



The Thick and the Thin: On the Interpretive Theoretical Program of Clifford Geertz [and Comments and Reply]

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Current Anthropology, Vol. 25, No. 3. (Jun., 1984), pp. 261-280.

Stable URL:

<http://links.jstor.org/sici?sici=0011-3204%28198406%2925%3A3%3C261%3ATTATTO%3E2.0.CO%3B2-2>

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The Thick and the Thin: On the Interpretive Theoretical Program of Clifford Geertz¹

by Paul Shankman

IN A RECENT ASSESSMENT OF CLIFFORD GEERTZ'S contribution to anthropology, Peacock (1981:122-23) has stated:

Regardless of one's view of Geertz's scholarly work, one must accept that he occupies a critical place in the discipline. He is of strategic importance in the rebirth of an American cultural anthropology which by the death of Kluckhohn and Kroeber had already entered a dark age symptomized by excessive devotion to certain narrowly technical pursuits and a failure of nerve among those heirs of Boas who aspired to sustain the endeavor of a holistic and humanistic perspective. Without Geertz—or someone like him—the birthright of Boas, Kroeber, and others in the American tradition seemingly would have been sold for thin porridge.

While some may wish to qualify this assessment, there remains little doubt about Geertz's influence on the discipline. Beyond his contribution to anthropology, Geertz has become an interdisciplinary figure and a major presence at the interface of the social sciences and the humanities. His articulate intellectual program and his ethnographic studies have found a wide and growing audience.

The programmatic side of Geertz's work is an attempt to refocus anthropology—indeed all of social science—away from

¹ This paper was presented at a Seminar for the Committee on the History and Philosophy of Science at the University of Colorado, Boulder, in February 1982. Some funds for the research were provided by the Bureau of Anthropological Research, University of Colorado, Boulder. I would like to thank the following people for their assistance and comments on this paper: Ray Cuzzort, Walter DiMantova, Jack Kelso, Dennis McGilvray, Terry Odendahl, Robert Pois, William B. Taylor, and Andrew Weissman. Needless to say, they are in no way responsible for its content.

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the emulation of the natural sciences and toward a reintegration with the humanities. Geertz has proposed that social scientists study meaning rather than behavior, seek understanding rather than causal laws, and reject mechanistic explanations of the natural-science variety in favor of interpretive explanations. He has invited his colleagues to take seriously the possibilities of analogy and metaphor, to consider human activity as text and symbolic action as drama. In other words, he has asked social scientists to rework, if not abandon, their traditional assumptions about the nature of their intellectual enterprise.

This essay will explore the program that Geertz has proposed for a new, interpretive social science. Geertz's program has emerged gradually over the last two decades and is most apparent in two articles: "Thick Description: Toward an Interpretive Theory of Culture" (1973a) and "Blurred Genres: The Refiguration of Social Thought" (1980a). It is not a program explicated in all of Geertz's work and is not necessarily adhered to in each of his publications. Nor is it always consistent. Nevertheless, it constitutes a significant and systematic effort to rethink many of the basic premises of social science. It may be useful, then, to examine this program in its abstract, theoretical formulation, to identify its strengths and weaknesses, and to consider its prospects. This essay offers such an examination and assessment.

FROM THICK DESCRIPTION TO THE REFIGURATION OF SOCIAL THOUGHT

Geertz's basic premise for a different anthropology, and hence a different social science, begins with a reconsideration of the concept of culture. In "Thick Description," as elsewhere, Geertz affirms that culture is symbolic and meaningful, involving neither behavior nor social action directly: "The concept of culture I espouse . . . is essentially a semiotic one. Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning" (1973a:5). It is this concept of culture that is, for Geertz, the key to the analysis of cultures, each of which has its own unique configuration. "Analysis is the sorting out of structures of signification and determining their social ground and import" (p.

9). Through analysis, ethnography becomes “thick description,” and it is in the realities of fieldwork that thick description is based. Geertz explains (p. 10):

What the ethnographer is in fact faced with—except when (as, of course, he must do) he is pursuing the more automatized routines of data collection—is a multiplicity of complex conceptual structures, many of them superimposed upon or knotted into one another, which are at once strange, irregular, and inexplicit, and which he must contrive somehow first to grasp and then to render. And this is true at the most down-to-earth, jungle field work levels of his activity: interviewing informants, observing rituals, eliciting kin terms, tracing property lines, censusing households . . . writing his journal. Doing ethnography is like trying to read (in the sense of “construct a reading of”) a manuscript—foreign, faded, full of ellipses, incoherencies, suspicious emendations, and tendentious commentaries, but written not in conventionalized graphs of sound but in transient examples of shaped behavior.

Asserting that “culture is context,” Geertz argues that we must attempt to grasp it from the native point of view; “our formulations of other people’s symbol systems must be actor-oriented” (p. 14). This does not mean, however, resorting to ethnoscience, componential analysis, or cognitive anthropology, in which “extreme subjectivism is married to extreme formalism” (p. 11). In contrast to approaches that yield the illusion of operationalism and objectivity, Geertz’s approach acknowledges that its descriptions of other cultures are *already* interpretations of their interpretations; “*we begin our own interpretations of what our informants are up to, or think they are up to, and then systematize those.* . . . In short, anthropological writings are themselves interpretations and second and third order ones to boot” (p. 15). Geertz allows that this creates a “delicate” situation but on balance finds the risks worth taking. “Cultural analysis is (or should be) guessing at meaning, assessing the guesses, and drawing explanatory conclusions from the better guesses, not discovering the Continent of Meaning and mapping out its bodiless landscape” (p. 20).

Geertz also favors a “microscopic” approach to ethnographic description. It is not that large-scale interpretations are impossible, but rather that anthropologists tend to work in “obscure” places and that data tend to be drawn from “exceedingly extended acquaintances with extremely small matters” (p. 21). Generalizations are possible, according to Geertz, because “social actions are comments on more than themselves; . . . where an interpretation comes from does not determine where it can be impelled to go. Small facts speak to large issues . . . because they are made to” (p. 23). At the same time, these small facts constrain generalities. “What generality [thick description] contrives to achieve grows out of the delicacy of its distinctions, not the sweep of its abstractions” (p. 25).

This brief summary of Geertz’s “thick description” hardly does justice to his exposition, but in its broad outlines it introduces a programmatic synthesis that is important not only because it contains Geertz’s vision of what cultural anthropology should be but also because it forms the basis of a broader vision—the vision of a major intellectual movement based on the analysis of meaningful forms linking the social sciences and the humanities (p. 29). In his introduction to the interdisciplinary volume *Myth, Symbol, and Culture* (1971: x–xi), Geertz catches a glimpse of this movement:

What, dimly perceived, these assorted enterprises seem to have in common is a conviction that meaningful forms, whether they be African passage rites, nineteenth-century novels, revolutionary ideologies, grammatical paradigms, scientific theories, English landscape paintings, or the way in which moral judgments are phrased, have as good a claim to public existence as horses, stones, and trees, and are therefore as susceptible to objective investigation and systematic analysis as these apparently harder realities.

Everything from modern logic, computer technology, and cybernetics at one extreme to phenomenological criticism, psychohistory, and ordinary language philosophy at the other has conspired to undermine the notion that meaning is so radically “in the head,” so deeply subjective, that it is incapable of being firmly grasped, much less

analyzed. It may be supremely difficult to deal with such structures of meaning, but they are neither a miracle nor a mirage. Indeed, constructing concepts and methods to deal with them and to produce generalizations about them is the primary intellectual task now facing those humanists and social scientists not content merely to exercise habitual skills. The surge of interest in “myth,” “fiction,” “archetype,” “semantics,” “systems of relevance,” “language games,” and so on is but the symptom that this transformation in viewpoint has in fact taken place, and—from the very multiplicity of the terms—that it has taken place in intellectual contexts much more isolated from one another than the commonality of their concerns would warrant.

