Cultural Relativism and the Future of Anthropology

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Unlike old generals, the debate regarding cultural relativism neither dies nor fades away, as a spate of recent publications indicates (for example, Brown 1984; Geertz 1984; Gellner 1985; Hatch 1983; Hollis and Lukes 1982; Jarvie 1984; Lloyd and Gay 1981; Meiland and Krausz 1982; Shweder 1984). If the present contribution to this debate is not entirely redundant, it is because it has somewhat different aims.

The first aim of this article is to clear away some of the intellectual underbrush that has served to obfuscate some of the controversies surrounding this perennial debate. These controversies, I hope to show, have been beset by two conceptual muddles. The first derives from the frequently committed fallacy of confusing cultural relativism with cultural variability or diversity. The second derives from the fact that in contemporary anthropology there are not one, but three types of cultural relativism—descriptive, normative, and epistemological—which, because they are designated by the same term, are often conflated. These three types of cultural relativism are not merely analytically separable but historically distinct.

The second, and more important, aim of this article is to explicate the adverse consequences of epistemological relativism—arguably the dominant contemporary type of cultural relativism—for anthropological theory and research.

On the Three Types of Cultural Relativism

Descriptive Relativism

Descriptive relativism (a judgment concerning the fact of cultural variability) is based on the theory of cultural determinism. Since, according to that theory, human social and psychological characteristics are produced by culture, then, given the fact of cultural variability, descriptive relativism is its obvious corollary: the variability in social and psychological characteristics across human groups is relative to—depends on—cultural variability.[1]

Since, however, different theorists make different assumptions in respect not only to the magnitude of cultural variability, but also the scope of cultural determinism, there are actually three forms of descriptive relativism. We shall refer to

them as the strong, moderate, and weak forms respectively. Although, as we shall observe below, the differences among them are fundamental, we shall postpone their explication until we have had an opportunity to discuss the other two types of cultural relativism, normative and epistemological.

Normative Relativism

Building on descriptive relativism, normative relativism actually consists not of one, but of two judgments, one regarding culture itself, the other regarding its putative social and psychological products.

Regarding culture, the claim is as follows: because all standards are culturally constituted, there are no available *trans*cultural standards by which different cultures might be judged on a scale of merit or worth. Moreover, given the fact of cultural variability, there are no universally acceptable *pan*cultural standards by which they might be judged on such a scale. In short, since all judgments regarding the relative merit or worth of different cultures are ethnocentric, the only valid normative judgment that can be made about them is that all are of equal worth. That judgment holds in respect to total cultures (German as compared with Hopi); single cultural systems, such as religion (Judaism as compared with Mithraism); and particular cultural propositions (monotheism as compared with polytheism).

Since any cultural system (religion, kinship, science, ethics, and the like) consists of both *descriptive* and *evaluative* propositions, when a cultural proposition of one group is judged to be better or worse than an alternative proposition of some other group, that judgment is perforce based on a different dimension of merit or worth, depending on whether the paired propositions are descriptive or evaluative. Thus, while descriptive propositions (for example, "the earth is round," "witchcraft causes cancer," etc.) are judged along a *true-false* dimension, inasmuch as evaluative ones (for example, "stealing is immoral," "atonal music is beautiful," etc.) are statements of value, preference, and the like, they are judged along a *right-wrong* dimension. That being the case, normative cultural relativism comprises for the most part two subtypes, usually designated as "cognitive" and "moral" relativism, respectively (Jarvie 1984; Meiland and Krausz 1982).

According to cognitive relativists the truth claims of descriptive propositions are relative to the cognitive standards of the cultlures in which they are embedded. In short, all science is ethnoscience. Hence, since modern science is Western science, its truth claims (and canons of proof) are no less culturally relative than those of any other ethnoscience (Scholte 1984).

According to moral relativists, the claims of ethical propositions are relative to the moral standards of the cultures in which they are embedded. Just as in the case of food preferences or aesthetic canons, so also in the case of moral codes, there are no universally acceptable standards by which the latter might be validly judged on a scale of relative merit or worth.

Turning now to the putative social and psychological products of culture, the claim of normative relativism is as follows: since there are no universally accept-

able evaluative standards, any judgment regarding the behavior patterns, cognitions, emotions and the like of different social groups—judgments such as good or bad, right or wrong, normal or abnormal, and the like—must be relative to the variable standards of the cultures that produce them. Thus, for example, although the Kwakiutl exhibit a constellation of characteristics which, according to Western standards, are paranoid, the latter judgment is invalidly applied to the Kwakiutl constellation because, according to Kwakiutl standards, it is judged to be normal (Benedict 1934). Similarly, if the logical processes underlying Azande magic violate normal canons of logic, it is nevertheless impermissible to judge it as irrational because logical canons, like anything else, are culturally variable. In short, since all logic is *ethno*logic or *socio*logic, the judgment that Azande magic is irrational merely reflects an ethnocentric preference for Western logic (Barnes and Bloor 1982; Winch 1958. For detailed discussion, see Finnegan and Horton 1973; Hollis and Lukes 1982; Lloyd and Gay 1981).

Epistemological Relativism

The point of departure of epistemological relativism is the strong form of descriptive relativism. Adopting the Lockeian view that the mind of the human neonate is a *tabula rasa,* the strong form of descriptive relativism claims—now departing from Locke—that everything that is eventually found on that blank slate is inscribed by culture. Thus, if "culture patterns provide the template for *all* human action, growth and understanding" (M. Rosaldo n.d., emphasis mine), and if moreover "culture does not dictate simply *what* we think but how we feel about and live our lives" (M. Rosaldo n.d., emphasis in original),[2] then virtually all human social and psychological characteristics are culturally determined.

This wholesale theory of cultural determinism is not in itself, however, the distinguishing feature of the strong form of descriptive relativism. Rather, it is distinguished by the conjunction of wholesale cultural determinism with an all but limitless view of cultural diversity. That conjunction produces—what might be called—the particularistic, in contrast to the older generic theory of cultural determinism which is associated with a less sweeping view of the magnitude of cultural diversity.[3]

According to generic cultural determinism, culture is man's species-specific mode of adaptation, analogous to those biological specializations that characterize the adaptive modes of nonhuman animals. From that perspective, every culture is a variant of a universal culture pattern (Wissler 1923), and therefore any culture produces a common set of *uniquely human* psychological characteristics which comprise what is called the psychic unity of mankind. Hence, man's phylogenetically determined biological unity combined with his culturally determined psychic unity produce a pancultural human nature.

By contrast, particularistic cultural determinism, the version which is held by the strong form of descriptive relativism, holds that inasmuch as cultures are radically different from each other, each culture produces a set of *culturally particular* human characteristics. The degree of particularity, and, therefore, the

magnitude of cultural diversity which is postulated by this version of cultural determinism may be gauged from Geertz's comments on the academic world. The various subgroups that comprise that world (scholars, artists, scientists, professionals, and administrators) are, he writes, "radically different not just in their opinions, or even in their passions, but in the very foundations of their experience" (Geertz 1983:160).

Now if the subcultures *within* a single society are so different that they produce radically different psychological characteristics, then the magnitude of the differences that separate cultures *across* societies must be even larger, and the degree of psychological differences that they produce must be correspondingly large. And that is precisely what is claimed by the strong form of descriptive relativism.

