a worldwide sample of societies from the ethnographic record in anthropology. Data quality control tests warn us, in effect, against uncritical acceptance and use of ethnographic (and historical) sources.

Since ethnographic accounts of African societies (and of other non-Western societies) have traditionally relied, admittedly, so heavily on native informants, control of informant bias is obviously a most urgent task. Three main sources of informant error are described by Naroll. These are (1) the distorting effects of indigenous cultural theory or stereotype, (2) the distorting effect of poor choice of informant by the ethnographer, and (3) the distorting influence of faulty memory of the details of a particular unique event (see 1962:80-82 for details).

Of course, informants may deliberately or unintentionally mislead, lie, or refuse to

answer questions or provide needed information. That is, informants may indulge in various kinds of systematic deception. To find out whether or not any of the above-mentioned and other forms of informant or ethnographer bias (in contradistinction to random error) exist in field reports, Naroll proposes six bias-sensitive control factors or tests for the purpose and applies them to a cross-cultural study of culture stress. The relevant control factors are (1) case reports, (2) participant observation, (3) length of stay in the field, (4) native language familiarity, (5) ethnographer's role (e.g., as scientist, government official, or missionary), and (6) explicitness and generality of report. The "provenience of the ethnographer" may also be a possible control test (see Naroll 1962:85-99 for a detailed discussion).

In this study, based on a final worldwide sample of 37 societies, including seven in Africa (1962:46), Naroll constructs an "index of culture stress" involving four adequately operationalized and transculturally equivalent substantive variables or traits, which are (1) drunken brawling, (2) defiant homicide, (3) protest suicide, and (4) witchcraft attribution. That is, the quality control factors are measured in each sample ethnography and they are then correlated with the substantive variables. If a data quality control factor—we need to stress that each separate quality control test "stands on its own feet" in regard to the observation conditions in the field (1962:22)—is significantly related in a statistical sense to a pair of substantive variables, then the effects of that control factor must be considered in the interpretation of the relationship between the bias-sensitive substantive variables or traits.

Naroll is able to show in his study of culture stress, for example that ethnographers who live in the research community for a year or more ("length of stay in the field") tend significantly more often than "short stayers" to report the presence of witchcraft. If reports on warfare are biased in the same way, with "long stayers" being more likely to report the presence of warfare than "short stayers," then cross-cultural survey researchers may discover a statistically significant but spurious cross-cultural relationship between the incidence of witchcraft and that of warfare. The true relationship between witchcraft and warfare would thus be obscured because of systematic ethnographer bias in the ethnographic reporting process (1962:88-89; see also Rohner 1975 and Rohner et al. 1973 for a recent application of the data quality control technique to a cross-cultural study of the effects of parental acceptance and rejection).

Most pertinent to my argument is Naroll's observation that the quality control tests of witchcraft attribution and of protest suicide reports have produced statistically significant or near significant evidence of bias. According to him, the evidence that turned up suggests, among other things, that reports by ethnographers unfamiliar with the native language may tend consistently to underestimate suicide and witchcraft attribution rates. Naroll's language familiarity test, for instance, shows a high association between native language familiarity and high witchcraft attribution reports (1962:89-90). Naroll (1962), Rohner (1975), and others do recognize that the effects of ethnographer bias shown by the statistical analysis of the ethnographic data could be due to a number of factors, including sampling error and coder bias.[4]

Nonetheless, the commonsense remark by Naroll (1962:90) that ethnographers who stay longer in the field and who master the native language have better—superior—rapport with informants and hence are less likely to be imposed upon or more likely to detect deception when it is tried can hardly be refuted and needs to be restated again and again.

Unfortunately, as I have implied, few ethnographers, if any, working in African societies in the 1920s, 1930s, 1940s, and 1950s—the senior anthropologists whose work laid the foundation for African studies—had any appreciable control of the native languages.[5] The ability to use effectively native African language(s) by the ethnographer

would require, under normal conditions, several years of sojourn among the target and *related* peoples before one could be ready for the serious task of studying the culture.

But in a *colonial situation*, characterized as it was by a diffused sense of the White Man's Burden, serious misconceptions about the nature of traditional African societies and cultures, and Eurocentric intellectual orthodoxies and preconceptions, conditions could hardly have been normal, let alone ideal, as Fortes' and Evans-Pritchard's remarks, to be discussed later, attest. The ethnographer was, therefore, forced almost invariably to rely heavily on the overburdened native-servant-interpreter-informant. As Lowie concludes with characteristic candor, "We use interpreters, not because we like to, but because we have no other choice" (1940:89).

The basic epistemological issue is whether a true dialogue can be obtained between the foreign ethnographer and his native interpreter-informant, which will provide a basis for real understanding of the native's culture and society and for removing any serious mutual historical misconceptions that may hinder genuine communication where one or both parties have little or no effective control of the other's vernacular (in the phonetic, lexical, and idiomatic senses).

A related question is what constitutes *acceptable* anthropological paradigms and ethnographic findings, given the fact that the anthropology of Africa is still largely a European enterprise, dominated by European scholars who define what anthropology is? Further, given that Europeans provide the rules for "scientific" or legitimate anthropological work and also provide the criteria by which academic recognition and rewards are allocated among deserving anthropologists, both African and European, what real contribution can a native literate or semiliterate interpreter-informant make toward genuine understanding of his society and culture? Answers to these questions may be found in part by a careful analysis of aspects of the conventional role of the literate native interpreter-informant in ethnographic fieldwork and the procedural rules by which anthropologists arrive at their data and the nature of the conclusions based on them.

THE ETHNOGRAPHER'S MAGIC: THE DISCOVERY OF "STRUCTURES"

There are basically three interrelated stages and processes by which data on African cultural realities have been and continue to be systematically gathered and their substance transformed and often mistranslated by Western ethnographers into the so-called valid cross-cultural, universally applicable institutional types. These stages are as follows: (1) initial, and often persistent, linguistic and psychological (cultural and racially defined) gaps between the foreign ethnographers and the peoples they study; (2) the urgent demand for "theories" to assist the ethnographer in organizing his field data and in presenting the conclusions derived from the data; and (3) the uncritical treatment of "authoritative" ethnographic or ethnological hypotheses and hunches as accepted or established facts of native life.

First, faced with the cognitive and linguistic gap between himself and the subjects, or natives, the ethnographer is forced to apply rigidly the rather convenient rule of "scientific detachment" in fieldwork and to aim primarily, even solely, at providing sociologically intelligible accounts of the beliefs and practices of native populations. As Evans-Pritchard clearly indicates, sociological intelligibility means that

> the social anthropologist *discovers* in a native society what no native can explain to him and what no layman, however conversant with the culture, can perceive—its basic structure. This structure cannot be seen. It is a set of abstractions, each of which, though derived, it is true, from analysis of observed behavior, is fundamentally an *imaginative construct of the anthropologist himself.* By relating these abstractions to one another logically so that they present a

> pattern he can see the society in its essentials and as a single whole . . . . Having isolated these patterns in one society he compares them with patterns in other societies [1968:51; emphasis added].