In “Blurred Genres” Geertz moves beyond the dim apprehension of intellectual convergence in the social sciences and the humanities to the conviction that this convergence presages a “refiguration of social thought” (1980a:165):

Certain truths about the social sciences today seem self-evident. One is that in recent years there has been an enormous amount of genre mixing in social science, as in intellectual life generally, and such blurring of kinds is continuing apace. Another is that many social scientists have turned away from a laws-and-instances ideal of explanation toward a cases-and-interpretations one, looking less for the sort of thing that connects planets and pendulums and more for the sort that connects chrysanthemums and swords. Yet another truth is that analogies drawn from the humanities are coming to play the kind of role in sociological understanding that analogies drawn from the crafts and technology have long played in physical understanding. I not only think these things are true, I think they are true together; and the culture shift that makes them so is the subject of this essay: the refiguration of social thought.

Geertz finds that the movement is growing to “formidable proportions” and that interpretive explanation is a legitimate alternative to mainstream social science (p. 167):

. . . the move toward conceiving of social life as organized in terms of symbols (signs, representations, *signifiants*, *Darstellungen* . . . the terminology varies), whose meaning (sense, import, *signification*, *Bedeutung* . . .) we must grasp if we are to understand that organization and formulate its principles, has grown by now to formidable proportions. The woods are full of eager interpreters.

Interpretive explanation—and it is a form of explanation, not just exalted glossography—trains its attention on what institutions, actions, images, utterances, events, customs, all the usual objects of social-scientific interest, mean to those whose institutions, actions, customs, and so on they are. As a result, it issues not in laws like Boyle’s, or forces like Volta’s, or mechanisms like Darwin’s, but in constructions like Burckhardt’s, Weber’s, or Freud’s: systematic unpackings of the conceptual world in which *condottiere*, Calvinists, or paranoids live.

What the social sciences need to revitalize them, according to Geertz, are new analogies. Mechanistic analogies from the natural sciences and engineering should give way to more fruitful ones “familiar to gamesters and aestheticians” (p. 168). “In the social sciences, or at least in those that have abandoned a reductionist conception of what they are about, the analogies are coming more and more from the contrivances of cultural performance than from those of physical manipulation—from theater, painting, grammar, literature, law, play. What the lever did for physics, the chess move promises to do for sociology.” In summing up the thrust of the new movement, Geertz numbers the days of consensus social science and portends a sea change in social thought (p. 178):

. . . however raggedly, a challenge is being mounted to some of the central assumptions of mainstream social science. The strict separation of theory and data, the “brute fact” idea; the effort to create a formal vocabulary of analysis purged of all subjective reference, the “ideal language” idea; and the claim to moral neutrality and the Olympian view, the “God’s truth” idea—none of these can prosper when explanation comes to be regarded as a matter of connecting action to its sense rather than behavior to its determinants. The refiguration of social theory represents, or will if it continues, a sea change in our notion not so much of what knowledge is, but of what it is we want to know. Social events do have causes and social institutions effects; but it just may be that the road to discovering what we assert in asserting this lies less through postulating forces and measuring them than through noting expressions and inspecting them.

There can be no doubt that the intellectual ferment that Geertz has discussed and advocated is occurring. In cultural anthropology, symbolic approaches including, but not confined to, Geertz's work now represent a major school among the younger generation. Yet it must be asked where this new social science is taking us. What are the strengths and weaknesses of Geertz's interpretive social science? And are social scientists ready to make the major paradigm shift that Geertz calls for?

INTERPRETIVE THEORY: SIC ET NON

Geertz's essay on "thick description" is subtitled "Toward an Interpretive Theory of Culture." Because Geertz continually contrasts the rich, thick, textured qualities of interpretive theory with the worn, threadbare qualities of a mechanistic, reductionist social science, it is important to examine what kind of theory Geertz is talking about. Geertz is explicit in calling interpretive theory a science, but it is a science with a difference.

For example, while Geertz allows that interpretive explanation should somehow "fit" realities past and future, he acknowledges that interpretive explanation "is not, at least in the strict meaning of the term, 'predictive'" (1973a:26). Nor is it verifiable. As he candidly remarks (p. 24):

The besetting sin of interpretive approaches to anything—literature, dreams, symptoms, culture—is that they tend to resist, or to be permitted to resist, conceptual articulation and thus to escape systematic modes of assessment. You either grasp an interpretation or you do not, see the point of it or you do not, accept it or you do not. Imprisoned in the immediacy of its own detail, it is presented as self-validating, or, worse, as validated by the supposedly developed sensitivities of the person who presents it; any attempt to cast what it says in terms other than its own is regarded as a travesty—as the anthropologists' severest term of moral abuse, ethnocentric. For a field of study which, however timidly (though I, myself, am not timid about the matter at all), asserts itself to be a science, this just will not do. There is no reason why the conceptual structure of a cultural interpretation should be any less formulable, and thus less susceptible to explicit canons of appraisal, than that of, say, a biological observation or a physical experiment—no reason except that the terms in which such formulations can be cast are, if not wholly nonexistent, very nearly so. We are reduced to insinuating theories because we lack the power to state them.

Interpretive theory, Geertz readily allows, lacks precise criteria for evaluating cultural interpretations. How, then, does one assess an interpretation? According to Geertz, "a good interpretation of anything—a poem, a person, a history, a ritual, an institution, a society—takes us into the heart of that of which it is the interpretation" (1973a:18). By implication, there are "bad" interpretations that do not take us to the heart of the matter, but when it comes to elucidating what is good or bad or how one discerns the heart of the matter, Geertz provides few guidelines. Further, he concedes that interpretive theory has no means for evaluating alternative accounts of the same phenomenon: "This raises some serious problems of verification, all right—or, if 'verification' is too strong a word for so soft a science (I, myself, would prefer the word 'appraisal'), of how you can tell a better account from a worse one. But that is precisely the virtue of it" (p. 16). Whether or not this is a "virtue," it does help us to understand why advances in interpretive theory are so difficult to comprehend. The criteria for assessment are not clearly defined. Geertz states that "a study is an advance if it is more incisive—whatever that may mean—than those that preceded it" (p. 25). Precisely what is meant here seems at best unclear and at least a bit obscure.

Geertz discusses other characteristics of cultural interpretation that "make the theoretical development of it more than usually difficult" (p. 24):

The first is the need for theory to stay rather closer to the ground than tends to be the case in the sciences more able to give themselves over to imaginative abstraction. . . .

The whole point of a semiotic approach to culture is, as I have said,

to aid us in gaining access to the conceptual world in which our subjects live so that we can, in some extended sense of the term, converse with them. The tension between the pull of this need to penetrate an unfamiliar universe of symbolic action and the requirements of technical advance in theory of culture, between the need to grasp and the need to analyze, is, as a result, both necessarily great and essentially irremovable. Indeed, the further theoretical development goes, the deeper the tension gets.

The difficulty here, and Geertz appreciates it, is that explanation and description, distinct in conventional science, become almost indistinguishable. Geertz, however, does not see this blurring as a problem, for he views the distinction between description and explanation as "relative in any case" (p. 27). But if description and explanation are so tightly intertwined as to be hardly distinguishable, what kind of generalizations can interpretive theory offer?

Generalization and comparison are vital components of science; indeed, they permeate some of Geertz's own empirical work. Yet in his programmatic essays, Geertz contends that "the essential task of theory building here is not to codify abstract regularities but to make thick description possible, not to generalize across cases, but to generalize within them" (p. 26). His rationale for a noncomparative approach (p. 26) is as follows:

To generalize within cases is usually called, at least in medicine and depth psychology, clinical inference. Rather than beginning with a set of observations and attempting to subsume them under a governing law, such inference begins with a set of (presumptive) signifiers and attempts to place them within an intelligible frame. Measures are matched to theoretical predictions, but symptoms (even when they are measured) are scanned for theoretical peculiarities—that is, they are diagnosed. In the study of culture the signifiers are not symptoms or clusters of symptoms, but symbolic acts or clusters of symbolic acts, and the aim is not therapy but the analysis of social discourse.