As for cultural diversity, its magnitude is held to be all but limitless. Each culture, it is claimed, is unique, not in the trivial sense in which, for example, every snowflake or crystal may be said to be unique, but in the fundamental sense of being incommensurable with any other (Schneider 1984). That being the case, particularistic cultural determinism explicitly rejects the concept of the psychic unity of mankind, arguing instead that human nature and the human mind—the two components of the psychic unity concept—are culturally variable (Geertz 1984; M. Rosaldo 1984). That is why some advocates of this view—following contemporary French thinkers like Ricoeur and Foucault—refer to persons and groups whose cultures are different from our own as the "Other" (Rabinow 1977), their "fundamental Otherness," of course, being culturally, not biologically, determined.[4]

From this strong construal of descriptive relativism, epistemological relativism draws two conclusions. First, panhuman generalizations regarding culture, human nature, and the human mind are likely to be either false or vacuous; only if their provenance is confined to particular groups are generalizations likely to be both true and nonvacuous (Geertz 1973:25–26). By the same token, any general theory that purports to explain culture, human nature, and the human mind is likely to be either invalid or trivial; only if its explanatory scope is restricted to the characteristics of a particular group—and even then, only if it incorporates the latter's cultural assumptions regarding those characteristics—is a theory likely to be both valid and nontrivial (Barnes and Bloor 1982; M. Rosaldo 1984).

Put differently, significant generalizations and theories can be true and valid, respectively, if and only if they are both group-specific and culturally relative. Thus, since the "science" of psychology is Western ethnopsychology "disguised in weighty tomes" (Lutz n.d., quoted in M. Rosaldo 1984:142), its theory of aggression, for example, might explain aggression in the West, but it cannot explain aggression among the Ilongot because they make very different cultural assumptions regarding the vicissitudes of hostility and anger (op. cit.). Similarly, because the "science" of anthropology is Western in its assumptions, its theories of culture and society cannot explain Muslim social institutions; the latter can only be explained by an "Islamic Anthropology," one based on Islamic cultural as-

sumptions (Ahmed 1984). In sum, anthropology has the choice—to modify the famous dictum of F. W. Maitland—between being emic ethnography or nothing.[5]

That brings us to the second conclusion that epistemological relativism draws from its strong construal of descriptive relativism, a conclusion that entails no less than a sea change in the conception of anthropology as a scholarly discipline. Since cultures are incommensurable and all science is ethnoscience, the very notion of cultural explanation is misplaced. Explanations are based on general theories, causal laws, and the like, which pertain to classes or types of phenomena, whereas the radical pluralism of cultures precludes the establishment of classes or types without doing violence to the "particularity and complex texture" of each culture (Rabinow and Sullivan 1979:4). In short, since in the nature of the case, "anthropology is the study of particular cultures" (Schneider 1984:196), the explanatory paradigm of scientific inquiry, though entirely appropriate for the study of the physical world, is misconceived when applied to the study of cultural worlds.

Hence, embracing the older Continental dichotomy between the *Naturwissenschaften* and the *Geisteswissenschaften*[6] and viewing anthropology as a discipline of the latter type, epistemological relativism regards the aim of anthropology to be not the *explanation* of culture, but rather the *interpretation* of particular cultures. Since, according to this view, culture is conceived as a symbolic system, the interpretation of a culture consists of the elucidation of its symbolic meanings. That aim is achieved not by the application of general principles or theories (of culture, behavior, mental functioning, symbolic processes, whatever) to the subject under investigation, but by the ethnographer's empathy, *Verstehen,* insight, imagination, understanding, and the like. (Geertz 1973; Habermas 1971; Rabinow and Sullivan 1979; Ricoeur 1970; Winch 1958).

Although epistemological relativism is viewed by epistemological relativists themselves as both logically entailed by descriptive relativism, and as itself entailing normative relativism, that is a decidedly modern view. In fact, each of these types arose independently in separate historical periods.

The Relationship Among the Three Types of Cultural Relativism

Although 19th-century cultural anthropologists were descriptive relativists—for them diversity in beliefs and customs was a function of cultural diversity—none of them so much as contemplated epistemological relativism, and they all explicitly opposed normative relativism. Thus, with respect to the latter, the diversity in culture which they documented over space and time was construed by them as supporting not only the unilineal stage theory of social and cultural evolution, but also the theory of progress, according to which Victorian culture—their own—was better by far than any of the cultural stages that preceded it.

It was only with Boas, and the rise of the American Historical School, that anthropologists came to believe that descriptive relativism entailed normative relativism. Boas and his followers rejected not only the theory of social and cultural evolution, but also—and more important for the present discussion—the notion

of progress as well. Since the latter is a value judgment, and since values, they argued, are culturally relative, the judgment that Western culture is more advanced than, and therefore superior to, primitive cultures is only an expression of Western ethnocentrism (Herskovits 1972). In the service of this egalitarian view, the very term "primitive" was expunged from the anthropological lexicon and replaced, first by "preliterate" and then—because that, too, seemed invidious—by "nonliterate."

For those still not convinced that descriptive relativism does not entail the other two types, it might be observed, first, that not all of Boas's students accepted normative relativism. Kroeber, for example, rejected his teacher's views in this matter (Kroeber 1948:597–607), even while supporting his opposition to cultural evolution and extending the range and scope of his cultural determinism (Kroeber 1917). Second, just as the cultural evolutionists, though adopting descriptive relativism, did not espouse normative relativism, similarly most Boasians adopted normative relativism, but none espoused epistemological relativism.

To be concrete neither Boas himself, nor any of the pioneering normative relativists who followed his lead, held that because of the diversity of culture, anthropological generalizations and theories are necessarily culturally relative, and that consequently anthropology cannot or ought not be a scientific discipline. On the contrary, they all affirmed the obverse of these propositions.

Melville Herskovits, for example, perhaps the most polemical—if not the most radical—of the early normative relativists, was simultaneously the most insistent upon the scientific status of anthropology (Herskovits 1949). Moreover, it was precisely because he viewed all cultures as of equal value that he held, contrary to epistemological relativism, that empirical cultural generalizations and lawlike cultural theories could be sought. And it was because he believed in the "psychic unity of mankind" that he expected that search to be successful. Indeed, for Herskovits, anthropology had the choice (to now reverse the dictum of Maitland) between being science or nothing.

It was not until the 1960s, some 30 years following the development of normative relativism, that epistemological relativism, a creation of a new, post-Boasian, generation of anthropologists—known as "symbolic anthropologists"—entered upon the anthropological stage. Although the relationship between them is by no means perfect (cf. Turner 1978), it is not inaccurate to say that, in general, epistemological relativism is the philosophical and methodological concomitant of the principle tenets of symbolic anthropology (the symbolic constitution of social reality, the radical diversity of symbolic worlds, the essential arbitrariness of symbols, and the like).[7]

It should now be evident that although normative and epistemological relativism alike take their point of departure from descriptive relativism, they are otherwise historically and analytically independent of each other. That is because, in addition to their shared agenda, each also has its separate agenda. Their shared agenda—the agenda of descriptive relativism—is to challenge the validity of *noncultural* theories of social and psychological variability. But whereas the separate agenda of normative relativism is to challenge the validity of *ethnocentric* theories

of variability (particularly, in the case of Boas and Herskovits, those that are racist in inspiration), that of epistemological relativism is to challenge the validity of *scientific*, that is, universalistic-explanatory, theories of variability.

Given their separate agendas, it is no accident that while epistemological relativism takes its departure, as we have seen, from the strong form of descriptive relativism, normative relativism, in its Boasian version, takes its departure from the moderate form. Although agreeing with the former that (1) virtually all social and psychological characteristics are culturally determined, the moderate form departs from it in holding that (2) many of those characteristics are universal because (3) despite the wide diversity in culture, there is also a significant degree of cultural universality. In short, this form espouses the notion of the psychic unity of mankind.

That the moderate form of descriptive relativism should have served as the scientific charter for the agenda of Boasian normative relativism is not difficult to understand. Rejecting biological, that is, racial, diversity as the explanation for social and psychological diversity, and replacing it with cultural diversity, Boas and his students then made two crucial moves. First, all cultures are equally valuable; second, the nonwhite races that preponderantly make up nonliterate societies are as capable as any other of inventing and acquiring the technologically more complex cultures of literate societies (Boas 1938).