There are thorny problems concerning how "structures" as abstractions from reality are generated; the extent of the logical validity of the abstractions; and, more critically, the degree of their correspondence with native realities. The major "discoveries" of the Western ethnographer (and historian) can and have been made with little concern for the integrity of the cultural realities of the individuals and groups of the societies in question. For example, Chukwuemeka Onwubu, in a review essay, has recently demonstrated, through semantic analysis of Igbo terms, the error committed by Simon Ottenberg in his *Leadership and Authority in an African Society: The Afikpo Village-Group* when he presents as the structural attributes of Igbo society such taxonomic categories as subsets, grades, wards, village segments, subsegments, and clans, thus creating the impression of a formally organized village bureaucracy (Onwubu 1975:71-77).[6]

The ethnographer's magic wand, his most *personal* and prized property, seems then capable of conjuring up a fantastic array of truly head-spinning hierarchies of "structures" and reticulated "structures" of "structures," according, no doubt, to a preconceived, well rehearsed and orchestrated, little understood philosophical plan. In the main, these hierarchies have little correspondence with local realities. This ethnographic shadowboxing continues to make open transcultural scientific, even humanistic, discourse difficult and truly cumulative progress impossible.

In fact, Lévi-Strauss, renowned for the brilliance of his imaginary excursions and the tremendous power and attraction of his divining rod capable of "discovering" universally valid primordial "structures" in the most unsuspected areas, admits that "the best ethnographic study will never make the reader a native" (1968:16). In a fundamental sense then, ethnography (of Africa and elsewhere) is not really about native societies in the way that sociology is said to be about Western societies. Whatever the imperfections, sociology is about Western man and woman, analyzing and writing in their native languages about themselves in a self-conscious way.[7]

Commenting on the relationship between structural abstractions and the realities to which they pertain, Firth similarly reminds us that it does not really matter so much if the anthropologist gets his facts wrong as long as he can argue his theories logically (1954:vii).

The second stage in the process by which African cultural realities are often mistransformed through mistranslation by ethnographers is associated with the urgent demand for "theories" to assist the ethnographer in organizing his field data and in presenting the conclusions derived from the data. As Fortes indicates with regard to his African data:

> It is not merely a question of putting his [the ethnographer's] observations on record. Writing an anthropological monograph is itself an instrument of research and perhaps the most significant instrument of research in the anthropologist's armory. It involves breaking up the vivid kaleidoscopic reality of human action, thought, and emotion which lives in the anthropologist's notebooks and memory, and creating out of the pieces a coherent representation of a society, in terms of the general principles of organization and motivation that regulate it. It is a task that cannot be done without the help of theory [1945:vii].

Alas, the "theory" or "theories" generally turn out, on closer inspection, naturally to be well established, fairly orthodox Western views of society and culture, their origins and development, based on European academic and popular philosophical thought and experience, which are then applied to the whole of humanity. The power of Western

science and technology and the related ability of the West to establish and maintain its political and economic domination and intellectual leadership particularly of the non-Western world have successfully turned dominant Eurocentric theories of history, culture, and society into "cosmos-centric" systems, i.e., universal systems of thought and belief (despite the popularity of the current distinctions between the so-called emic-etic approaches to ethnographic fieldwork).

The negative intellectual effects of various aspects of this type of deep-rooted Western "prejudice" or "psychocultural bondage" on anthropological studies, for example of witchcraft and caste, have been effectively argued by Hsu (1973:6-9). As Hsu points out, the major weakness in American anthropology "is found in its general theories on the determinants of human social and cultural behavior. This major weakness is in my [his] view directly attributable to the failure of white American anthropologists to consider views other than those to which their cultural conditioning has led them" (1973:9). Hsu concludes rightly that truly universally applicable theories of man can hardly emerge unless Western anthropologists break out of their near obscurantist "mental bondage" (1973:16) and recognize and accept the significance and validity of competing non-Western assumptions and theories and contary viewpoints about man and culture not in conformity with conventional Western orthodoxy.[8]

The rather distressing difficulty for the native ethnographer brought up in the European intellectual traditions is clearly how to overcome his own Eurocentric biases; penetrate the granitic Eurocentric structural crust; and get to the deeper, graphitic, turbulent substantive layers of African cultures and societies.[9]

Third, the process of cultural mistranslation in African ethnography reaches its apogee when "authoritative" ethnographic or ethnological hypotheses and hunches are treated uncritically as accepted or established facts of native life, i.e., when, as Wagley puts it, "classificatory types, formulated in the first place for their heuristic value . . . [are] translated into developmental stages, conceived as having real existence and arranged in a hierarchy which is both chronological and qualitative" (1971:121).

Thus Sahlins, ignoring Evans-Pritchard's own caution regarding the tentative nature of the Nuer data, attempts a reanalysis of the Nuer material from an evolutionary perspective. On the basis of precategory assumptions and taking the historical validity of the Tiv-Nuer data for granted, Sahlins argues rather speciously that the Tiv-Nuer segmentary lineage organization "is a specific adaptive variety within the tribal [pre-*chiefdom,* post*band*] level of society and culture" (1967:89). He believes, in a historicist vein, that without Anglo-Egyptian intervention, the Nuer would have in time overthrown the segmentary lineage system and "catapulated themselves to the chiefdom level of evolutionary progress" (1967:119). Sahlins further contends without any precise specification that his reformulation of the Nuer social structure "leads to certain empirically testable conclusions about its genesis and incidence" (1967:90).

As Ravindra Jain of the Institute of Social Anthropology, Oxford, reminds us with reference to Evans-Pritchard's ethnographic approach, "He could seriously mislead the less imaginative, as he did those anthropologists who took Evans-Pritchard *literally* to mean that the segmentary structure of Nuer society was 'revealed' to him" (1974:3; emphasis added; see also, for example, Terray's [1975] critical review article on Jack Goody's *Technology Tradition and the State in Africa*).

Thus, where there are yawning cognitive and cultural gaps, as is frequently the case, between the ethnographer and the natives under study, there is bound to be hardly avoidable working misunderstanding between the ethnographer and his subjects—no pun intended. The much-quoted abortive dialogue (the language of the original conversation is not clear from the account) between Evans-Pritchard and Cuol, the Nuer, on a subject, as the former puts it, "which admits of some obscurity" (1940:12) and

succeeded in making Evans-Pritchard in the end "Nuerotic," is a telling case in point. The English rendering by Evans-Pritchard of the incident may have glossed over serious semantic problems.

Even where the lucky native has indeed mastered through years of apprenticeship the rules of ethnographic "discovery" and their successful, if often misleading, application in different societies, and where therefore possible grounds for meaningful communication exist between the ethnographer and the native, the fact still remains that the native as often as not discovers that in order to communicate effectively and convincingly as a *professional anthropologist*, he is forced by intellectual community pressure to continue to use, develop, and apply what could be shown to be often highly inappropriate, very anachronistic "scientific" jargon and paradigms developed at a period when ethnographic interests were narrowly defined, that is, when the so-called "anthropological societies" everywhere—Aleut, Tupinamba, Andamanese, as well as the Asante, Zulu, Yoruba, etc.—were all presumed *at the time of study* to be self-contained, static, primitive *tribal* isolates.