If there is no generalization across cases, then how does Geertzian theory proceed in terms of cumulative knowledge? It does not, according to Geertz (p. 25):

. . . as a simple matter of empirical fact, our knowledge of culture . . . cultures . . . a culture . . . grows: in spurts. Rather than following a rising curve of cumulative findings, cultural analysis breaks up into a disconnected yet coherent sequence of bolder and bolder sorties. Studies do build on other studies, not in the sense that they take up where the others leave off, but in the sense that, better informed and better conceptualized, they plunge more deeply into the same thing. Every serious cultural analysis starts from a sheer beginning and ends where it manages to get before exhausting its intellectual impulse. Previously discovered facts are mobilized, previously developed concepts used, previously formulated hypotheses tried out; but the movement is not from already proven theorems to newly proven ones, it is from an awkward fumbling for the most elementary understanding to a supported claim that one has achieved that and surpassed it.

All in all, though, interpretive theory does not yield much in the way of theoretical formulations. About this Geertz is quite candid: "Theoretical formulations hover so low over the interpretations they govern that they don't make much sense or hold much interest apart from them. That is so, not because they are not general (if they are not general, they are not theoretical), but because, stated independently of their applications, they seem commonplace or vacant" (1973a:25). The result is what Geertz calls "nook-and-cranny" anthropology, an interpretive anthropology grounded in particular cases (Geertz 1979:1–2).

THE "STRANGE SCIENCE" OF "NOOK-AND-CRANNY" ANTHROPOLOGY

Geertz's candor about the weaknesses of interpretive theory provides an interesting counterpoint to his effusiveness about

the possibility of interpretation. At times, Geertz is quite guarded about the practice of interpretation. His uncertainty about the interpretive enterprise and his sensitivity to the charges of subjectivism and intellectual chic lead him to warn (1973a:30) that:

Nothing will discredit a semiotic approach to culture more quickly than allowing it to drift into a combination of intuitionism and alchemy, no matter how elegantly the intuitions are expressed or how modern the alchemy is made to look.

The danger that cultural analysis, in search of all-too-deep-lying turtles, will lose touch with the hard surfaces of life—with the political, economic, stratificatory realities within which men are everywhere contained—and with the biological and physical necessities on which those surfaces rest, is an ever-present one. The only defense against it, and against, thus, turning cultural analysis into a kind of sociological aestheticism, is to train such analysis on such realities and such necessities in the first place.

Yet even when dealing with such “realities and necessities,” Geertz points to the limitations of what he terms “a strange science”: “Cultural analysis is intrinsically incomplete. And, worse than that, the more deeply it goes the less complete it is. It is a strange science whose most telling assertions are its most tremulously based, in which to get somewhere with the matter at hand is to intensify the suspicion, both your own and that of others, that you are not quite getting it right” (p. 29).

These limitations are present in interpretive theory not simply because it lacks predictability, replicability, verifiability, and law-generating capacity. They are present, in large part, because of the assumptions of interpretive theory concerning its aims, the nature of theory, and the role of objectivity. These assumptions almost preclude the clarity and specificity that would make Geertz’s “strange science” more “complete” and more plausible.

When Geertz discusses the aims and nature of interpretive theory, he seems more interested in possibility than in tangibility. Thus the primary aim of anthropology is “the enlargement of the universe of human discourse” (p. 14), and the essential task of theory building is “to make thick description possible” (p. 27): “In ethnography, the office of theory is to provide a vocabulary in which what symbolic action has to say about itself—that is about the role of culture in human life—can be expressed.” The very nature of the aims and theory in the interpretive approach yields a notion of progress markedly different from that of conventional social science. As Geertz states: “Anthropology, or at least interpretive anthropology, is a science whose progress is marked less by perfection of consensus than by a refinement of debate” (p. 29). But is this refinement of debate actually occurring?

Although Geertz is quite clear about the differences between interpretive theory and normal science, the vocabulary that he employs remains a source of concern. The loose equation of description with analysis, analysis with explanation, explanation with description, and theory with all of these does not offer a refinement of debate based on “the precision of distinctions” that Geertz promises (p. 29). The same kind of imprecision and ambiguity can be found in Geertz’s discussion of “objectivity” and “subjectivity,” a distinction that he finds “misconceived” (p. 10). Arguing that complete objectivity is impossible but that one cannot simply “let one’s sentiments run loose” (p. 30), Geertz leaves a rather large area over which the intellectual imagination can roam. At the same time, he offers no clarification of the ontological status of knowledge gained in the exercise of cultural interpretation, regarding this issue as unimportant (p. 10). Perhaps it is true, as Geertz would have it, that interpretation is difficult because meaning may be an “elusive and ill-defined pseudoentity” (p. 29), and perhaps, as Geertz claims, he has resisted the dangers of subjectivism and cabbalism (p. 30). But heralding the difficulties of interpretation and demurring from the temptations of subjectivism do not necessarily combine to produce a “refinement of debate.” Geertz seems more concerned with suggesting a sci-

ence of interpretation than with developing it in a systematic, rigorous fashion (Walters 1980:547).

THEORETICAL DIFFERENCE OR THEORETICAL SUPERIORITY?

Geertz’s use of the term “science” derives from the hermeneutical “human sciences” as opposed to the natural sciences. The intellectual forebears of modern interpretivists—the German idealist philosophers of the 19th century—also spoke of interpretation as a “science.” Like Geertz, they emphasized that human life was characterized by self-awareness, reflexivity, creativity, intentionality, purposiveness, and meaningfulness; human life thus took on dimensions different from those of the natural world and was not subject to natural law. And, like Geertz, they sought an alternative to approaches based on the natural-science model. Instead of formulating general explanatory systems, the idealists pursued the organization and presentation of data “in such a way as to make them intelligible through a process of individual understanding, empathy, or *verstehen*” (Kaplan and Manners 1972:27). Most German idealists did not claim that *Verstehen* was a superior approach; they considered the two approaches equally valid although irreconcilable.

Bettelheim (1982:70, italics mine) has elaborated the distinctions relevant to the 19th-century idealists:

In the German culture . . . there existed and still exists a definite and important division between two approaches to knowledge. Both disciplines are called *Wissenschaften* (sciences), and they are accepted as equally legitimate *in their appropriate fields*, although their methods have hardly anything in common. These two are the *Naturwissenschaften* (natural sciences) and, opposed to them in content and in methods, the *Geisteswissenschaften*. The term *Geisteswissenschaften* defies translation into English; its literal meaning is “sciences of the spirit,” and the concept is one that is deeply rooted in German idealist philosophy. These disciplines represent entirely different approaches to an understanding of the world. Renan, trying to translate them into French, suggested that they divided all knowledge into *la science de l’humanité* and *la science de nature*. In such a division of knowledge, a hermeneutic-spiritual knowing and a positivistic-pragmatic knowing are opposed to each other. . . . The influential German philosopher Wilhelm Windelband, Freud’s contemporary, elaborated on the fundamental differences between these two approaches to knowledge. He classified the natural sciences as nomothetic, because they search for and are based on general laws and in many of them mathematics plays an important role. The *Geisteswissenschaften* he called idiographic, because they seek to understand the objects of their study not as instances of universal laws but as singular events; their method is that of history, since they are concerned with human history and with individual ideas and values. Nomothetic sciences require verification through replication by experiment; their findings ought to permit mathematical and statistical analysis, and most important, ought to permit exact predictions. Idiographic sciences deal with events that never recur in the same form—that can be neither replicated nor predicted [but that can be understood].

Although the orientation of 19th-century German philosophy was by no means as uniform as Bettelheim’s summary suggests, nonetheless, many thinkers of that period viewed the two sciences as separate but equally legitimate (see Iggers 1968). Contemporary interpretive theory, following the *Geisteswissenschaften* tradition, is scientific in this special and limited sense.

Some contemporary interpretivists continue in this 19th-century *Geisteswissenschaften* tradition. Geertz, in contrast, views interpretive theory not merely as a legitimate alternative to conventional social science, seeking parity with it, but rather as a theoretical framework that will “challenge” and ultimately replace the tired mechanistic, reductionist approach of positivism with a “refiguration of social thought.” That is, he suggests that interpretive theory is *superior* to conventional social science, arguing that the road to discovering the causes and effects of social phenomena lies “less through postulating forces

and measuring them than through noting expressions and inspecting them." In his attempt to replace conventional social science with interpretive theory, Geertz distances himself from part of the German idealist tradition by implicitly rejecting that special niche that allowed idealists to give meaning to their science without claiming superiority over other forms of science.