To support the latter claim they invoked the traditional anthropological axiom that all races, whatever their culturally determined psychological diversity, share a "psychic unity." In order, however, to reconcile that axiom with, on the one hand, the fact of cultural diversity and, on the other hand, the theory of wholesale cultural determinism, they adopted a moderate view of cultural diversity, one which allowed for a certain degree of cultural universality (which then accounted for psychic unity).

Epistemological relativism by contrast not only espouses the theory of wholesale cultural determinism, but it also holds a maximal view of cultural diversity; a combination, so it contends, that precludes both nonvacuous cultural generalizations as well as the axiom of psychic unity. Hence, that combination—that is, the strong form of descriptive relativism—constitutes its charter for the claim that anthropology is an interpretive, ("hermeneutic"), not an explanatory ("scientific") discipline. (That same combination, of course, also constitutes the charter for the normative agenda associated with epistemological relativism, whose target is not a racially inspired ethnocentrism—which is no longer scientifically problematic—but a socially and culturally inspired one.)

A Critique of Epistemological Relativism

The Claim of Radical Cultural Diversity

Espousing the strong form of descriptive relativism, epistemological relativism takes its point of departure, it will be recalled, from the premise that the range of cultural—and, therefore, social and psychological—diversity is virtually limitless. Hence, before assessing its specifically epistemological claims, it is per-

haps useful to assess this premise from which they are derived. There are, I believe, two grounds—theoretical and empirical—for rejecting that premise. The theoretical ground rests on a set of considerations derived from human biological and behavioral evolution, the empirical on the epistemological entailments of ethnographic research. We shall begin with the former.

Since, as a species, we are the product of biological (including behavioral) evolution, it is not necessary to subscribe to the program of sociobiology to acknowledge—together with our colleagues in biological anthropology—that as a result of selective evolutionary pressures, our hominid ancestors acquired a set of species-specific biological characteristics, many of which are socially and culturally relevant. I would mention, for example, the relationship between the evolutionary development of bisexual reproduction, prolonged infantile dependency, and the suppression of estrus, on the one hand, and the universality of the biparental human family and the parent-child incest taboo, on the other, as only one instance of that rather elementary proposition (cf. Fox 1980; Symons 1979:ch. 4).

Given this causal (but ultimately feedback) relationship between a set of evolutionary biological characteristics, on the one hand, and a set of universal social and cultural characteristics, on the other, we might then expect to find, in accordance with even the weak form of cultural or social determinism, a set of universal psychological characteristics attendant upon that relationship. And to this set, let us add a second which is the direct product of biological evolution (unlike the former, which is socially or culturally mediated). In this second set I would include such characteristics, for example, as pain avoidance, object constancy, attachment behavior, and the like, which, of course, are rooted in our mammalian heritage.

To these two sets of universal psychological consequences of biological and behavioral evolution, we may now add a third—one consisting of the products of the interaction among the putative biological, social, and cultural universals mentioned above. Thus, for example, it would not be incautious to assume that everywhere the violation of strongly internalized cultural norms and values arouses some type of emotionally painful reaction such as shame, guilt, the lowering of self-esteem, and the like. Given, moreover, the universal tendency to avoid pain, we might expect to find that everywhere psychological defenses of some form—repression, displacement, etc.—are found as cognitive resources for coping with these types of emotional pain (Hallowell 1955:ch. 1, 4).

Many more items might be added to this brief inventory of cultural, social, and psychological universals. But these few are perhaps sufficient to support my contention that it is only by rejecting human biological evolution that the premise of radical cultural pluralism—the incommensurability of cultures—can be sustained. Contrariwise, the acceptance of human evolution, together with its highly probable social and psychological consequences, all but assures the universality of many nontrivial cultural universals.

Put differently, although the impressive degree of cultural, social, and psychological variability renders either the weak or moderate forms of descriptive relativism inevitable, human biological evolution renders the strong form unimag-

inable. In my view, however, it is not the moderate form (with its wholesale cultural determinism) that is inevitable, but rather the weak form.

Briefly, the latter form can be outlined as follows: (1) because the range of cultural diversity, though formidable, is constrained by adaptive evolutionary imperatives, cultures also display universal features; (2) although human social and psychological characteristics are culturally determined to some (but to, as yet, an empirically unknown) degree, they are also determined by other variables, such as ecology, biology, subsistence economy, social structure, socialization, and the like; consequently, (3) although the diversity in human social and psychological characteristics is large, to the degree that their determinants, both cultural and noncultural, display universal features, their diversity is constrained to the same degree; therefore, (4) although much of the diversity in those characteristics is culturally relative, much of it is also relative to the diversity in noncultural variables; moreover, (5) to the extent that many social and psychological characteristics are (to, as yet, an undetermined degree) universal, they are not relative at all.

Lest I be misunderstood, it should be emphasized that cultural "universals" refers not only to cultural "content"—which is what the strong form of descriptive relativism refers to in claiming that cultures are unique—but also to cultural function. This qualification has two theoretical consequences.

On the evolutionary argument that many cultural, and social, characteristics of human groups are responses to a set of species-specific needs, it follows that cultural variability is constrained by the adaptive and integrative prerequisites of any viable social group. This is the functionalist assumption of "functional prerequisites" (Merton 1957:19–84).

The latter assumption, however, does not entail that for every prerequisite there is only one viable social or cultural response for its satisfaction. On the contrary, in view of the remarkable degree of human plasticity (also a product of human evolution), for any prerequisite there exists a wide range of potentially viable alternative responses; and, for reasons peculiar to their historical circumstances, different groups have actualized some, rather than others, of those potential responses. In consequence, many differences in kinship systems, religious doctrines, and the like may be properly viewed—but to, as yet, an empirically unknown degree—as structural alternatives for satisfying one and the same prerequisite. In sum, although differing in content, these different social and cultural structures serve the same function. This is the assumption of "[structurally alternative] functional equivalents" (Merton 1954:19–84).

Rejecting this functionalist view of cultural and social systems and directing its interpretations exclusively to the emic meaning of cultural content, it is hardly surprising that epistemological relativism holds that cultures are incommensurable. Given this limited and limiting perspective, how could it be denied, for example, that the doctrine of karma is unique to the religious tradition of India? Or that, even within that tradition, its meaning in Buddhism may be different from that in Hinduism. Or, for that matter, that even within Buddhism (Spiro 1982a) and within Hinduism (Keyes and Daniel 1983) it again has different meanings.

If, however, the concept of karma is understood in the context of Hindu and Buddhist praxis—that is, functionally—it is then apparent that, at only a slightly more abstract level, it bears a striking family resemblance to concepts found in many other cultural traditions. Consider, for example, such concepts as luck, fate, predestination, God's will, kismet, fortune, destiny, or, for that matter, cultural determinism! Although formally and semiotically different from each other, and they in turn from karma, all of those concepts, just like karma, provide an explanation for the vagaries of an actor's "life chances" (as Weber called it) without recourse to the agency (and therefore the responsibility) of the actor himself.

Let us turn, now, to the empirical, or epistemological, challenge to the premise of radical cultural—hence social and psychological—diversity. That challenge is simply put: if cultures are incommensurable and if the characteristics of human nature and the human mind are predominantly culturally determined, how is it at all possible for an ethnographer to understand a group that is different from his or her own?