THE PROBLEM OF "PARALITERATE FEEDBACK"

In a brief, critical, and provocative survey of social anthropology in Nigeria during the colonial period, Jones (1974:280-289), colonial administrator turned professional anthropologist, comes to some hard, unflattering conclusions that may be of general application. Singling out for rare praise S. F. Nadel's work on the Nupe, Rupert East's translation of *Akiga's Story* on the Tiv, and the essays by Forde on the Yako—one might perhaps add a few others of comparable value and usefulness to his list, e.g., Bradbury's work on the Benin—Jones is nevertheless convinced that by and large the contributions to the general field of anthropological studies, made in the course of rather hectic anthropological activity in Nigeria from the 1850s to the 1950s, have been disappointing.

> Many monographs, reports, and papers have been published by anthropologists, some of them professional, most of them amateur, most of the earlier ones self-taught, most of the later having taken some university courses on the subject. Much of their descriptive ethnography is pretty poor, and their monographs on particular people vary; those written under the influence of anthropological hypotheses in vogue at the time they were written are worse than the others (1974:286).

One may disagree with Jones on points of detail, but his basic conclusions are, I believe, valid. The generally poor quality of early African ethnography was partially due to dominant Eurocentric prejudices, distortions, and errors of fact associated with the exciting but hopeless search for the *real*, raw, exotic native based on a highly discredited conjectural history and the application of current anthropological theories. It is clear from Jones' discussion that the quality of descriptive ethnographies was adversely affected as much by the serious linguistic problems of the anthropologists as by the uncritical use of primary ethnographic data collected by the colonial governments for their own purposes. Jones notes that "the Nigerian Colonial records provide a mass of material not only for historians but also for social anthropologists as soon as they become interested in diachronic studies and social change" (1974:287). But unfortunately, Jones points out that "official government report seems to exercise a mesmerizing effect, not only on many colonial historians, but also on some anthropologists who lose sight of the point that, although its manifest function is to present the facts, its latent function is usually to cover up" (1974:287). Robertson (1975) makes a similar point with regard to colonial archives in Ghana.

However, in connection with the role of the native interpreter-informant in ethnographic fieldwork, we may observe with Jones that undoubtedly the most *alarming*

consequences of colonialism and the initiation and continued control of anthropological activity in Africa by Europeans has been the effect that anthropology (and European writings on Africa generally) has had upon the *Africans* themselves.[10] It is crucial to remember in this connection that one of the subtler and more effective weapons of imperial supremacy was the European language. Subject peoples were obliged to adopt and use it if they wished to succeed in the colonial world. In time, the colonized African was made to believe that anything written in a European language was sacrosanct, infallible, and beyond question. Few natives, however, mastered the foreign language perfectly. As a result, the European (and any native who could master the European language) enjoyed unparalleled psychological (and social) advantages. There are numerous humorous examples throughout Africa of natives painfully and tragically pretending to speak and/or understand English or French in a rather hopeless effort to improve their standing in the eyes of the European. But indigenous African languages survived and even flourished. Jones correctly identifies one of the primary sources of the confusion in the translation of cultures when he notes that

> To the average Ibo villager an anthropologist is someone who knows more about Ibo traditional culture than he does himself. Any monograph written by an anthropologist on a particular tribe and accessible to its *literate* members becomes the tribal Bible, the charter of its traditional history and culture. . .
>
> The oral tradition of many of these. . .communities has completely absorbed, and been corrupted by, the myths of the anthropologist. The Wheel has come full circle and, to paraphrase C. L. Temple's remark, "a knowledge of the beliefs and practices of the European anthropologist is now of first importance to the native" [1974:287].

Similarly, on the "authoritative" writings on the Tiv by the Bohannans, Dorward (1974) makes the following assessment.

> They too were armed with conceptual models, the most influential being that of the *segmentary lineage system* which they refined and gave substance. Like their predecessors, they too were to *create an image* of the Tiv, far more influential for being scientific. Theirs was the *"reality"* through which academics and outsiders have since come to perceive the Tiv; one might say the Nuer and the Tiv have segmentary lineage systems, thus segmentary lineage systems *exist because* we have the Tiv and the Nuer (1974:474-475; emphasis added).

Dorward also notes that "with the spread of literacy among the Tiv, the influence of the written work, and the availability of latter-day publications on the Tiv (by Europeans), the extent of 'feedback' has been considerable" (1974:475; see also the warnings of Owusu [1975] and Vansina [1974] against "paraliterate feedback").

NATIVE SCHOLARS AND ETHNOGRAPHY

In his *American Kinship: A Cultural Account* (1968), David Schneider presents clearly and cogently the scientific and practical arguments in favor of the central role of the native scholar in ethnographic fieldwork. His remarks merit full quotation:

> There is another reason why the study of kinship in America is especially important to Americans and that is that as Americans, this is a society and a culture which we know well. We *speak* the language *fluently,* we know the customs, and we have observed the natives in their daily lives. Indeed, we *are* [emphasis in original] the natives. Hence we are in an especially good position to keep the facts and the theory in their most *productive* relationships. *We can monitor the interplay between fact and theory where American kinship is concerned in ways that are simply impossible in the ordinary course of anthropological work.* When we read about kinship in some society *foreign* to our own we have only the facts which the author chooses to present to us, and we usually have no independent source of knowledge against which we can check his facts. It is thus very hard to evaluate his theory for ordering those facts.

> By the same token of course *we are able to achieve a degree of control over a large body of data which many anthropological fieldworkers hardly approach, even after one or two years in the field. Hence the quality of the data we control is considerably greater, and the grounds for evaluating the fit between fact and theory is correspondingly greater* [vi; emphasis added].

The point that needs special emphasis is that African scholars who have given serious thought to the quality of the huge masses of data on African societies and cultures written mainly by foreign anthropologists and other experts have independently long come to similar conclusions, though not all have always succeeded in articulating the theoretical and substantive issues involved as clearly and effectively as has Schneider (see for instance, Owusu 1970, 1975; Kenyatta 1962).

FORTES, EVANS-PRITCHARD AND COMPANY AND DATA QUALITY CONTROL

In the light of the foregoing epistemological and methodological problems raised by the comments of Schneider and others concerning ethnographer bias, let us return to the epistemological basis of the theoretical and empirical contributions to African ethnography of two most distinguished and influential founding fathers, Evans-Pritchard and Fortes.