The critical issue here is not a semantic one concerning the use of the word "science"; set in proper historical and theoretical context, this issue is not particularly troublesome. Nor is the issue one of theoretical difference; all sides agree that interpretive science and conventional social science are quite different. Nor is the issue one of incompatibility or incommensurability of the two approaches, since Geertz has compared them and found the interpretive approach superior. The key issue is whether Geertz can demonstrate that an approach that is not conventionally scientific in aims, nature, and method—that originally was not meant to be directly comparable to science in the usual sense—is superior to conventional social science. Unfortunately, as Geertz himself notes, interpretive theory lacks criteria for comparison and assessment, and while he maintains that this is "precisely the virtue of it," this and his admitted inability to state interpretive theory clearly make it difficult to demonstrate its superiority.

Geertz is not alone among interpretivists in lacking the criteria to establish the superiority of the interpretive approach over the scientific one (or even of one interpretation over another). For example, Rabinow and Sullivan (1979:9) affirm that a narrow rationalism can lead to the "impoverishment" of science while "the interpretive turn" can increase the means of understanding human phenomena. Human phenomena are like texts and therefore open to several readings—indeed, so much so that Rabinow and Sullivan feel compelled to assure their readers that the range of interpretations is "not infinite" (p. 12). Yet when it comes to evaluating alternative interpretations, they concede that interpretive theory in its many varieties has made little progress in the realm of verification (pp. 7–8).

Taylor (1979:66) presses the interpretivist position to its logical extreme. He states that there is "no verification procedure we can fall back on. We can only continue to offer interpretations." Although he realizes that this position is "scandalous" by conventional social science standards, for him the study of human phenomena requires a radical subjectivism with some theoretical differences arbitrated only by deeper insight or superior intuition. That is, "If you don't understand it, then your intuitions are at fault, are blind or inadequate . . ." (p. 67). Or, as Geertz has remarked, "you either grasp an interpretation or you do not."

The inability of interpretive theory to offer criteria for evaluating either different interpretations or different paradigms poses a formidable barrier to claims of theoretical superiority. In some of his works, such as *Agricultural Involvement, Peddlers and Princes*, and *Islam Observed*, Geertz seems to have transcended this barrier, but these works contain a good deal of conventional social science and are far less programmatic for interpretive theory than the sources referred to in this essay. Geertz does not seem to consider his comparative and historical works important foundations for interpretive theory. Instead, he chooses other type cases to argue the superiority of a non-comparative, idiographic interpretive approach. In these type cases (such as the Balinese cockfight), however, the limitations of the interpretive approach evident in Geertz's abstract theoretical program become even more apparent. Two of these cases involve Balinese trance and the 19th-century theatre state of classical Bali.

BALINESE TRANCE

Balinese trance is one of the earliest subjects from which Geertz argues for a particularistic, interpretive approach. In "The

Impact of the Concept of Culture on the Concept of Man," originally published in 1966, Geertz discusses Balinese trance in the context of cross-cultural comparison and finds that drawing a line between what is natural, universal, and constant in man and what is conventional, local, and variable is so difficult as to suggest "that to draw such a line is to falsify the human situation, or at least to misrender it seriously" (Geertz 1973b:36):

Consider Balinese trance. The Balinese fall into extreme dissociated states in which they perform all sorts of spectacular activities—biting off the heads of living chickens, stabbing themselves with daggers, throwing themselves wildly about, speaking with tongues, performing miraculous feats of equilibration, mimicking sexual intercourse, eating feces, and so on—rather more easily and much more suddenly than most of us fall asleep. Trance states are a crucial part of every ceremony. In some, fifty or sixty people may fall, one after the other ("like a string of firecrackers going off," as one observer puts it), emerging anywhere from five minutes to several hours later, totally unaware of what they have been doing and convinced, despite the amnesia, that they have had the most extraordinary and deeply satisfying experience a man can have. What does one learn about human nature from this sort of thing and from the thousand similarly peculiar things anthropologists discover, investigate, and describe? That the Balinese are peculiar sorts of beings, South Sea Martians? That they are just the same as we at base, but with some peculiar, but really incidental, customs we do not happen to have gone for? That they are innately gifted or even instinctively driven in certain directions rather than others? Or that human nature does not exist and men are pure and simply what their culture makes them?

Geertz feels that such comparative questions do little justice to the phenomenon of Balinese trance and therefore opts for an idiographic approach in which human nature is interdependent with culture in its "highly particular forms" (1973b:49). "It may be in the cultural peculiarities of people—in their oddities—that some of the most instructive revelations of what it is to be generically human are to be found; and the main contribution of the science of anthropology to the construction—or reconstruction—of a concept of man may then lie in showing us how to find them" (p. 43). While Geertz does not deny that generalizations can be made, he questions their basis and utility, preferring the details and richness of an interpretive particularistic approach: "We must, in short, descend into detail, past the misleading tags, past the metaphysical types, past the empty similarities to grasp firmly the essential character of not only the various cultures but the various sorts of individuals within each culture if we wish to encounter humanity face to face" (p. 53).

Although one might wish to "descend into detail," Geertz's discourse on Balinese trance ends here. The trances are briefly described, questions are asked, and assertions are made, but there is no further analysis or explanation of the phenomenon. Geertz does not ask why trance should exist in Bali, why it takes the forms that it does, why men and women participate, or any number of other questions. One is left to wonder why he has not made a stronger case for an interpretive approach to Balinese trance, especially since the classic work in the field, Belo's detailed *Trance in Bali* (1960), uses a scientific one. A plausible interpretive approach to Balinese trance should include a rebuttal of Belo's comparative and scientific argument and an effective counterargument. At this point, Geertz's assertions about the propriety and superiority of the interpretive approach are merely suggestive, and the question that he had hoped to resolve persists: to what extent is a comparative or cross-cultural approach antithetical to the ethnographic detail that interpretivists so value?

In the case of trance, there is a body of relevant comparative material that merits attention. For a number of years, Bourguignon and her associates have been conducting both detailed ethnographic and large-scale holocultural studies of possession (see Bourguignon 1978 for a bibliography). On the basis of her

own field research on Haitian *voudon*, Bourguignon began looking at other case studies of trance and found that, partly because of their idiographic nature, the accumulation of individual studies had not led to a greater theoretical understanding of trance: "The argument that cultures were unique entities, and the consequences of cultural relativism that flowed from it, seemed to encourage a tendency for ad hoc interpretation. The result was that no systematic theory building, at least in the area of interest to me, was possible" (Bourguignon 1978:186). She and her associates mapped the worldwide distribution of dissociative states and found institutionalized forms of trance in 90% of the 488 societies in her sample. Furthermore, she was able to correlate different types of dissociation with variables like population size, subsistence type, marriage type, hierarchy of jurisdictional levels, etc. She discovered that the level of social complexity could predict with a fair degree of certainty the type of possession state found in a particular society.

Correlations and predictions derived from cross-cultural research are often viewed with skepticism by anthropologists, including symbolic anthropologists, but in Bourguignon's approach there are many ideas that are compatible with an interpretive framework. Bourguignon is interested in meaning, the native point of view, and the concepts of person, self, drama, and performance. She begins her interpretation of similarities and differences in trance states with a human universal—the idea of "self-awareness," derived from the work of A. I. Hallowell. This universal is also a cultural and social product requiring a consideration of how personal identities are maintained within particular cultures. Bourguignon not only asks how people conceive of themselves and how these conceptions are maintained, but seeks to know how they are altered and why. Trance in a ritual context may represent dissociation and discontinuity. It is thus necessary to inquire about conditions that might favor or discourage dissociational states of different kinds.

Bourguignon finds that the more complex the society, the more likely it is to rely on possession trance, in which individuals, often women as well as men, are publicly possessed by spirits, whereas in less complex societies, individuals, usually men, go into trance to serve as intermediaries interacting with the supernatural. She also finds that in complex societies powerlessness, anxiety, and fear in the context of rigid social hierarchies may lead to possession trance as an indirect means to prestige and power. Why the use of such an indirect means? "The answer appears to be that it is, indeed, a world perceived as hostile in which the individual is anxious and powerless. Only the spirits appear to have the power to effect the required changes. And so the individual—partly in fantasy and partly through the acceptance of collective fantasy by his peers—may become powerful by impersonating the spirits" (Bourguignon 1978:487). She cites case studies to support this assertion.