For ethnographers to understand an alien people, it is a *sine qua non,* as epistemological relativists among others rightly claim, that they be able to exploit their own powers of empathy and insight. But how can the ethnographer participate to any degree in the thoughts and feelings of others—which is what empathy means—if their psychological makeup is radically different from the ethnographer's? Moreover, how can the ethnographer make any sense of their thoughts and feelings—which is what insight means—if their culture, or conceptual framework, is also radically different from the ethnographer's? As Sperber has tellingly put it,

> If members of other cultures live in different cognizable worlds and if one thing we can take for granted is that these worlds are more complex than that of cats [then, since we cannot intuit what cats feel or think] how can we get to know them? Shouldn't we conclude with Rodney Needham that "the solitary comprehensible fact about human experience is that it is incomprehensible"? [1982:157]

Nevertheless, it is an anthropological fact, as Gellner has stressed, that every ethnographer, without exception, has been able to comprehend the "human experience" of alien peoples.

> It is an interesting fact about the world we actually live in that no anthropologist, to my knowledge, has come back from a field trip with the following report: *their* concepts are *so* alien that it is impossible to describe their land tenure, their kinship system, their ritual. . . . As far as I know there is no record of such a total admission of failure. [1981:5, emphasis in original]

In short, since the "human experience" of all those alien peoples *is* comprehensible to ethnographers, it can only be assumed that although culture, human nature, and the human mind are diverse enough, they are not all *that* diverse. In other words, if alien peoples are comprehensible to ethnographers (and the ethnographers to them), it is because the characteristics which they share—the uni-

versal characteristics of culture, human nature, and the human mind—are at least as prominent as those in which they differ.[8] To be sure, ethnographers may be deluded in their belief that they understand all those alien peoples; but if that is the case, that delusion is shared by epistemological relativists, as well, for they also claim to understand the peoples they study.

In fact, if cultures were truly incommensurable, then (as I have observed elsewhere [Spiro 1984:345]) the ethnographer not only could not understand, but could not even describe a culture unless the ethnographer himself or herself had been enculturated in it. Moreover, to accurately convey its meanings, the ethnographer would have to describe it in the native language for, *ex hypothesi,* the conceptual system of one culture cannot be adequately rendered by the concepts of another.[9] Consistent with that conclusion, Schneider (1984:ch. 31), in his "Second Description" of the Yapese kinship system, refrains from translating Yap kin terms, just as he refuses to designate it as a kinship system.

In sum, epistemological relativists can't have it both ways. They can't at one and the same time argue that cultures are incommensurable while also claiming that ethnographers (themselves included) are able to understand the cultures and minds of alien peoples. That they have been able to achieve such understandings is itself sufficient to refute the premise of radical cultural diversity from which epistemological relativism derives its epistemological claims.

Can Anthropology be a Science?

Given the premise of radical human diversity, epistemological relativism, it will be recalled, makes two fundamental claims: (1) panhuman generalizations and theories (cultural, social, and psychological) are in general either false—because ethnocentric—or trivial and vacuous. Hence, anthropology neither is nor can it in principle become an explanatory—a scientific—discipline. Rather, (2) it is, or it should constitute itself as, an interpretive—hermeneutic—discipline. We shall examine these claims *seriatim.*

That any or all of the generalizations and theories of the social sciences (including anthropology) may be culture-bound is the rock upon which anthropology, conceived as a theoretical discipline, was founded. But the proper scholarly response to this healthy skepticism is not, surely, their a priori rejection, but rather the development of a research program for their empirical assessment. And that is precisely what I take anthropology to have been attempting among other things, to do over the past 70 or 80 years of its existence. Altough these attempts have not been as successful as we might have wished, given the formidable conceptual and operational difficulties of such an enterprise, our studies have nevertheless yielded some not inconsiderable results (for example, Levinson and Malone 1980).

This achievement has been possible precisely because of the wide range of human diversity (cultural, social, and psychological). That is, far from constituting an insurmountable obstacle to the establishment of generalizations and theories, such diversity, I shall now argue, is, paradoxical though it may seem, its necessary condition.

A scientific generalization, whether in the physical or the human sciences, is a statement not to the effect that some object or event is universal—though it might be—but that, the world being lawful, its occurrence (whether universal or singular, frequent or infrequent) is governed by a *principle* that is universal. In other words, a scientific generalization states that whenever and wherever some object or event occurs, its occurrence sustains a systematic—that is, a predictable—relationship to some other object(s) or event(s).

Consider, for example, religion. Although by almost any definition religion is a universal cultural system, in light of the bewildering variety of religious belief systems, that generalization is in itself of only limited intellectual interest. It is the kind of generalization that epistemological relativists quite properly call trivial. Hence, a nonvacuous generalization regarding religion is a generalization not about religion in general, but about concrete types of religious-belief systems: monotheism, polytheism, and the like. And in this regard, a scientific generalization is a statement of the kind not that monotheism is universal—and not only because it is false—but that wherever and whenever monotheism occurs, it sustains a systematic relationship to the occurrence of some other specified social or cultural condition.

By the application of Mills's canons of similarity and difference, ethnographic studies of different religious belief systems can ascertain to what degree, if any, such regularities obtain.

Taking these generalizations as paradigmatic, it will now be noticed (as I argued above) that (1) scientific generalizations regarding society or culture are statements not of frequencies but of *regularities* and (2) although these regularities are of universal scope, they pertain not to universals but to *differences* in social and cultural conditions (in this case religious belief systems).

Both propositions apply to psychological generalizations in anthropology as well. Thus, the traditional postulate of the psychic unity of mankind does not claim that the content of the human mind is everywhere the same—of course the Ifaluk and Ilongot, for example, differ in respect to aggression and they, in turn, from the French and the Fulani—but that the working of the mind is everywhere the same. In short, psychic unity is a generalization to the effect that despite the wide differences in the form and frequency of aggression, wherever and whenever aggression occurs, it is governed by the same set of principles.

If, then, anthropological generalizations are statements of regularities in respect to group differences, anthropological theories are statements that purport to explain or account for such differences.

Suppose, for example, that differences in religious belief systems have been shown to be related to differences in family systems. If that were the case, it might then be proposed that the latter generalization might be explained by the theory that the belief in supernatural beings is a projection of the conceptions of parents that are formed in childhood. Thus, since children's conceptions of parents are formed in large part as a result of the parent-child relationship, such a theory can (in principle) explain not only the universality of religious belief systems, but also their diversity—the association of different types of religious belief systems with

different types of family and socialization systems. Again, this theory like the previous generalization can be tested by employing Mills's canons in comparative ethnographic investigations.

We may now briefly summarize this discussion. Scientific generalizations take the form of "if . . . then . . ." propositions, and scientific theories purport to explain generalizations of that form. That being the case, in the absence of a wide range of diversity in the social, cultural, and psychological characteristics of human groups, generalizations of that form could not be discerned in the first instance, theories that purport to explain them could not be formulated, and empirical tests of those theories could not be conducted. In short, if *pace* epistemological relativism, nonvacuous generalizations, and nontrivial theories have been discovered by anthropology, it is because of—not in spite of—cultural, social, and psychological diversity.

If it may be now concluded that human diversity does not preclude anthropology from being or becoming a scientific discipline, epistemological relativism opposes that conclusion on still other grounds; not descriptive but prescriptive.

Should Anthropology be a Science?

Even conceding that nontrivial generalizations and nonvacuous theories are discoverable, epistemological relativism rejects the conception of anthropology as a scientific discipline not so much on pragmatic as on principled grounds. The proper aim of anthropology, it contends, is not explanation but interpretation—the elucidation of symbolic meanings—and that aim requires that anthropology be conceived and practiced not as a scientific but as a hermeneutic discipline.

Actually, many epistemological relativists, following the Continental tradition mentioned previously, prefer another set of locutions. Although anthropology, like the other disciplines that study man and culture, is a science, it is not, they say, a natural science but a human science *(science humaine),* one whose mode of inquiry is hermeneutic not positivistic (Rabinow and Sullivan 1979).