The particular focus on the two classic ethnographies perhaps requires some further explanation: both works clearly exemplify the built-in epistemological dilemmas generally characteristic of structural-functional anthropology; they illustrate graphically the serious confusion of time levels, time scale, and perspectives associated with the ethnographer's standard use of the "ethnographic present" and the resultant structural and empirical distortions and oversimplifications of the cultural-historical processes usually found in ethnographic accounts of African societies. Thus, it is difficult, for example, to reconcile Evans-Pritchard's description of the Nuer as "an acephalous kinship state" lacking generally organized political life and legal institutions, and as a society that had, until 1928, generally remained intact (1940:271), with his very brief discussion that from about 1821, the Nuer continued to resist the Arab, British, and Egyptian intervention and invasions; that in 1920, despite large-scale military operations, including bombing and machine gunning of Nuer camps causing much loss of life and destruction of property, the Nuer remained unsubdued (1940:132-135); and that the 1928-30 prolonged military operations conducted against Nuerland truly marked the end of serious fighting between the Nuer and the Anglo-Egyptian government. A more systematic and empirically valid description of Nuer society and polity (a) before 1821, (b) after 1821 and before 1930, and (c) after 1930 under the new administration of Anglo-Egyptian Sudan is urgently needed. John Tosh, writing recently about the Nilotic Langi of Uganda described by anthropologists along with the Nuer, etc., as "segmentary," "stateless," "amorphous," or "acephalous" societies, notes after analysis of oral traditions and documentary evidence that Lango society was far from static during the precolonial period. A comparison between 1870 and 1900, for example, "would reveal that significant structural change had occurred in that time" (1973:475). In fact, Evans-Pritchard (1949), working under more favorable research conditions, describes a similar process of structural transformation for the Bedouin society of Cyrenaica, between 1837 and 1902.

Again, *The Nuer* and Fortes' *Dynamics of Clanship among the Tallensi* (1945) show clearly the inherent essential anachronism of the ethnographic enterprise—ethnographic techniques, research orientations, "theories," terms of reference, and descriptions in the colonial age. As Margery Perham candidly notes, "while with one hand [colonial] government was trying to *preserve and control* tribal society with the other it was opening Africa to economic and other forces which were bound to undermine it" (1962:68-69).

In some instances African societies had long been undermined by these forces. It was the rather thankless job of Malinowski and his students to recover rapidly the real nature and characteristics of precolonial "traditional" African societies, a task that forced structural-functional anthropologists to break their own self-imposed taboo against speculative history. In fact, by the end of colonial rule, all indigenous African polities, including the Nuer and the Tallensi, had become for a considerable period of time constituent units of European centralized administrative bureaucracies. The inevitable result of this ethnographic anachronism is that ethnographies ended up being, by and large, neither reliable, thoroughgoing cultural histories or ethnohistories, nor valid, critical, empirical sociology.

Ethnographies neither made serious, systematic, and critical use of the available relevant documents or of oral traditions—in the latter case, no doubt, mainly because of language problems—nor did they consider sociological "theories" of modern imperialism and capitalism applicable, despite a Eurocentric approach, to the conditions of colonized natives.

Both monographs also reveal the urgent necessity for reanalyzing the data, especially on the much misunderstood, so-called "acephalous" societies within a comprehensive historical and sociological framework. For some of these societies may have been, as the Tallensi and Kokomba cases show, constituent units of precolonial chiefdoms or kingdoms, and a critical reexamination of the factors or circumstances that may have led to their subsequent structural dispersal, dissolution, and decay, or to structural destabilization and decentralization, would be interesting and informative. After all, centralized kingdoms in Africa could often tolerate varying degrees of subordinate structural autonomy and wide latitudes of policymaking independence on the part of component units without losing their identity as "states." In the final analysis, however, one could, of course, speculate on what might have been the course of social and political development in Africa if there had never been a colonial era.

Finally, since the so-called acephalous, segmentary, or band societies are frequently cited by Western ethnographers as archetypical examples of "simple," "archaic," neolithic, or "savage" forms of societies, a focus on the Nuer and the Tallensi is meant to force us to reevaluate our simple-minded, ahistorical, if social Darwinist, approach to African societies and cultures.

In a revealing, perhaps forgotten, foreword to his *Dynamics of Clanship among the Tallensi*, Fortes reiterates the nature of the basic difficulties of *European* anthropologists studying African cultures, particularly in the colonial era. But instead of discussing systematically the implications for cross-cultural research of the complex intellectual and substantive issues raised by the usual fieldwork problems of the typical "outsider" ethnographers, e.g., lack of language familiarity, Fortes sidesteps the issues involved by cleverly elevating the difficulties into cardinal and universal principles of value-free scientific anthropology. He accordingly states:

> It is true that he [the anthropologist] can never feel himself completely at one with the people he is studying, however gifted he may be, *linguistically* or *psychologically*. He may make some real friends among his hosts; but he can never adopt their cultural values. If he did, he would lose that *detachment without which anything he wrote would be of no scientific value* [!][11] [1945:vii; emphasis added].

It is worth noting that the thoroughly Akanized *Okomfo* Rattray, whose rich and detailed ethnographic accounts of the Asante (Ashanti) people provided a solid empirical foundation for Fortes' own much later stimulating writings on the Asante, had better thoughts. He notes concerning ethnographic data collection:

> If these "ancients" [the older Asante men and women who can provide valuable ethnographic information] are asked to converse through the medium of an interpreter, who often does not

> know English at all well and is generally quite incapable of rendering into English many of the words used in the vernacular, they usually become reticent and suspicious, or at any rate uninterested, and likely to withhold their stores of knowledge. If, however, they are able to talk freely and without the aid of an interpreter to one who has their confidence, *who they know can sympathize with them and understand not only their language, but their modes of thought and pride of race, then and then only are they likely to pour out their store of ancient lore and to lay bare their thoughts* [1969:7; emphasis added].

Yet Fortes seems to accept Rattray's position, shared by most ethnographers, that the good ethnographer thoroughly masters the native language so that he or she does not need to use an interpreter or a contact language, when he points out that in the course of two and one half years (1934-37) of fieldwork

> as there is no linguistic literature for the Tallensi, we had to learn the dialect from scratch, with the assistance of a semi-literate interpreter and the scanty literature on Mole and Dagbane . . . . By the end of our first *tour* we became proficient enough to dispense with an interpreter. Nevertheless, I know only too well that we reached but a *moderate standard in our vocabulary and in our appreciation of the finer shades of thought and feeling that can be expressed in Talni* [1945:xli; emphasis added].

Apart from linguistic problems, Fortes mentions other "extraneous difficulties," namely economic and political, which affected the quality of his data, for example "the war, bringing with it issues of far greater moment than the study of the social structure of *a remote and unimportant African people* [!]" (1945:viii; emphasis added).