Bourguignon links the presence of different dissociational states with socialization practices and sexual hierarchies; in more complex societies, compliance and obedience are demanded of children, with women socialized for even higher degrees of compliance. Thus in populous, stratified agricultural societies adult economic role behavior requires obedience in children, who become conforming adults who believe in spirits that demand compliance. Possession trance reflects and gives expression to this adaptation (pp. 506–7).

In addition, Bourguignon finds that in populous, stratified agricultural societies, the differentiation of social roles is paralleled in cosmological roles (pp. 504–5):

Possession trancers reflect this complexity by impersonating the spirits and possession trance rituals thus may be thought of as expressions of a society's model of its own social structure. Furthermore, such rituals often occur within structured cult groups. These groups are themselves a feature of complex societies, including specialized and ranked positions. The structure of these groups, too, is reflected in the possession

trance rituals. By playing various spirit roles an individual may find ways of coming to terms with alien or dangerous aspects of his society or of his society's relations with other groups or forces in the environment.

Of what relevance are Bourguignon's findings for Balinese trance? Clearly Bali is a populous, stratified agricultural society. Geertz (1975:50) would concur, perceiving the social hierarchy through a set of symbolic labels:

The Balinese have at least a half dozen major sorts of labels, ascriptive, fixed, and absolute, which one person can apply to another (or, of course, to himself) to place him among this fellows. There are birth-order markers, kinship terms, caste titles, sex indicators, teknonyms, and so on, each of which consists not of a mere collection of useful tags but a distinct and bounded, internally very complex, terminological system. To apply one of these designations or titles (or, as is more common, several at once) to a person is to define him as a determinate point in a fixed pattern, as the temporary occupant of a particular, quite untemporary, cultural locus.

These, he says, are part of the Balinese life as *theatre*:

... there is in Bali a persistent and systematic attempt to stylize all aspects of personal expression to the point where anything idiosyncratic, anything characteristic of the individual merely because he is who he is physically, psychologically, or biographically, is muted in favor of his assigned place in the continuing and, so it is thought, never-changing pageant that is Balinese life. It is *dramatis personae*, not actors, that endure; indeed it is *dramatis personae*, not actors, that in the proper sense really exist.

So great is the emphasis on the proper performance of ascribed social roles that Balinese life is permeated by *lek*, or what Geertz calls "stage fright" (pp. 50–51):

... what is feared is that the public performance to which one's cultural location commits one will be botched and that the personality (as we would call it but the Balinese, of course, not believing in such a thing, would not) of the individual will break through to dissolve his standardized public identity. When this occurs, as it sometimes does, the immediacy of the moment is felt with excruciating intensity, and men become suddenly and unwillingly creatureal, locked in mutual embarrassment, as though they had happened upon each other's nakedness. It is the fear of *faux pas*, rendered only that much more probable by the extraordinary ritualization of daily life, that keeps social intercourse on its deliberately narrowed rails and protects the dramatis personae against the disruptive threat implicit in the immediacy and spontaneity which even the most passionate ceremoniousness cannot fully eradicate from face-to-face encounters.

In Geertz's ethnography, some of the key variables in Bourguignon's explanation are described, although he does not relate dissociative states to the nature of Balinese stratification or the anxiety and fear engendered by the acting out of social roles. Nor does Geertz indicate why Balinese should fall into mass trance during ritual performances instead of simply acting out their roles as one might expect in a society in which life is viewed as a pageant or social drama. To be sure, Balinese do enact the roles of minor witches, demons, and other mythical legendary figures (1973c:116), but as Geertz observes, "mostly it takes place through the agency of an extraordinarily developed capacity for psychological dissociation on the part of a very large segment of the population." Geertz thus describes the situation but is unable to explain it. He does say that trance cannot be understood apart from the religious beliefs that shape it (1973c:119), but, while most anthropologists, including Bourguignon, would concur with this position, it only suggests where a potential explanation might lie. It does not account for the presence or type of trance behavior found in Bali or elsewhere, nor does it clarify how religion shapes trance behavior or why this should be so. The idea that religious beliefs shape trance is, in this form, a nonexplanatory orienting statement, directing us where to look without providing specific relationships between the variables.

In contrast, Bourguignon is fairly specific about the relationships between ecological, social structural, and socialization variables that explain dissociative states like Balinese trance.

THE BALINESE THEATRE STATE

From her cross-cultural propositions, the general configuration of Balinese trance—from its public, mass nature to states of amnesia to sex role performance—can be predicted. Moreover, Geertz and Belo provide evidence linking Bourguignon's general variables to Bali specifically. Belo offers support for the psychological mechanism—socialization anxiety—that Bourguignon employs to link her general variables with trance states. At the same time, Belo (1960:255) delineates the relationships between trance, the individual, and the religious system:

The performances of the various types of trance constituted a setting up of communication with the power of the gods and an exorcism of powers of evil. The fact of the trance taking place would reassure the people. In this culture considerable anxiety was established early in life, and this anxiety created a need for persistent reassurance. . . . The practice may have had therapeutic value for the individual. The fact that seizures were scheduled in accordance with the religious life, and integrated with it, brought them under social control and gave them meaning and social significance.

Another parallel between Bourguignon's general explanation of trance and Belo's account of trance in Bali is the socially and individually rewarding nature of trance performances. According to Belo (p. 3), "the tendency to dissociate was rewarded with social approval and brought under social control. The subject developed a secondary personality which was honored in the cultural religious scheme. A rise in status was effected."

There can be little doubt that Balinese trance is "extraordinary and deeply satisfying" (Geertz 1973b:36) as both Belo and Geertz describe it. "To become entranced is, for the Balinese, to cross a threshold into another order of existence—the word for trance is *nadi*, from *dadi*, often translated 'to become' but which might even be more simply rendered 'to be'" (Geertz 1973c:116). Yet Geertz perceives that this satisfaction often comes from violations of normal behavior during possession states. Although he does not explain why this should be the case, Belo offers a psychological explanation based on Balinese and cross-cultural data. She believes that the pleasurable quality of trance experience may derive, in part, from the surrendering of the self (1960:223): "What could induce a grown man to wallow around in a mud puddle and eat filth, to hop about on all fours, or slither over the ground on his belly, if he were in his right mind? Behavior that would be degradation—animal-like behavior which the Balinese were careful to avoid in their current manners, and which was even institutionalized as punishment for incest—becomes in the trance state pleasurable and delightful." Trance states in Bali, according to Belo, provide individuals with opportunities to be angry or "crazy" (p. 223), to change sex roles, and to act out higher as well as lower statuses (p. 3). This corresponds to Bourguignon's view that ritualized dissociation provides the self with an alternate set of roles "in which unfulfilled desires, 'unrealistic' in the context of the workday world, get a second chance at fulfillment. . . . In a world of poverty, disease and frustration, ritual possession, rather than destroying the integrity of the self, provides increased scope for fulfillment" (1978:487). In this view, trance is a widespread, culturally constituted defense mechanism.

Balinese trance has been used by Geertz as a type case to argue for a noncomparative, interpretive approach in anthropology. However, even this brief review—which does not cover the full range of materials on trance in Bali or elsewhere—suggests that a comparative, scientific approach to Balinese trance has a good deal to offer. Moreover, Bourguignon's general explanatory framework is supported by data from Geertz and Belo. Of course, Bourguignon's explanation could be spurious, and, as she herself says (p. 508), much more work needs to be done. Nevertheless, this example illustrates how a comparative, explanatory approach based on human universals and cultural variables can illuminate idiographic descriptions of phenomena like trance without falsifying or misrendering them as Geertz fears.