To be sure, if "science" is conceived substantively—as a *subject* of inquiry—there are then, of course, many kinds of science, from the study of atoms and galaxies to that of mind and culture; and it might then be agreed that at the most inclusive level of classification, it is useful to distinguish the class of human sciences (anthropology, history, psychology, etc.), on the one hand, from that of the physical sciences (physics, chemistry, astronomy, etc.), on the other.

It will be noticed, however, that I refer to the members of the latter class not as natural, but as physical, sciences, in order to explicitly reject the dichotomy between nature and culture. The notion that somehow atoms, trees, and stars are part of nature whereas mind, culture, and human experience are not is a notion which, I would have thought, had been laid to rest many years ago by John Dewey (1929) among others. To place culture outside of nature is an anthropological conceit reminiscent of the creationist conceit that places man outside of nature. To say that mind and culture are part of nature does not, however, entail the reductionistic fallacy that they can be explained by the laws and principles of the physical sciences.

If, now, physical is not synonymous with natural, the physical science/human science dichotomy does not entail, as the hermeneutic tradition claims, the natural science/hermeneutic science dichotomy, for the former dichotomy is substantive, the latter epistemic.

If science is conceived epistemically, I would then submit in agreement with many logicians and philosophers of science (for example, Dewey 1938; Grunbaum 1984; Hempel 1965; Popper 1963; Rudner 1966) that there is only one kind of science. For whatever the subject (whether atoms and galaxies or man and culture) if the aim of scientific inquiry is reliable (replicable) and public knowledge, only one method is properly designated as scientific. That method consists in the formulation of explanatory theories in respect to that subject, and the employment of both empirical and logical procedures which, at least in principle, can lead to their verification or falsification.

The hermeneutic tradition (including now epistemological relativism) rejects that view, arguing that the substantive differences between the physical (what they call the "natural") sciences and the human sciences entail a critical epistemic difference as well.

Because, it is argued, the aim of the physical sciences is *explanation,* they offer causal accounts of the subject under investigation, for which the scientific method as defined above is entirely appropriate. Inasmuch, however, as the aim of the human sciences is *interpretive,* that method is inappropriate because interpretive accounts have reference to meanings, not causes. Consequently, the human sciences require a hermeneutic method of inquiry (whose characteristics will be described in the following section). Let us now evaluate this argument.

When it is claimed that interpretive accounts have reference to meanings, what is usually meant[10] is that culture, mind, action, social institutions, and the like—the subjects comprising the human sciences—are to be understood in terms of intentions, reasons, purposes, motives, and the like (Vendler 1984). Inasmuch as that claim explicitly opposes meanings to causes, it rests on the presumption that the concept of cause, on the one hand, and the conceptual set comprising, *inter alia,* intentions, reasons, and motives, on the other, are mutually exclusive.

Now that presumption would be entirely convincing if, as its proponents contend, the scientific concept of cause referred to material conditions alone (Habermas 1971; Ricoeur 1981; Vendler 1984). For by that conception of cause, a causal account of culture could only have reference to ecological niches, modes of production, subsistence techniques, and the like, just as a causal account of action could only have reference to the contraction of muscles, the firing of neurons, the secretion of hormones, etc.

But the contention that the scientific conception of cause is restricted to material conditions is hardly self-evident. To be sure, psychological behaviorists deny that purposes, motives, and intentions serve as causes of human action, just as cultural materialists dismiss their causal relevance for the creation and persistence of culture. But such views are rejected even by tough-minded philosophers of science (Grünbaum 1984:69–94; Hempel 1965:225–258). For by the most rigorous conception of cause—any antecedent condition in the absence of which

some stipulated consequent condition would not occur—purposes, motives, intentions, and the like, for all their being nonmaterial, are no less causal than hormonal secretions and subsistence techniques.

It is only on the assumption that motives, intentions, and the like *are* causes that it is not only important, but crucial to study meanings; crucial in order to offer not just any old interpretation, but rather a valid interpretation, for the actions, customs, and institutions under investigation.

Indeed, that is precisely why, beginning with Sapir (Mandelbaum 1949:part III), psychological anthropology has made the study of meanings, both cultural, as well as noncultural, and unconscious as well as conscious, the centerpiece of anthropological inquiry. For in addition to their representational functions, cultural meaning systems, as D'Andrade has observed, also have three causal properties: they "create cultural entities, direct one to do certain things, and evoke certain feelings" (1984:96–101).

If, however, cultural meanings, as epistemological relativism claims, have no causal relevance, what then *is* their relevance for the understanding of society and culture? And since, under that circumstance, meanings could only be, as behaviorists and cultural materialists claim, epiphenomena, why should they be the focus of inquiry?

Indeed, I would claim that the opposition that the hermeneutic tradition has erected between interpretation and explanation is yet another false dichotomy. For if the interpreter is concerned with formulating a valid interpretation of some particular case, not just any old interpretation, then (as I shall argue below) his idiographic interpretation must be consistent with, if not derived from, a nomothetic (theoretical) explanation which it instantiates.

In any event, since the study of meaning does not entail that anthropology cannot be a scientific discipline, when epistemological relativists claim that a focus on meaning requires that it is, or ought to become, a hermeneutic (interpretive) discipline, that claim is made not by default, but by choice. That choice, I now wish to argue, has adverse consequences for the anthropological enterprise.

A Critique of the Hermeneutic Agenda of Epistemological Relativism

The Subjective Methodology of Hermeneutic Anthropology

According to the hermeneutic tradition, two characteristics in particular, in addition to the differences in their aims, distinguish a hermeneutic from a scientific discipline. First, whereas the former employs insight, imagination, empathy, Verstehen, etc., the latter relies on objective methods of inquiry. Second, because a hermeneutic discipline is interested in the particular, interpretation is always concerned with individual cases, which it addresses in all of their particularity. A scientific discipline, on the other hand, is concerned with principles, laws, theories, and the like, so that even when explanation is applied to individual cases, it addresses them as instances of the general. We may now examine each characteristic separately.

The proposition that the study of man and culture requires a subjective method of inquiry including empathy, intuition, Verstehen, imagination, and the like, rather than the objective methods employed in the study of natural phenomena is a fallacy, one which is based on the confusion of method with techniques (see Rudner 1966:4–7).

A scientific technique consists of any empirical procedure—observation, experimentation, instrumentation, interviewing, and the like—that is employed for obtaining or eliciting data. Now few, surely, would dispute the claim that the techniques that are available to or required by different subjects of inquiry are different one from another. Indeed, seeing as the various disciplines comprising the human sciences themselves employ different techniques—the techniques employed by anthropology, for example, are different from those employed by history or psychology, and they, in turn, differ from each other—it is not to be wondered that the techniques employed by any or all of them may be different from those employed by the physical sciences.

Although it is pluralistic in technique, scientific inquiry is not, however, pluralistic in method. If scientific techniques consist of the empirical procedures employed for obtaining or eliciting data, the scientific method consists of the logic or rationale according to which those data are judged to be evidentially relevant, adequate, or sufficient for the acceptance or rejection of hypotheses, whether explanatory or interpretive.

Attending to that distinction, I shall now argue that when the hermeneutic tradition claims that insight, imagination, empathy, and the like mark a watershed between the human and the physical sciences, it confuses their proper role as techniques in the context of discovery with their improper role as methods in the context of validation.

That insight, imagination, empathy, and the like are indispensable in the human sciences as techniques of inquiry is a proposition, surely, with which almost everyone (except for unregenerate or born again behaviorists) would agree. Indeed, as techniques they are also, with the exception of empathy, indispensable in the physical sciences as well. If not by insight and imagination, how else might an investigator, whatever the subject of inquiry, arrive at a hypothesis—a guess, a hunch, a speculation—regarding the proper explanation or interpretation of his observations and data? Indeed, how else could certain kinds of data be obtained, if not by means of those same techniques? (Though in the latter regard, these subjective techniques play a more important role in the human than in the physical sciences.)