Concerning the central analytical concepts of "segmentation" and "social equilibrium" as applied to the Tallensi (which go back to Durkheim and Evans-Pritchard), Fortes cautions that "their virtue lies not in their *explanatory* but in their *exploratory* value" (1945:xi; emphasis added).[12]

EVANS-PRITCHARD AND THE NUER

Similar self-critical observations and caveats, often not heeded by others, are found in the introductory remarks of Evans-Pritchard's classic, *The Nuer* (1940). We cannot afford to pooh-pooh authors' cautions. For, as Evans-Pritchard himself attests and any one who has carefully read him knows, his account of the political development of the Islamic Order of the Sanusiya among the Bedouin tribes of Cyrenaica is definitely far superior to his discussion of the political and social institutions of the Nuer. The existence of extensive literature in Arabic on Cyrenaica, three years' residence in Egypt, travels in other Arab lands, some knowledge of Arab history and culture, experience of Bedouin, and, most crucial of all, proficiency in spoken Arabic, clearly account in large part for the relatively high substantive and analytic quality of the book (1949), which is explicitly cast in a genuine historical mold. Evans-Pritchard began his research among the Nuer of the Sudan in 1930 under very difficult circumstances, since the Nuer had only recently been subjected to harsh military suppression of a series of revolts against British colonial authority and since the area was physically difficult to reach. As Evans-Pritchard notes, "my total residence among the Nuer was. . . about a year. I do not consider a year adequate time in which to make a sociological study of a people in adverse circumstances, but serious sickness on both the 1935 and 1936 expeditions closed investigations prematurely" (1940:14). He goes on, "Besides physical discomfort at all times, suspicion and obstinate resistance encountered in the early stages of research, *absence of interpreter, lack of adequate grammar and dictionary, and failure to procure the usual informants,* there developed a further difficulty as the inquiry proceeded" (1940:14; emphasis added). All the same, Evans-Pritchard could conclude on an intriguing note that ultimately he knew more about the Nuer than about the Azande, "about

whom I am able to write a much more detailed account" (1940:15). To add to our epistemological confusion, Evans-Pritchard presents the Nuer monograph as "a contribution to the *ethnology* of a particular area rather than as a detailed *sociological* study" (1940:15; emphasis added), remembering that ethnology in Britain, as Malinowski indicates, refers to speculative and comparative theories, as opposed to the "empirical and descriptive results of the science of man" (1961:9fn).

The practical and linguistic problems did not, however, prevent Evans-Pritchard from making a two-and-a-half-month survey of the political institutions of the Anuak of the Southern Sudan in 1935 and a six-week research survey of the Nilotic Luo of Kenya in 1936, in addition to brief tours of other African peoples of the area. It is interesting that in 1936 he had hoped to study the Masai but was discouraged by the Kenya Government on the grounds that the Masai had recently tried to kill their District Commissioner (see Beidelman 1974:2-3).

The point of all these textual excursions is to demonstrate the crucial epistemological role of language understanding and the political environment—e.g., the colonial situation—in determining ethnographic research priorities and in shaping the qualitative content of research results. It is also meant to show the all-too-obvious fact that it takes a great deal of time, energy, and scholarship to penetrate the hermeneutic meaning of African cosmogony and cosmology as well as years of continued interest in a single culture. Discussing the critical problems of anthropological research in Kenya Colony, I. Schapera (who has himself successfully demonstrated in his own studies of South African populations the crucial significance of many years of continuous fieldwork through native languages and has made noteworthy contributions to South African ethnography) observes that "ideally a thorough study of each of those peoples should extend over a period of roughly five to seven years" (1949:18). The fact of the matter is that time has never been on the side of most foreign anthropologists.

FRENETIC SEARCH FOR MEANING: DUNN AND ROBERTSON

The linguistic difficulties that still beset the foreign ethnographic fieldworker remain basically the same as those present in the 1930s and 1940s. They are phonetic, lexical, and idiomatic. If anything, it appears that in many areas of Africa the problems have become rather more complex as a result of rapid modernization and population movements leading inevitably to increased frequency of intercultural and interlinguistic contacts and change. The possession of polylinguistic skills is now a practical necessity for successful ethnographic research in all regions of Africa.

Let me illustrate with reference to a recent case study of local politics in Ghana. Robertson (Dunn and Robertson 1974) attempts an interpretation of Ahafo politics in terms of the *semantic range* and *symbolic manipulation* of the concept of *krom* (i.e., "community" or "town") in Ahafo. For Robertson, Ahafo factional politics is the struggle for "individual and corporate advancement reckoned in terms of the economic and demographic growth of communities" (p. 40). Ahafo political rivalries are all expressions or strategies of "communal aggrandisement" (p. 209).

On the significance of the interpretative role of the concept *krom,* Robertson states,

> In objective terms, *krom* is a large, populous and politically superior settlement such as the capital of a state. Used in the context of a small, politically humble settlement, *krom* has clear connotations of aggrandisement, and its people use the term even if their community is classified objectively as *akura* (village) or even *osese* (camp) [emphasis in original]. . . . In a more objective sense, krom, akura, and osese *are expressive of a continuum of settlement* growth which is of fundamental importance to any interpretation of politics in Ahafo. [1974:17-18; emphasis added]

Robertson's (and Dunn's) self-confessed linguistic incompetence in Twi leads him to avoidable theoretical conclusions and misleading interpretations of the context of Ahafo politics. It is incorrect to limit the *objective* usage of the word *kro (kuro)* to state capitals, or to large populous towns. A state capital is *ahen-kro (kuro)* and a large populous town is *kro-(kuro)kese,* or *akropong,* as opposed to a small town, which is *kro-(kuro-)ketewa,* or *kro-(kuro)wa, ((a) kura).*

Again, a large populous town is not necessarily *politically* superior, although state capitals when strategically placed at geographic and transportation nodes may in fact grow into large urban centers. The seat of the *Omanhene* in Akan areas has preeminence independently of its actual demographic and other socioeconomic characteristics. In Ghana, as elsewhere, the principal factors making for the growth or decline of communities (towns, villages, etc.) are often *extralocal,* e.g., internal migrations, international trade, colonial administrative, and foreign business decisions, over which local populations have little or no say.

Clearly, in the Agona State discussed by Owusu in his *Uses and Abuses of Political Power* (1970), the demographic and socioeconomic preeminence attained by Swedru in the 1940s and 1950s as a result of its position as a node of networks of motorable roads in a cocoa boom area, etc., did not diminish (and could not have) the *political* superiority of either Nyakrom or Nsaba (both within 10 mi. of Swedru), as seat of the *Omanhene.*

In Akan areas, villages are sometimes thought of as satellites of larger towns. Townspeople may and often do commute between town and village and vice versa, whether or not they live permanently in either of them. Wealthy villagers living permanently on their cocoa farm-villages often invest their newly acquired wealth in towns (e.g., Ahafo investments in Kumasi) in the form of store buildings and businesses and *not* in the villages!

There are numerous mistranslations of Twi sentences, phrases, and words throughout Robertson's chapters. For instance, on page 17, *me ko ne krom,* which he translates "I have come home," should read, depending on the orthography, phonetics, etc., either, "I am going to his (or her) hometown" or "I will go to his (or her) hometown." "I have come home" is *Ma ba me krom.* On page 191, *Adwene pa* is not "good character or attitudes," which is *suban pa* or *obra pa,* but "intelligence or thoughtfulness." On page 211, *panini afutuom* is *not* "grievous eldership" but "counsel of eldership." "Grievous eldership" should be *panini awerehom,* or *aniberem.*

One cannot deny the strong sense of local identification found in most Akan and other communities—a sense of Goaso-ness, Mim-ness, Ahafo-ness, Asante-ness, Swedru-ness, etc. Yet Robertson's "communal aggrandisement" thesis, deriving in great part from misconceptualization of *krom,* is contradicted by much of the internal evidence provided by the authors. Surely, individual self-interest, frequently unrelated to communal interests, class, status, party, and other *extra*local considerations, played a tremendous part in the dynamics of Ahafo politics.