A second and somewhat different type case from which Geertz argues for a particularistic, interpretive approach is presented in his *Negara: The Theatre State in Nineteenth-Century Bali* (1980b). The Balinese *negara*, like other Southeast Asian states, was not highly centralized or tightly integrated. It was neither feudal, patrimonial, nor bureaucratic; rather, it took the form of "a ceremonial order of precedence imperfectly impressed upon a band of sovereigns" (p. 62). Lavish and dramatic, "court ceremonialism was the driving force of court politics," and, thriving as it did on ceremony and pageantry, "power served pomp, not pomp power" (p. 13). The *negara* was therefore a special kind of state, a "theatre state."

Geertz rejects the traditional European framework for political analysis in the case of the *negara* because in Bali the state drew its power not from conventional sources, but from "its imaginative energies, its semiotic capacity to make inequality enchant" (1980B:123, italics mine). Pomp and pageantry did not simply represent the state or reflect its power arrangements; they were the state, "the thing itself," its very essence (pp. 13, 120, 123). "The dramas of the theatre state, mimetic of themselves, were, in the end, neither illusions nor lies, neither sleight of hand nor make-believe. They were what there was" (p. 136). Because of the unique nature of the Balinese polity, based as it was on "a controlling political idea" (p. 13), Geertz calls for a different kind of political theory to comprehend it, invoking a semiotic conception of the state founded on "a poetics of power, not a mechanics of power" (p. 123).

From the cult of the divine king to the intricacies of agricultural organization, Geertz delineates the complexity, permeability, and fragmentation of a political order suffused with religious symbolism. Indeed, this political order seems to lack so much in the way of conventional authority that it hardly seems to be a state at all. As Geertz observes (pp. 62–63):

. . . if one considers it in terms of political legitimacy, it appears to be organized from the top down—to descend from the paramount lord, or king, through the varying grades of lesser lords, related and unrelated, to the lowly villager, the hapless object of its arbitrary exactions. But if one examines it as a system of domination, a structure of command and compliance, it does not look that way at all. Rather than flowing down from a pinnacle of authority or spreading out from a generative center, power seems instead to be pulled up toward such a pinnacle or to be drawn in toward such a center. The right to command was not delegated from king to lord, lord to lordling, and lordling to subject; rather it was surrendered from subject to lordling, lordling to lord, and lord to king. Power was not allocated from the top, it culminated from the bottom.

At the top, the "ordering force" was "display, regard, and drama" (p. 121)—a cultural order that interdigitated with local lords, kin groups, temple societies, and irrigation networks. Nevertheless, the "exemplary center" had "virtually no policies" (p. 132), limited interests, and little direct coercive power over the peasants. Its contribution to Balinese peasant life was symbolic. "What the Balinese state did for Balinese society was to cast into sensible form a concept of what, together, they were supposed to make of themselves: an illustration of the power of grandeur to organize the world" (p. 102).

Once again Geertz has offered a fascinating and evocative look at an unusual cultural phenomenon, but while this portrait of the *negara* is stimulating, it is largely descriptive and analytic rather than explanatory. As Geertz states, "two approaches, two sorts of understanding must converge if one is to interpret a culture: a description of particular symbolic forms (a ritual gesture, an hieratic statue) as defined expressions; and a contextualization of such forms within the whole structure of meaning of which they are a part and in terms of which they get their definition. This is, of course, nothing but the by-now-familiar trajectory of the hermeneutic circle . . ." (p. 103). This

approach is essentially configurationist (p. 132) rather than causal. According to Spiro (1966:100), accounts which "delineate the configuration in, or relationships among, a set of sociocultural variables are essentially descriptive rather than explanatory—unless of course some theory, causal or functional, is offered to explain the configuration." Spiro goes on to point out that the use of ordering principles does not provide explanation either. Instead, such principles are verbal labels "which at best order a set of data according to a heuristic scheme" (p. 100). The questions of why the Balinese *negara* assumed its unique configuration and why "display, regard, and drama" were such important ordering principles in Balinese political life are neither answered nor even raised.

Geertz is less interested in explanation than in capturing the very essence, the "reality," of Balinese politics, and most of *Negara* is devoted to explicating this "densely configured reality" (p. 132). Yet the exact nature of power in the Balinese state remains somewhat elusive. Geertz contends, on the one hand, that the contribution of the state to the populace was in terms of the "grandeur of power" and, on the other hand, that local institutions were the primary agencies of government. But if the state and its rulers were so far removed from governance and if power "cumulated from the bottom," it is difficult to understand those Balinese political activities that seem to have operated from the top down. From *Negara* in passing and from other sources, there are indications that in classical Bali taxation could be oppressive, that slavery existed, that warfare and rebellion were present, and that monarchs manipulated the symbolic order to their advantage. Although Geertz does not discuss these activities in detail, they do suggest that the state was more than a distant symbolic constellation, a representation of the "grandeur of power" to the populace.

For example, in the footnote in which Geertz's main exposition of the tax system is found, he remarks that while taxation policy varied from location to location, it was "no less explosive in classical Bali than it has been elsewhere in the world" (p. 180). The implication is that taxation could be exploitative, and although reliable information is hard to obtain, Boon's (1977) recent work on Bali indicates that sovereigns had considerable sway over the local population through their revenue-gathering activities. Boon quotes at length (p. 28) from a Dutch commissioner's report originally published in 1835:

The revenues of the rajas consist of a kind of land-tax, which every farmer who has rice fields must bring in, amounting to ten *dubbeltjes* for each landed proprietor, whether farming many or few fields. The *vorst* was paid a specific tribute for marriages. . . . There is also a toll levied on bazaars or markets, which are tolerably extensive; moreover on cockfights, besides other gambling games, and on holding opium dens. In kingdoms with harbors the raja leases the import and export duties to Chinese or even to Balinese, although this seldom yields much profit. . . .

The exclusive right to hold puppet theatre (*wayangs*) and *Ronggings* are the rajas', and this provides them considerable revenue. They also have all extensive manufacture of silk and other fabrics . . . also many fruiting and flowering trees, the produce of which they have sold in the markets and along the road. . . .

Opium, iron, and all necessary foreign articles are purchased by the rajas and on their behalf are resold on a small scale to their subjects with great usury. If the folk perform a service for them, to pay for it, however, on the following day they hold shadow puppet plays or cockfights in their palace, and are thus sure that the farthing expended would within twenty-four hours be back in their coffers. I have already mentioned that the slave trade made up one of the most important sources of the rajas' income.

Boon cites another mid-19th-century source on services the population provided for the sovereigns (p. 99):

The primary feudal duty, as in the Middle Ages, is *service in war*; and further the Punggawas (deputy overlords) and their subordinates have to furnish assistance in all *public works and festivals* of the prince, and the lower orders also have to carry out all the works of the Punggawas. The people, under the guidance of the Punggawas, have to build the

princes' palaces and places of cremation, to repair the roads and besides this to contribute mostly in kind, towards the expenses of all offerings, family feasts, and creations.

These activities did not make authoritarian despots of the rajas, and, as Boon points out, symbolic as well as material ends were served. Nonetheless, the symbolic contribution of the state to the populace may not have been sufficient to extract the peasants' material contribution to the state. That is, that contribution may not have been "surrendered" from peasant to lord (Geertz 1980b:63). If the *negara's* ability "to make inequality enchant" was not sufficient for revenue collection and if coercion was necessary, then it is easier to understand why taxation policy had the "explosive" consequences that Geertz mentions.

Certainly there were less enchanting aspects of the Balinese state. As Geertz reminds us, "No one remains dominant politically for very long who cannot in some way promise violence to recalcitrants, pry support from producers, portray his actions as collective sentiment, or justify his decision as ratified practice" (p. 123). But Geertz deemphasizes the comparison of *forms* of political behavior, preferring to stress the symbolic *content* of Balinese politics that can only be understood intraculturally. Thus, when analyzing Balinese political ideology in symbolic terms, he warns against equating this ideology with "mystification," "lies," "illusion," or "class hypocrisy" (pp. 122, 136), since such a Western bias would preclude seeing Balinese ideology for what it really was.