Nevertheless, however indispensable these subjective procedures may be for the formulation of interpretations and explanations, in the scientific mode of inquiry they are entirely disqualified as a method for their validation. It is in the latter context—and in the latter context only—that scientific inquiry requires objective (public and replicable) procedures. That requirement is rejected by the hermeneutic mode of inquiry. According to the latter, empathy, Verstehen, and the like are indispensable not only as techniques in the context of discovery, but also

as methods in the context of validation. From a scientific perspective such a procedure can only be characterized as intellectually irresponsible.

The reason for such a characterization is obvious. Suppose, to take an anthropological example, an ethnographer has formulated an interpretation of a religious rite, a myth, a marriage custom, whatever. He must then decide whether and to what degree his interpretation (hypothesis) is valid. But having already employed his insight and empathic understanding for arriving at an interpretation, to then accept that interpretation because it is empathetically, intuitively, or insightfully grounded is hopelessly circular. In sum, short of a public and replicable method, there is no way by which the validity of the interpretation can be evaluated. However acute his insight or powerful his empathy, when they are employed both as a technique, as well as a method of inquiry, there is no escaping the ''hermeneutic circle,'' as it is called.[11]

In justification of this method hermeneutic anthropology has taken literary criticism as its model. Adopting the metaphor of culture (or society or action) as ''text'' (Ricoeur 1971), any interpretation is said to be a ''reading.'' And since, according at least to one construal of textual analysis, there may be as many readings as there are interpreters, the question of validating any particular reading does not have to be confronted; either because that question is irrelevant, or because it is impossible of resolution. ''Ultimately,'' so the argument goes,

> a good explanation [that is, interpretation] is one which makes sense of the behavior; but then to appreciate a good explanation, one has to agree on what makes good sense; what makes good sense is a function of one's readings; and these in turn are based on the kind of sense one understands. [Taylor 1979:35–36]

That argument, of course, is unexceptionable. Indeed, I would take it much further. Not only does every observer understand and make sense of behavior from his own perspective, but his very perception of that behavior is a function of that perspective. Thus, the ethnographer's theoretical orientation, psychological conflicts, ideological biases, narcissistic investment, personal values, scientific ambitions, cultural background, and many other subjective factors, at least guide (when they do not determine) both his perception, as well as his interpretation, of the behavior.[12]

But that fact which, among others, precludes the possibility of a logic of discovery, is precisely what requires a logic of validation. For if ethnographic interpretations are processed through all those subjective filters,[13] an objective—a public and replicable—method is required for deciding whether an interpretation should be accepted or rejected. The scientific method, which assesses the validity of interpretations by the logical procedure of testing their predictive or retrodictive consequences, constitutes such a method.

If that method or some functional alternative is rejected, and if consequently competing interpretations are merely variant ''readings,'' then anything goes. And if anything—well, almost anything—goes, a scholarly discipline is not intellectually responsible.

It might be argued that in large measure that is the way that, in fact, it has usually been. But to adopt the hermeneutic method is to ensure that, in principle, it is also the way it must always be. For while the boundary in all of the social sciences between theory and ideology, fact and value, objective and subjective, evidential constraint and personal preference is often blurred, the hermeneutic method guarantees that those boundaries will never be clear.

The Ethnographic Particularism of Hermeneutic Anthropology

An anthropology which abdicates the search for explanatory theories of culture and society in favor of particularistic interpretations of specific cultures and societies exclusively is an anthropology whose attraction will (in my opinion) become confined to scholars whose intellectual curiosity is limited to, and whose intellectual appetite is nourished by, strange customs of exotic peoples. For the rest—and from the increasing grumblings one hears at the annual meetings of the American Anthropological Association, that would seem to include more than a few—that aim produces (if I may be forgiven, a neologism) anorexia curiosa; in a word, boredom.

Now I have no doubt that as a consequence of our traditional focus on social and cultural particularities "we have with no little success sought to keep the world off balance; pulling out rugs, upsetting tea tables, setting off firecrackers" (Geertz 1984:275). Nor can it be doubted that "it has been the office of others to reassure; ours to unsettle. . . . We hawk the anomalous, peddle the strange. Merchants of Astonishment" (Geertz 1984:275).

The problem, however, is that nothing fails like success. Having already documented the entire range of cultural differences, we no longer astonish, the strange is jejune, there are no anomalies. Hence, if even today, some fifty years after Malinowski and one hundred after Tylor, not to mention twenty five hundred after Herodotus, our aim is to document the fact of cultural variability (without ever, however, explaining it) and to offer interpretive accounts of particular cultures—but never, however, explanatory accounts of culture—such an aim may be of value on many counts, but intellectually it has become trivial.[14]

That judgment is perhaps best supported by contrasting that hermeneutic aim, the intellectual child of epistemological relativism, with the aims that informed ethnographic inquiry during the hegemony of descriptive and normative relativism.

When cultural relativism was confined to the descriptive type—not, however, in its contemporary (strong) form, but its original (moderate) form—ethnographic inquiries were harnessed to the idea of progress. Thus the purpose for studying all manner of societies, including and most especially primitive societies, was to document the stages, and to discover the processes of unilineal cultural and social evolution. That enterprise may have been (as most, but not all, anthropologists believe it to be) abortive, but few would deny that its aim was scientifically important, or that it conferred intellectual importance on the study of primitive peoples.

When, however, the idea of progress, together with the unilineal stage theory of cultural evolution, was discarded, and when concomitantly cultural relativism came to include the normative type, the aim of ethnographic inquiry and the rationale for concentrating on primitive peoples were transformed. Because the diversity of cultures was now viewed as comprising not a scale of cultural evolution, but rather the range of cultural variability, its aim was taken to be the discovery of principles and theories that might explain the diversity in cultural and social systems.

With that change in aim, the rationale for focusing on primitive peoples correspondingly changed. No longer viewed as similar to one another—each being an instance of an archaic type of society—they were now chosen for study precisely because (as Margaret Mead observed) they were viewed as different, each different from every other, and all, in turn, from Western peoples. Hence, since every culture was now conceived in Ruth Benedict's famous metaphor as one "segment" of "a great arc" (Benedict 1934:24) and since the other social sciences studied only one of those segments (Western), the unique function of anthropology, and its rationale for focusing on primitive cultures, was to fill in the rest of—that is, most of—that "great arc." It was no accident that Ralph Linton, writing at the zenith of that period, titled his book, not *The Primitive World,* but *"Most of the World"* (Linton 1949).

Given, then, that this was the era of Boasian normative relativism, which viewed all cultures as equally valuable, and given, too, that every society was conceived, as the saying went, as an "experiment of nature," ethnographic studies of primitive peoples were viewed as contributing not only to a substantive (particularistic) aim, but also to a scientific (theoretical) aim. That is, they were viewed as a means not only for expanding our knowledge of the range of social and cultural diversity, but also for arriving at valid theories and principles that might explain society and culture.

The latter aim, it was believed, could not be achieved by the study of Western culture alone because the latter represents only one segment of the arc of culture. To express it in the cultures-as-natural experiments metaphor, primitive cultures were viewed vis à vis Western culture as experimental control groups.

To be sure, given the philosophy of science of that era—radical empiricism and naive inductivism—it was believed that only when the descriptive and classificatory aims of ethnographic inquiry were completed—"when all the facts are in"—could that scientific aim be accomplished (Boas 1940:243–289). In the meantime, the typical anthropological response to the typical generalization in sociology or economics or psychoanalysis—"But the Hopi . . ." or "But the Kwakiutl . . ."—was intended to hold up a mirror not only for man (Kluckhohn 1957), but also for the other social sciences whose theories of society and culture were essentially based on a sample of one.