THE NEW ETHNOGRAPHY OF AFRICA

If the vaunted aim of the ethnography of Africa is to provide, on the basis of systematic fieldwork done through native languages or native interpreter-informants, careful descriptions and explanations that can be substantiated, interpretations that have insight, generalizations that can be factually supported, and findings that can provide a clear basis for governmental policy in Africa, the record of the results of conscientious European ethnographic explorations and discoveries has been, by and large, truly disappointing. The evidence for their claims is in most cases often unclear, imprecise, or simply lacking.

Throughout this discussion I have pinpointed some of the principal sources of the ethnographic confusion and errors: Eurocentric and social Darwinist conceptions of African societies; colonial policy constraints on ethnographic research; the inherent anachronism of ethnographic data collection; the reliance on a few key, often misguided, native interpreter-informants (see for example, Chilungu's timely discussion [1976]); the "paraliterate" feedback problem; and, above all, the ignorance of European ethnographers of native languages, even as they shouldered the heavy burden of revealing and translating African realities to the Western and Westernized world.

By this observation, we are hardly saying that there can be no validity whatsoever to the Africa of ethnographers, who by their elaborate pioneering intellectual efforts put "traditional" Africa on the map, gave Africa a new "tribal" identity, and African studies enduring, if dubious, European and, hence, world recognition.

Of course, to a growing number of well-informed African writers and critics, anthropology as a study of "primitive" peoples by "civilized" Westerners is or ought to be dead. The reasoning is quite simple: African societies and cultures on balance are no less or more "primitive" than any others. In any case, the "primitive world" as a subject of serious scholarship is spent in two related senses: (1) classical ethnographic techniques and methodology have in their Eurocentric way said all they could possibly say and (2) colonialism or modernization has transformed "the primitive world" itself out of existence. The "tribal" microcosm, if it ever existed, has vanished.

Nevertheless, the monographs and essays on Africa by such eminent and distinguished scholars as Malinowski, Evans-Pritchard, Fortes, Mair, Gluckman, Forde, Kabbery, Turner, Schapera, and the Wilsons among others, some of whom I am proud to say have been my close and respected teachers and good friends, will in the manner of our capricious ancestral spirits, for many, many years to come, continue to daunt and overwhelm us and to provide a tremendous and intriguing fascination and challenge for indigenous African scholars (a number of whom may, from a respectable distance, still take for granted the "factual" correctness of the ethnographic data). Indeed, some African historians and sociologists of precolonial Africa who have yet to find themselves, faced with overwhelming problems of research using oral traditions, are often compelled to take the line of least resistance by falling back, often too uncritically, on the only published data available—the ethnographic data, which, with all their notorious factual errors and other imperfections, are considered useful, if shaky, props in a *terra incognita*.

Yet these African scholars are sharply aware of the unavoidable historical fact that we could not and *should not* expect European and other foreign scholars, given their very different backgrounds, language problems, cognitive orientations, and intellectual and other interests, to continue to be, as was inevitably the case in the colonial era, our trusted or unquestioned guardians of Africa's collective memory.

Ethnographers have bequeathed to Africa a formidable literary colonial legacy. For all such apparently beneficial European legacies, Africa has had to pay a high cultural and cognitive price. Through their inherent distortions, the classic ethnographies will continue to provide an unfailing, sometimes the only, stimulus for African scholars to newer, bolder, better, more realistic, and more reliable ethnographic and historical research in Africa. Herein lies their lasting usefulness.

While paying lip service to the ideal of objectivity and the pursuit of truth, Western ethnographers have often demanded, unbelievably, in the past that critics must judge their famous classics not by their self-imposed and proclaimed canons of science and scholarship but by appeals to *argumentum ad hominen.* Thus Evans-Pritchard is able to say with reference to his Nuer research and data, that "A man must judge his labours by the obstacles he has overcome and the hardships he has endured and by these standards I am not ashamed of the results" (1940:9). We can indeed appreciate the practical

difficulties of Western ethnographers in foreign lands and still hold them responsible for the erroneous or misleading results of their intellectual efforts.

The principal lesson to be learned from all this is that the validity and intrinsic merit of Western ethnographic "theories," research data, accounts, and interpretations of African societies and cultures, however brilliant, prolific, imaginative, and suggestive, cannot be taken for granted and incorporated uncritically into the comparative generalizations on other cultures in the future if social science is to progress. The persuasive character of ethnographic findings, which still dominate the non-Western field of scholarship—itself a function of the world power structure—is based less on their factual correctness than on the well known fact that they are mostly consistent with or have successfully molded or manipulated over the years—because of their "scientific" claims and the prestige of their authors—Western (or even thoroughly Westernized African) public opinion. They cannot, therefore, be substitutes for the well informed, critical, and original insights and real understanding based on native reserach and scholarship.

The simple commonsense truth is that no person, not even a de Toqueville studying African cultures, can understand another whose language he does not speak, read, and understand, and, hence, whose world view he cannot truly share. The position is already very well understood by some Western scholars. As Kenneth Hale, writing recently on the role of native knowledge in anthropological linguistics (Hymes 1972:382-397) observes, "the linguist depends upon native speakers of the language he studies. It is a prevailing fact about anthropological linguistics. . . that the linguist and the native speaker are not the same individual" (1972:384). He continues, "I question whether significant advances beyond the present state of knowledge of the world's languages can be made if important sectors of linguistics continue to be *dominated* by scholars who are not native speakers of the languages they study" (1972:385-386; emphasis added). Of course, Hale is aware that "it would be incorrect to assert that a linguist is absolutely incapable of making important observations about. . . language not his own or that such observations are of limited scientific interest. . . . Nevertheless. . . even where insights of great importance have been contributed by non-native speakers to the study of English, for example, it is possible to argue that the insights are based on intuitions which, in all essential respects closely approximate those of a native speaker" (1972:386). Hale's arguments apply with equal force to foreign ethnographic research in Africa.

To drive home the obvious point, one may very well ask how many Euro-Americans know our language beyond the usual literal dictionary translations that inevitably make a caricature of native terms and idioms and confuse local meanings and expressions? I have not met one yet, certainly not among our esteemed ethnographic "experts" and critics. And what is even more disturbing about their general attitude is that they continue to produce "authoritative" monographs and essays on African cultures without seriously worrying about the degrading effects of their language deficiencies on the quality of the data. Publishing editors often cannot ensure or do not care whether the native terms are even spelled correctly.

This type of unethical intellectual arrogance, cocksureness, or nonchalance characteristic of Western social scientists studying African societies and cultures—their insulting insistence that one could still be an African "expert" without the need to master any indigenous language—has recently led one anthropologist reviewing Godfrey Muriuki's history of the Kikuyu, 1500-1900, to remark rather frivolously that the book, otherwise an excellent one, is "*marred* by an extraordinary profusion of place names . . . countless native terms which are not always explained. This makes the early chapters particularly hard going" (Dahlberg 1975:84; emphasis added). Although June Nash does not mention the problem of language, I would suggest that this is at the root of the so-called crisis in contemporary fieldwork so well adumbrated and sensitively discussed by her (1975).

This crisis will be perpetuated as long as we keep sending into the field graduate students with little or no linguistic training.