Although Geertz assures us that there was a separate Balinese political reality, perhaps a case can also be made for viewing it in terms of mystification and interests. After all, it is possible that symbolic dramas can serve the purposes of power, domination, exploitation, and resistance (Walters 1980:556). And this is the case that Boon makes when examining the continual reworking of ideas and alternative values in Balinese political life. Citing Bateson's earlier work, he discusses how power can "cumulate from the bottom" only to be reinterpreted in legend by the dominant political group: "Rather than [the] monarch regally bestowing favors on local populations, it might sometimes have been more a matter of commoners evolving their own lord, a master of their water to help them compete with other locales. Of course it would never look this way in legendary retrospect" (1977:55). What seems obvious here could be deceptive, following Geertz's line of thought, since the semiotic aspects might remain "so much mummerly" (1980b:123). But regardless of which interpretation is accepted, there is an important issue that needs clarification. How does one know what the reality of Balinese political ideology is? How does one discern whether this ideology reflects, represents, or otherwise expresses some symbolic principle in Balinese life or whether it masks, denies, obfuscates, or mystifies social reality (Silverman 1981:163)? The tendency has been to proceed on one or the other *assumption* without discussing the basis for preferring one over the other. Without some clarification of how one chooses between these assumptions, apart from hypothesis testing, Geertz's argument for a separate Balinese political reality is hardly persuasive.

Geertz's theoretical preference for semiotic theory over conventional political theory also seems to be based on assumption and assertion. For Geertz, the two types of theory are scarcely compatible or complementary. If they were compatible, then there would be a "poetics of power" and a "mechanics of power"; pomp could serve pomp and power. Yet Geertz asserts that this is not the case: "power served pomp, not pomp power"; what is needed is a "poetics of power, not a mechanics of power" (italics mine). The grounds for preferring semiotic theory over conventional political theory are given by Geertz in the following passage (1980b:123):

No one remains dominant politically for very long who cannot in some way promise violence to recalcitrants, pry support from producers, portray his actions as collective sentiment, or justify his decisions as

ratified practice. Yet to reduce the negara to such tired commonplaces, the worn coin of European ideological debate, is to allow most of what is most interesting about it to escape our view.

Here Geertz tacitly acknowledges that conventional explanations of political life are not irrelevant or unimportant for an understanding of power in Bali; for him, they are simply not very "interesting." He does not contest the applicability of conventional political theories to Bali, either individually or all together. Instead, he considers their application "fatally easy" (p. 123). But if these other theories are applicable, do they not warrant at least some consideration? And even if we accept Geertz's choice of problem focus and theoretical preference, does the dismissal of other theoretical perspectives on the basis of interest and ease of application really establish the theoretical superiority of the semiotic approach?

In the absence of consideration and refutation of available and potentially relevant political theories, Geertz's semiotic approach only challenges the conventional wisdom; it does not replace it. And as refreshing as this challenge may seem, there are conventional approaches that deal with the same kinds of problems, providing comparative, scientific perspectives on the relationship between politics and symbolism that neither jeopardize idiosyncratic descriptions of particular cultures nor deny the universe of meaning. Cohen's *Two-Dimensional Man* (1974) and Silverman's analysis of an Italian ritual (1981) are notable in this regard because they directly question Geertz's approach to the relationship between symbolism and politics. There are also particular theories relevant to the development of the Balinese polity, including the literature on "segmentary states," both classical and recent (Southall 1965, Claessen 1979, Skalnik 1979). Closer to Bali itself is Winzeler's (1976) attempt to set the Balinese case, among others, into an adaptive framework. Winzeler criticizes most current state-formation theories, including Geertz's ideas on the Balinese theatre state, and offers an alternative theory to explain the relative lack of centralization and incomplete vertical integration found in Southeast Asian states. Although this theory and the others just mentioned are not without their own problems, they do contest the Geertzian notion that the interpretive approach can provide a theoretical breakthrough for political theory. And since Geertz's theory has its drawbacks, other theoretical options should be explored.

The arguments that Geertz uses to advance interpretive theory through the cases of the Balinese theatre state and Balinese trance are clearly not as strong as they could be. Their weaknesses, however, are not those that Geertz has proposed. The major problem in the theoretical development of interpretive theory, as viewed by Geertz, is that there is a profound tension between "the need to penetrate an unfamiliar universe of symbolic action and the requirements of technical advance in the theory of culture, between the need to grasp and the need to analyze." This barrier is "essentially irremovable" (1973a:24). Our analysis of interpretive theory as applied to Balinese trance and the Balinese theatre state indicates that the problems lie elsewhere, in logistical inconsistency and overstatement, argument by assertion rather than clearly demonstrated links between theory and data, and failure to review and consider available, plausible theoretical alternatives. These problems are not necessarily inherent in interpretive theory; they are self-inflicted.

Geertz has cautioned against cultural analysis's losing touch with "the hard surfaces of life—with the political, economic, and stratificatory realities within which men are everywhere contained—and with the biological and physical necessities on which those surfaces rest. . . . The only defense against it, and against, thus, turning cultural analysis into a kind of sociological aestheticism, is to train such analysis on such realities and such necessities in the first place" (1973a:30). Had he followed his own advice more closely, his analyses of Balinese trance and the Balinese theatre state might have turned out differently.

CONCLUSION

Having reviewed the theoretical program of interpretive theory and two cases used by Geertz to support a theory that promises not only a reformulation of anthropological thinking but a more general "refiguration of social thought," we can now examine the current status and future direction of that theory. What seems to be happening in the development of Geertz's work is hardly unexpected. It is taking exactly the path that he anticipated for it: finer and more elaborate descriptions of culturally situated phenomena with less emphasis on theoretical or methodological rigor. The consequences of this development have sometimes been glossed over. For example, Rice (1980) concludes that the logical and methodological problems of Geertz's work are more than offset by his ethnographic contributions. But can such problems be overlooked in assessing the *general theoretical* contributions of interpretive theory? Peacock (1981:129), who is quite sympathetic to the interpretive approach, separates Geertz's theoretical and ethnographic contributions. He concludes his assessment of Geertz's work with high praise for his concrete, empirically based studies but remarks that while Geertz's ethnographic work is significant, "stated abstractly, the contribution may seem trivial."

Other anthropologists have been more specific about the theoretical weaknesses of interpretive theory. Roseberry (1982), in his reanalysis of the Balinese cockfight, takes the Geertzian notion of culture-as-text to task, arguing that interpretive analysis is frequently unclear about who in the text is speaking to whom about what; the text seems separated from its social context in terms of both social differentiation and social process. Keesing (1982) also finds that Geertzian analysis of symbolic forms has become unnecessarily abstracted from the process of their creation and from the people who hold them. As the analysis of meaning has become more cryptographic, it seems to have acquired the same elusiveness that functional analysis had 20 years ago and that structural analysis had a little over a decade ago.

Recent critiques imply that interpretive theory can be made more explicit and perhaps more scientific, but interpretive theory seems to be headed in a different direction, turning inward on itself. There are more and more "exotic" descriptions—denser, more detailed ethnographies—but they do not seem to yield greater theoretical development. Instead, they lead to an intensification of an already existing pattern, a pattern that is becoming increasingly involuted. Foster (1982:222) contends that *Negara* "represents a contribution to Balinese ethnography without advancing our knowledge of symbolization or larger questions of meaning and signification. How does the author arrive at his slick, symmetric formulations? What is the pathway between the 'data' and the meaning attributed to it? Because Geertz avoids these questions, one wonders whether this version of cultural analysis is reaching paradigm exhaustion."

Although ethnographies continue to multiply within the interpretive framework, there is no clear future for interpretive theory. Geertz, who has been so successful at apprehending symbolic order amidst cultural chaos, is himself at a loss to determine the direction of the intellectual movement that he helped found. In "Blurred Genres," after summarizing the diverse contributions that are leading to a "refiguration of social thought," he muses: "The interesting question is not how all this muddle is going to come magnificently together, but what does all this ferment mean" (1980a:178). If the "muddle" does not come together, how long can it continue on its involutory path? And if one of interpretive theory's most brilliant and ardent advocates, after two decades of working on a vocabulary to make thick description possible, cannot see where the movement he christened is going, how long will it be before this "strange science" has run its intellectual course?