Admittedly, in the execution of that inductively conceived scientific aim, many anthropologists, concerned as they were with the immediate task of ethnographic description and classification, lost sight of the forest for the trees. Nevertheless, a surprisingly large number never lost sight of that aim, and for

them ethnographic inquiry was more often than not theoretical in aim and comparative (if only implicitly) in method. Thus, whether fieldwork was conducted among the Trobrianders, Samoans, Navajo, Nuer, or Saulteaux (to take some obvious examples), it was in the service of some general theory of ritual exchange (Malinowski 1961 [1922]), adolescence (Mead 1928), witchcraft (Kluckhohn 1967 [1944]), segmentary lineage systems (Evans-Pritchard 1940), or cultural psychology (Hallowell 1955) respectively.

It should be emphasized, moreover, that many of these studies were, in the best sense of the word, interpretive. Beliefs and customs were described in their full ethnographic context, and they were interpreted (as Malinowski put it) from the "natives' point of view."

Nevertheless, for the influential anthropologists just mentioned the interpretive understanding of the cultures of particular tribal groups was only the first step—an intellectual way station—to the theoretical understanding of cultural systems. For them it was axiomatic that, conceived as a science, anthropology transcended the ethnoscience of any and all cultures—including Western culture—and that, consequently, it was capable both in principle and in fact of discovering theories that might explain the social and cultural systems of any society—including, once again, Western society.

It is by contrast with the latter two aims that, in my judgment, ethnographic particularism is intellectually trivial. For if, in principle, ethnographic studies cannot contribute to the formulation of nontrivial explanatory theories of society and culture, nor even to the discovery of nonvacuous empirical generalizations about them; if, on the contrary, they can only contribute to unique understandings of this or that belief or custom of this or that primitive or peasant culture in all of its particularity, what possible intellectual relevance might such studies have?

Put differently, if cultures are texts and the aim of ethnographic inquiry is interpretation, then in choosing *our* texts, we ought to adopt the criteria that literary critics employ for choosing *theirs*. Rather than wasting their time on any old text, literary critics, in contrast to book reviewers, devote their energies to the interpretation of exemplary ones—not, for example, the novels of Jack Kerouac and Herman Wouk, but those of Jane Austen and Dostoevsky. An interpretive understanding of the latter texts is intellectually important because, being exemplary, they speak to issues of general, sometimes perennial, human concern.

Analogously, if interpretation is our aim, then we should choose exemplary cultures—cultures whose interpretation can contribute to an understanding of the "great arc" of culture. But if, as epistemological relativism claims, the incommensurability of cultures precludes in principle the very possibility of such a contribution, then it is indeed difficult to discern how the particularistic interpretation of this or that belief or custom in this or that primitive or peasant culture might be of intellectual importance.

If, now, this judgment were idiosyncratic, or if, though widely shared, it had little practical import for the status of anthropology, it could be ignored as the expression of one scholar's intellectual preference. Unfortunately, it is neither the

one nor the other; that judgment is widely shared and it does have such import, as I now wish to indicate by reference to three warning signals.

Item: Recently, the National Research Council (the research arm of the National Academy of Sciences) constituted a blue ribbon committee to evaluate the "status of the behavioral and social sciences." The committee identified 32 research areas in which, according to a survey it conducted among leading researchers, the most important "basic research" in those disciplines is currently being pursued. Of the 32, only 3 might be considered as having some degree of anthropological input. If that were not bad enough, of the 75 persons appointed by the committee to chair or cochair subcommittees to evaluate research in those areas, only 2 are anthropologists. It would appear that contemporary anthropological research is not viewed by distinguished colleagues in the social and behavioral sciences as having much to contribute to "basic research."

Just as distressing, however, is the fact that while "culture and ideology" was identified as an area of basic research—which might be taken as a favorable sign—all the members (including the chairman) of the subcommittee appointed for its evaluation are anthropologists, in contrast to almost all the other subcommittees whose members represent a range of disciplines. In short, anthropologists are viewed or view themselves as inhabiting a kind of intellectual ghetto whose main resource—culture and ideology—has little to contribute to the intellectual capital of the collective social science enterprise.

Item: Recently, that same body (the National Research Council) appointed another blue ribbon committee to evaluate the status of black Americans. The original roster of that committee did not include even one anthropologist. When a protest was registered, it was dismissed on the grounds that anthropological studies of race relations are typically conducted overseas. But, it was countered, it is precisely for that reason that anthropological representation is important: the anthropologist can bring to the work of the committee a crucial comparative perspective. Ironically enough, that argument was dismissed on the very grounds adduced by epistemological relativists for denying that anthropology is a scientific discipline: every culture being unique, the study of race relations in non-Western contexts can contribute little to the understanding of race relations in the United States. (As it happens, under pressure, a highly accomplished anthropologist was eventually appointed to the committee.)

Item: It is an open secret that the National Institutes of Health (NIH), formerly the most important source of funding for sociocultural anthropology, has all but phased out its support for foreign area research, although anthropologists are still funded for research in the United States. In case there is any doubt regarding the reason for this policy change, consider the following colloquy between an NIH review committee and a department of anthropology whose training grant on culture and mental health the committee declined to renew on the grounds that training and research on non-Western societies—the focus of the program—would not contribute to an understanding of mental health problems confronting American society.

The counter argument of the Department—that the validity of any theory of mental health or mental illness can only be evaluated by evidence from a variety of different sociocultural contexts—was simply not grasped, either by the NIH staff or by its social science advisory panel. Research in non-Western societies might be relevant (their argument went) for understanding mental health and illness in the latter societies, but inasmuch as it has no relevance for the understanding of mental health and illness in the United States, it cannot be supported by a governmental agency whose mission, mandated by Congress, is the welfare of American society.

Thus it is that an unchallenged axiom of only a generation ago—the axiom that comparative ethnographic research is indispensable for the advance of knowledge in the human sciences—is now rejected as false. And it would be a transparent rationalization to attribute that change solely to an increasing parochialization of American social science. For if a large and distinguished group of anthropologists together with other social scientists claim that ethnographic research has no generalizability and that cross-cultural theories are, in principle, impossible to attain; and if, moreover, the bulk of current ethnographic research instantiates that claim, we can hardly expect nonanthropologists to accept as axiomatic a proposition that anthropologists, themselves, reject both in principle and in practice.

Recall that when, beginning in the 1930s, that axiom was regnant, it was hardly by accident. Rather, it was because a distinguished group of anthropologists—Sapir and Benedict, Murdock and Hallowell, Mead and Kluckhohn, Whiting and Linton, Eggan and Redfield, to name only a few—preached and practiced an anthropological agenda that would exploit the fact of cross-cultural diversity for the creation of a comparative "science of man." Moreover, some members of that group took the lead in institutionalizing that agenda at three prestigious universities—Yale (in the Institute of Human Relations), Harvard (in the Department of Social Relations), and Chicago (in the Committee on Human Development).