What emerges logically out of all this is simply that an authentic, reliable ethnography of Africa (the new sociology of Africa), which will provide material for the comparative study of other cultures, will have to satisfy at least three specific requirements. The first requirement is the mastery of the relevant African languages by Western ethnographers and other foreign social scientists doing research in Africa. Because of the relatively large size of research funding available to them and the Western world's powers of mass persuasion, these scholars and their views dominate African studies. It seems very unlikely that this particular condition will be met in the foreseeable future. Yet the very quality of ethnographic data from informants is greatly improved when the researcher speaks the relevant native language.

The second requirement is the readiness and commitment of native scholars—the Chilungus, the Uchendus, the Onoges, the Otites, the Magubanes, etc.—already aware of the dangers of uncritical adoption and application of Western social and culture theory to African conditions to do the necessary and basic research, which requires hard work and systematic effort, to take control of our literary and intellectual criticism. This condition is most likely to be satisfied but not in the short run.

The third requirement is a new frank and informed critical intellectual dialogue between the foreign Africanists and native Africanists, the former realizing that they can no longer hope for the role of unchallenged interpreters and translators of African cultures that they not too long ago took so much for granted.

African scholars today are seriously committed to emulate, as Chinua Achebe puts it, "those men of Benin, ready to guide the curious visitor to the gallery of their art, willing to listen with politeness even to his hasty opinions, but careful, most careful, to concede nothing to him that might appear to undermine their own position within their heritage or compromise the integrity of their indigenous perception" (1975:28).

NOTES

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[1]Despite conventional claims to the contrary, as George M. Foster reminds us, "anthropologists take it for granted that they must speak and understand the language of the people they study if their research results are to meet the exacting canons of excellence of contemporary fieldwork" (1969:66).

[2]One anonymous referee of this paper remarked that since, as I have argued, anthropology is a Western science, "control of a native language, however good, is not likely to produce native intuition." This may be so, but certainly language familiarity serves or should serve as a useful, indeed invaluable, check on both informants and interpreters and help improve the general quality or reliability of ethnographic data collection and description. I must strongly emphasize that my basic contention concerning the control and use of native languages in ethnographic research is *not*, as another anonymous referee suggested, a polemic about the "let-us-study-us" approach. Nor am I arguing in support of an extreme relativist position that asserts that since peoples in different cultures have often radically different ways of thinking and looking at life—philosophies of life which are expressed or embodied in their various languages—cultures (and

languages) are untranslatable. This would render anthropology as the study of *other* cultures impossible.

Rather, I am arguing as many others have done before me—though their advice is yet to be seriously heeded—that *in practice*, translation of cultures is extremely difficult, and that even the *possibility* of a tolerably satisfactory translation requires that we have better than a tolerably satisfactory control of the relevant local vernaculars. This is the *sine qua non* of every good (meaningful) ethnography, which is above all a *semantic* enterprise.

Thus I totally agree with John U. Ogbu's cogent point (personal communication) that except where one studies one's own language, dialect, or subcultural group, an African ethnographer will face problems similar to those faced by foreign ethnographers in Africa and elsewhere.

Professor Ogbu's account of his personal experience based on a brief study of the Pika of Northern Malawi some years ago is worth recounting. "I had no knowledge of CiTumbuka before arriving in the field. I hired a native interpreter-informant who had some formal education and had lived in South Africa as labor migrant for a couple of years. But my interpreter-informant proved to be a handicap in some respects. As I became 'proficient' in CiTumbuka I realized that he did not always translate my questions to local people accurately; nor did he always give me in English their responses to my questions fully or accurately. I had a crash language course in the field as a way to solve the problem. This consisted of not only being taught CiTumbuka by the interpreter-informant, but also using published materials. . . . Among these materials was a bible in CiTumbuka which proved to be very useful in self-teaching, given my background in mission schools in Nigeria. Unfortunately, the fieldwork ended just at the point where I was beginning to be quite conversant with the use of the local language; that is, at the point when I could confidently tell when my interpreter-informant was making a mistake in either translating my questions or translating other informants' responses" (personal communication).

All of this underscores my main point about the crucial role of language acquisition and understanding in ethnographic research anywhere. Of course, the control of the relevant vernaculars cannot be a panacea for the total range of our epistemological problems in all aspects of our research, e.g., those associated with the dynamics of class, ethnic, racial, caste, age, sex, and individual differences. However, it cannot be disputed that the control of, for instance, lower class or upper caste dialectical differences is an indispensable first step toward meaningful ethnographies of lower class or upper caste subcultures.

[3]The obvious sophistry and Eurocentrism implied by Mead's foregoing remarks notwithstanding, her distinction between two broad types of study, one requiring *minimal* use of the native language and the other requiring *maximal* use of it (see particularly Mead 1939:194–196), whatever its analytic value, leads to further practical obfuscation. Under the former category are studies (a) to rescue the remains of "dying cultures" and (b) of survivals of primitive culture in a hybridized cultural situation in which everyone speaks a contact language. Under the latter category of studies are (a) those of social functioning, except where a lingua franca is sufficiently widespread to enable a *male* investigator to follow social trends in specific situations without a knowledge of the vernacular, (b) those that deal with the relationship between culture and personality, and (c) those that are concerned with symbolism.

[4]The subject of *probability sampling* in ethnography is a thorny one and merits a separate detailed treatment. It may suffice to note here that one of the most serious weaknesses of the ethnography of Africa, especially of colonial anthropology (and other "anthropological societies" as well), has been the ethnographers' total reliance on opportunistic sampling. That is, from a few, occasional, even casual and sometimes "trained" informants or informers, they derived sweeping, if highly imaginative, generalizations about the whole society and culture. After all, anthropology must be holistic. The Nuer, for example, numbered, according to Evans-Pritchard, about 200,000 at the time of study. Evans-Pritchard tells us that he never succeeded in training informants capable of dictating texts and giving detailed description and commentaries. Information was thus collected in particles, each Nuer he met being used as a source of knowledge. We are not told how many of the 200,000 or so Nuer he met. Godfrey and Monica Wilson, in the words of the latter in her *Good Company* (1951), gathered data on the Nyakusa of Central Africa who totalled about 234,000 from only four or so key informants. Fortes' Tallensi numbered about 35,000 according to the 1931 Gold Coast census. The data on the Tallensi come from two principal informants and perhaps "the many others, too numerous to mention" who "were our

faithful friends and zealous helpers" (1945:xii). Some anthropologists working in Africa, such as Günter Wagner (1949) who studied the Abaluhyia of North Nyanza, Kenya, in the 1930s, did not even consider it necessary to discuss their research methods at all. We need only stress here that we cannot without serious distortion of reality derive valid macrosociological theories or cross-cultural generalizations from our crude microsociological techniques (see Naroll 1970a, 1970b; Rohner 1975; Chilungu 1976).

[5]In a recent study, Rohner (1975:252-253) provides a reasonable, if perhaps charitable, measure of the ethnographer's proficiency in the languages of the people he is studying. Three categories or ratings of proficiency are proposed: (1) little or no knowledge of the native language, (2) some knowledge and understanding of the native language, and (3) fluency in the native language.