That combination of distinguished scholars and prestigious universities had a powerful influence on the creation of a climate of opinion, both within and outside of anthropology, that would acknowledge the importance of ethnographic knowledge as one of the cornerstones for human understanding. With the death of Margaret Mead, the most distinguished and tireless public advocate (popularizer) of that view and in the absence of a contemporary advocate of equivalent stature, ethnography is again perceived as the study of strange customs of exotic peoples. Although, one hears it said, it may be important for our government to know about such peoples during times of international crises—when Miskitos or Shi'as or Hmong, though hardly household words, are yet pertinent for the national interest—the study of their customs is not the stuff from which a science of man is to be constructed. It would seem that Kroeber's warning, 66 years ago, has been confirmed:

> As long as we continue offering the world only reconstructions of specific detail, and consistently show a negative attitude toward broader conclusions, the world will find very little profit in ethnology. [Kroeber 1920:380]

In conclusion, if we continue to pursue the aim of ethnographic particularism, it would be better, if only for our enlightened self interest, to focus our studies on Western culture. Short of that, we have one other alternative. We can adopt or return to a generalizing-explanatory mode of ethnographic research, while focusing on the comparative study of non-Western cultures. Although either alternative would assure our vitality as a discipline, from the foregoing discussion my own preference should be obvious.

Notes

Acknowledgments: This paper is an expansion of a discussion paper presented to the Symposium on Ethical Relativism at the 1984 annual meeting of the American Anthropological Association. I wish to express my appreciation to Alan Fiske for his invitation to serve as a discussant, and to the participants on the Symposium—Caroline Edwards, James Fernandez, Alan Fiske, Robert LeVine, Joan Miller, Brad Shore, Richard Shweder, and David Wong—whose papers stimulated the present effort. I wish especially to acknowledge the helpful criticisms of Aaron Cicourel, Roy D'Andrade, A. K. Epstein, Michael Meeker, Robert Paul, Fitz John Porter Poole, Richard Shweder, Marc Swartz, Donald Tuzin, and Aram Yengoyan.

[1]It will be noted that the judgment that I have designated as "descriptive cultural relativism" must be distinguished from the judgment—which might be designated as "contextual relativism"—that the meaning or significance of some social or psychological variable is relative to (depends on) the total context in which it is embedded.

[2]In the published version of Rosaldo's paper, which was first presented and discussed at the Conference on Culture Theory, this passage was changed to "culture *makes a difference that concerns not simply* what we think but how we feel about and live our lives" (Rosaldo 1984:140, changes italicized). Because the published version is not only inconsistent with the main thrust of Rosaldo's paper, but is one to which any opponent of cultural relativism would assent, I have quoted the original version as a representative expression of the strong form of descriptive relativism.

[3]For a detailed comparison of these two theories of cultural determinism see Spiro 1978:350–358.

[4]This culturally grounded notion of "Otherness" is to be distinguished from the politically grounded notion according to which the Third World Other is a construction of the Western Self in the service of colonial and other forms of political domination (Dwyer 1982:253–287). Since, however, that notion takes us out of the realm of theoretical and into the realm of ideological discourse, I shall not address it in this paper.

[5]The sweeping claim that social science theories merely reflect Western ethnoscience would have to be qualified in respect, at least, to some of the most influential ones. The Darwinian, Marxian, and Freudian theories, for example, were, and are, rejected by scientists and laymen alike on the grounds that they violate common sense or are counterintuitive, and the like; in short, because they contradict the prevailing ethnoscientific theories. Conversely, those who have accepted them have done so in spite of the fact that they were, or are, in conflict with prevailing ethnoscientific theories. Moreover, if that sweeping

claim is correct, why is it that those three theories—among others—have found acceptance by non-Western social scientists when they clearly contradict their *own* ethnoscientific theories?

[6]Substantively—but not necessarily methodologically—this dichotomy parallels the contemporary American distinction between the physical sciences, on the one hand, and the social sciences and humanities, on the other. Originating in mid-19th-century German philosophy, it achieved prominence among contemporary social scientists as a result, most notably, of the influence of Wilhelm Dilthey (1833–1911). For an explication of Dilthey's views of the *Geisteswissenschaften,* Makkreel (1975) is an excellent introduction.

[7]Needless to say, these tenets were part of an intellectual *zeitgeist* which both influenced the rise of symbolic anthropology and was, in turn, influenced by it. Among the influences, I would only mention such obvious names—each for a somewhat different reason—as Cassirer, Langer, and Wittgenstein in philosophy, Garfinkel, Goffman, and Schutz in sociology, Saussure and Whorf in linguistics. For a sample of the work and thought of the first generation of symbolic anthropologists, see Basso and Selby 1976, Dolgin, Kemnitzer and Schneider 1977, Spencer 1969.

[8]When Renato Rosaldo, following a remarkably courageous act of insight, came to understand that his own feelings of grief and anger were identical with those of the Ilongot—a view that he had rejected in his previously published work on the Ilongot—he characterized the theory of psychological incommensurability (to which he had formerly subscribed) as the "pernicious doctrine that, my own group aside, everything human is alien to me" (R. Rosaldo 1984:188).

[9]Compare Putnam's critique of the relativistic views of Kuhn and Feyerabend regarding the history of western science (1981:114–115).

[10]It is not always easy to stipulate what hermeneutic anthropologists understand by "meaning" because that term is all too often obscure, if not opaque, in their work.

[11]Contemporary hermeneutic interpreters of psychoanalysis usually emphasize, with Ricoeur (1970), that the title of Freud's most important work is "The Interpretation of Dreams," and that (with some few excursions) its title accurately reflects its focus. True enough. But that claim overlooks the fact that the dream interpretations proposed in that book are derived from a general theory, a theory which purports to offer a causal explanation of dreams and dreaming.

[12]It is because self-conscious awareness of one's own biases is an important means for controlling for (if not reducing) perceptual distortions and the like that some anthropologists (Bateson, Kluckhohn, LaBarre, and Mead, among others) have recommended that ethnographers undergo a personal analysis.

[13]The most important of these filters derive perhaps from those unconscious anxieties aroused in the ethnographer in his encounter with an alien society. Since in this regard the emotional situation of the ethnographer is analogous to that of the psychoanalyst in his encounter with a patient, Devereux (1967) refers to these distortions as "counter-transference" reactions. Since Devereux's is perhaps the most trenchant analysis of the subjective factors in observer bias in the social sciences (including anthropology), it is regrettable that his book is little known, or at least infrequently cited in the growing literature on this topic.

[14]That judgment would also be shared by Max Weber, although, ironically, that towering figure is uniformly claimed by hermeneutic social scientists as a central actor and founding

father. Although Weber was a leading proponent and theorist of Verstehen, his agenda and work, alike, are otherwise in dramatic opposition to the current hermeneutic approach. Hence, it is perhaps desirable at this juncture to place him in respect to the argument of this paper.

First, while for Weber Verstehen was a crucial technique in sociocultural inquiry, he explicitly abjured its use as a method. For him, hypotheses formulated in respect to one culture or society, though arrived at by means of Verstehen cannot be accepted until they are tested in other sociocultural settings whose characteristics are evidentially relevant. And this methodological injunction was brilliantly implemented in his own work as, for example, in his famous thesis concerning the relationship between Calvinism and the rise of capitalism (Weber 1930), a magisterial undertaking involving ancient Israel (Weber 1952), India (Weber 1958), and China (Weber 1951).

Second, although stressing the actors' point of view, Weber explicitly rejected the epistemological relativism of hermeneutic social science. Denying that different historical groups are incommensurable and that consequently generalizing theoretical categories are impossible to achieve, he argued instead for the very opposite tenets, both in his theoretical statements (Weber 1957:87–123) and in their implementation in the comparative studies just mentioned.

Third, while he emphasized that the social sciences differed from the physical sciences in their need to attend to meanings, Weber was especially and explicitly concerned with meanings because of their causal relevance, which, of course, is his main thesis in his comparative study of the ''economic ethics'' of the great religions mentioned above. Finally, he insisted (as Parsons [1937:ch. 16] has shown in detail) that despite their differences in respect to techniques, the social and physical sciences must employ the same methodology: ''logical schema.'' That, now, Weber should be claimed by the interpretive social sciences as a founding father is rather puzzling.

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