An ethnographer may be considered fluent in the language only when he makes an explicit statement to that effect. The ethnographer may be thought to have some knowledge and understanding of the language when he says so or when he is able to follow at least the gist of most casual conversations without being able to speak the language well enough to converse in it, except for phrases of etiquette. The ethnographer is said to have little or no knowledge of the language when he states that this is the case, speaks only English (or whatever his own native language may be) while doing fieldwork, or relies almost exclusively on interpreters.

Fourteen sub-Saharan African societies are represented in Rohner's study sample. It is significant to note that of the ten ethnographers whose language familiarity is rated, only one, Ashton (Sotho), had fluency in the language. Herskovits (Fon) had little or no knowledge of the language, and neither had Evans-Pritchard (Nuer) nor LeVine (Gusii). Fallers (Soga) was rated as having some understanding of the language, and Fortes (Tallensi) received a zero score.

[6]In a recent monumental work on the 18th and 19th-century Asante political system, Wilks (1975) chooses to describe and interpret the Asante historical experience in terms of culturally alien European terminology and modern concepts, such as the overall continuity of the "bureaucratic process," the "executive arm" of the "central administration" in Kumasi, and the "middle class" engaged in ideological debate over the relative merits of "free trade" vs. "state capitalism." Wilks presents the Asante political system of the period as if it were almost the exact copy of the latter-day British colonial administrative structure with its district and provincial commissioners.

The most disturbing thing about all this is that Wilks defends the application of "concepts developed in geographically and temporally different contexts" by insisting that "only thus can the Asante past be viewed within the wider perspectives of human endeavour and its place within comparative history ultimately be assured" (1975:xiv). Thus, in the interest of cross-cultural comparison, Western scholars adopt readily an approach to African societies and cultures that inevitably produces hasty and superficial cross-cultural generalizations. This type of cognitive and linguistic imperialism is very common indeed in Western "scientific" studies of non-Western peoples.

[7]Excerpts from a lunchtime conversation on structural anthropology between Sir Evelyn Blood, an English poet; Professor Raymond Petitjacques, a French philosopher; and Tony Caspari, a young Copertinian Brother, in Arthur Koestler's recent novel *The Call Girls* attest as much to the universal prestige enjoyed by anthropology in intellectual and lay circles in Western societies as they dramatize vividly the sources of the current epistemological dilemmas faced by ethnographers in the course of free application of well-established rules of ethnographic discovery to *alien* societies.

> Petitjacques: ". . . Cartesian dualism has been replaced by the Hegelian trinity of thesis-antithesis-synthesis, reflected in Marxist-Leninist dialectics. This in turn has been re-interpreted in the philosophy of Chairman Mao, but also amalgamated with the existentialism of Sartre and the structural anthropology of Levi-Strauss. . . ."
>
> "I don't see a bloody thing," Blood said, inspecting the substantial plate of stewed meat that Mitzie banged down in front of him. "It's goulash," he stated.
>
> "Do you mean the dish or the philosophy?" Tony asked.
>
> "Both."
>
> "You are right, a goulash," Petitjacques confirmed enthusiastically. "We are cooking a very hot and piquant ideological stew. It will burn your mouth."

[Blood]: "Monkey chatter."

[Petitjacques]: "Perhaps. But the young baboons have shown that they mean business when they invaded the citadels of so-called learning."

[Blood]: "And shitted all over the place. What's that to do with structural anthropology?"

[Petitjacques]: "It is appropriate. You have not read Levi-Strauss." Blood stared at him. "You will be surprised. I had a go. Pure jabberwocky. I couldn't believe my eyes. I had another go. The dialectics of boiled, roast and smoked food—the contrast between honey and tobacco—the parallel between honey and menstrual blood—hundreds of pages of inane verbal jugglery—it's the biggest hoax since the Piltdown skull, and you lap it up—like honey." Blood's face had gone the color of Burgundy, and his eyes were bulging.

"I didn't know that you were much interested in anthropology," Petitjacques said "I shall not hesitate to admit that the great man has a tendency to go off the rails. It is the Gallic tradition. But that is not the reason why young baboons are attracted to him. It is the message he derived from his analysis of Greek mythology: 'If Society is to go on, daughters must be disloyal to their parents, and sons must destroy their fathers.' "

[Blood]: "And you are on the side of the baboons. An intellectual pimp." [Arthur Koestler, *The Call Girls*, pp. 56-57, 1973, Random House, Inc. Reprinted by permission.]

[8]Rohner has also recently pointed to one source of serious error in Western ethnographic research, namely, "the bias of romanticism" (1975:203-204), expressive of a kind of "moral commitment" of anthropologists to see "their people" in a positive light, to patronize the people they study. Rohner explains the problem in terms of ethnographers' uncompromising belief in cultural relativity and the functionalist interpretation of ethnographic data. Gellner (1971:18-49), in evaluating primarily Evans-Pritchard's interpretation of Nuer religion, has equally criticized British anthropologists for being "charitable," that is for employing in the interpretation of non-Western belief systems a "hermeneutic principle" that ensures "*in advance* of inquiry that nothing may count as pre-logical, inconsistent, or categorically absurd though it may be" (1971:36; emphasis added). Gellner locates the source of this peculiar ethnographer bias both in extreme functionalism and in the problems of translation—the striving to find equivalents in English or other European languages for native statements or concepts ill-understood by the anthropologist who is not fluent in the native lanuage, but making it a "condition of good translation that it conveys the coherence which he assumes is there" to be found in the thoughts of non-Western peoples (1971:26). My point here is not to argue whether or not non-Western thought is prelogical, logical, or postlogical, an issue which is itself a reflection of Western philosophical prejudices, but to stress the distortions in ethnography caused by the lack of language familiarity.

[9]Those of us natives who saw, even as undergraduate students in British universities, the dangerous limitations of tradition-bound *tribal* research in Africa and its distorting epistemological presuppositions and sterile, self-opinionated theories, and who later as *anthropologists* have attempted to transcend these limitations by venturing into the broader, historically more relevant, context of colonialism and political economy of development in Africa, are labeled, at best, *sociologists* and *political scientists*, branches of social science not much disconcerted by real issues. At worst, we are polemists, propagandists, and anything but anthropologists and scholars. (See Robertson 1975 for a frank assessment of an aspect of this problem.)

[10]Commenting recently on local T.V. on the Second World Black and African Festival of Arts and Culture (FESTAC) in Nigeria, Dr. Mahdi Adamu, Director of the Centre for Nigerian Culture Studies of Ahmadu Bello University, went as far as saying that the festival could not help project the true concept of African culture, for FESTAC was "an elitist phenomenon based on Western cultural values . . ." (see *FESTAC Notebook*. In *West Africa*, 13 December 1976:1,923).

[11]If one were to take Fortes' comments seriously, one would have to reject as scientifically useless what Western social scientists write about their own societies. Schneider's *American Kinship*, for example, would have to be scientifically worthless!

[12]These are the people who were described by British colonial administrators as the "martial races" and since 1900 had been some of the empire's dutiful forced laborers and colonial soldiers in both World War I and World War II.

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