A movement without direction, a program troubled by inconsistency, an approach that claims superiority over conventional social science but is limited by the absence of criteria for evaluating alternative theories, and type cases that do not necessarily support the interpretive theory—can this be the basis for a different anthropology and a major intellectual movement? Granted that the ideas are alluring, exciting, and even glamorous, the assessment of theory is not merely a matter of taste. As the historian Walters (1980:556) warns, Geertzian models may lead to a retreat into “sterile elegance . . . read for the amusement of one’s peers with little regard for the world of process and change.” There is the possibility that interpretive theory will remain little more than a style, a fashion, a genre, one encumbered by “qualitative analysis and virtuoso interpretations based on thick descriptions with all of the personal idiosyncrasies of method that such an approach presumes” (Colby, Fernandez, and Kronenfeld 1981:425).

Geertz has offered anthropologists a choice and a promise, but perhaps the matter is not so simple. Must anthropologists choose between a dull, plodding, conventional scientific approach and Geertz’s imaginative but often flawed interpretive one? Is it not possible to continue as we have in the past, to live, however uncomfortably, with two distinctive frameworks—one humanistic and the other scientific—without blurring the genres and while recognizing the strengths, weaknesses, and limitations of each? Since the founding of the discipline, these two perspectives have existed in an uneasy tension. As Rappaport (1979: back piece) has noted:

Two enterprises have proceeded in anthropology since its earliest days. One, objective in its aspirations and inspired by biological disciplines, seeks explanation and is concerned to discover laws and causes. The other, subjective in its orientation and influenced by philosophy, linguistics and the humanities, attempts interpretation and seeks to elucidate meaning. I take any radical separation of the two to be misguided, for the relationship between them, with all of its difficulty, ambiguity, and tension, is a reflection of, or metaphor for, the condition of a species that lives in terms of meanings in a physical universe devoid of intrinsic meaning but subject to causal laws.

This tension, its sources, and its implications for the development of different theories should be remembered in assessing the extent to which interpretive theory will lead to a genuine refiguration of social thought. A small industry has grown up around the work of Clifford Geertz, generating hopes of a major theoretical breakthrough. But, as theory watchers in anthropology know, anticipation and the contemplation of possibility are only the first tentative steps in the stalking of the elusive paradigm shift.

Comments

by ATTILA ÁGH

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Shankman’s paper is a “thin” evaluation of Geertz’s theory because his criticism remains embryonic or vague. He quotes Geertz as cautioning against cultural analysis’s losing touch with “the hard surfaces of life—with the political, economic, and stratificatory realities”—and comments, “Had he followed his own advice more closely, his analyses of Balinese trance and the Balinese theatre state might have turned out differently.” I see here a genuine starting point for the analysis of Geertz’s theory, but Shankman does not follow his own advice either, and the comment remains a comment. To contrast the Balinese political and economic realities with their cultural counterparts, with trance and the theatre state, Geertz did attempt an analysis of these realities that is largely overlooked by Shankman. Perhaps he should have gone back to Geertz’s *Agricultural Involution* (1963) for a start. Instead, we have a description of Geertz’s theory of culture, a list of “recent cri-

tiques,” and a brief glimpse of Geertz’s views on “conventional political theory” that is incapable of bridging the gap between the body of the paper and its conclusion.

by ERIKA BOURGUIGNON

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In this critique of Geertz’s theoretical program, Shankman cites his 1966 statement (Geertz 1973*b*) on Balinese trance and contrasts his rejection of the pursuit of comparative questions in dealing with that topic with my attempt at carrying on just that kind of research. The passage in question, cited in full, appears to have been written for rhetorical purposes, to give dramatic emphasis to the author’s arguments about cultural uniqueness. The very questions that conclude the paragraph are not of the sort that would lead one to comparative research, but rather offer caricatures of anthropological popularizations. Trance itself appears to be incidental to this exercise.

Geertz has given very limited treatment to the topic of trance and does not appear to be especially interested in it. He does offer a description (Geertz 1973*c*) of the ritual drama during which several types of trance occur. Here, however, the emphasis is on the theatrical and ritual context rather than on trance itself. It is his view of Balinese life as theater that appears to link this discussion of trance in ritual with his analysis of the *negara*. Interestingly, trance and ritual in Bali in this paper serve the larger purpose of supporting a cross-culturally useful definition of religion.

Regrettably, Geertz does not refer to the topic of trance in his 1975 comparative discussion of concepts of selfhood in Java, Bali, and Morocco. This is the more surprising in view of his discussion, cited by Shankman, of Balinese concerns with roles and stage fright (1975:50–51). This subject could easily have led into a discussion of trance, given the relationship between dissociation, fear, and disorientation. Moreover, trance plays a significant, albeit varying, role in each of these three cultures.

Whether or not Geertz is interested in trance is, however, not really the issue. I would agree with Shankman that trance, no less than other recurrent phenomena of human life, can be illuminated by comparative as well as interpretive study and that it is possible to approach an understanding, as well as an appreciation, of cultural differences. Moreover, we cannot know what, if anything, is truly unique to a particular culture if we lack a comparative basis from which to make such a judgment.

A major contribution of holocultural research has been to reduce the tendency of anthropological textbook writers and others to generalize about the diversity of human institutions and behaviors on the basis of a handful of cases. I should like to think that our work on trance and its social and ideological contexts has provided an example indicating that the interpretive and the comparative need not be mutually exclusive.

by DOUGLAS E. BRINTNALL

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Shankman’s article is a clear and sensitive exposition of the theoretical thought of Clifford Geertz. The suggestion in the conclusion is that there is a need to recognize the strengths, weaknesses, and limitations of both humanistic and scientific approaches in anthropology. Geertz’s humanistic kind of approach need not, therefore, be seen as a replacement for a scientific framework for research and analysis. Shankman’s conclusion, I believe, is both correct and useful. All too often anthropological theoretical schools have taken the form of cults around dominant personalities. Amidst the zealotry which sometimes arises there is value in putting positions into a broader historical perspective.

by JOHN R. COLE

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The attempt to create antiscientific anthropology is perhaps unnecessary, given the profession's seemingly *nonprogrammatic* tendencies in that direction. Culture is mystification-bound enough without "thick description" and obfuscation. Shankman's critique is very welcome, although perhaps it does not go far enough. While sympathetic in tone, it clearly is devastating, and one is tempted to ask "Why?"

Why is the critique *necessary*? Why is involuted theory so successful and appealing? Why use the term "science" for something which is not science? I do not mean to fault Shankman for the paper he did not write *explaining* the phenomenon he criticizes so well. (What is the meaning of my search for causes?)

by LINDA CONNOR

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The best contribution I can make to Shankman's excellent examination of Geertz's program is the perspective of an anthropologist who has spent two and a half years in Bali researching phenomena associated with trance. My own misgivings extend beyond the promise of Geertz's interpretive theory to the quality of the ethnography it spawns. In the case of Balinese trance, several of Geertz's key statements are exaggerated or misleading. Moreover, some of his interpretations of the significance of trance phenomena in Balinese culture are inconsistent with his own ethnographic descriptions.

For example, the statement "Trance states are a crucial part of every ceremony" has not been made by any anthropologist working on Bali other than Geertz, and it would take only a few weeks' residence in most Balinese villages to conclude that trance is part of only a minority of ceremonies. Ritual possession trance is pervasive in Balinese religious life because its occurrence is a possibility in many different ceremonial contexts, not because it always occurs. In the next sentence we are informed that Balinese who are possessed are "totally unaware of what they have been doing and convinced, despite the amnesia, that they have had the most extraordinary and deeply satisfying experience a man can have." Here we can only wonder why a scholar so interested in "actor-oriented" formulations of symbol systems neglected an opportunity to carry out the patient and careful interviewing that might have uncovered important information about the consciousness of Balinese trancers, beyond the claims of amnesia. Jane Belo provides fascinating verbatim interview material to illustrate the range of perceptions and emotions experienced and recalled by those possessed (e.g., Belo 1960:219-25).

In another article (1973c), we learn from Geertz that the word for "trance" in Bali is *nadi*. It is difficult to understand how this obscure word could be rendered as *the* Balinese word for trance. Belo records at least 11 words, I have recorded at least 13, all of which could be translated by the relatively imprecise English phrase "to go into trance." There are dozens more terms recorded that describe phenomena associated with trance. *Nadi* appears in Belo as a word used by the inhabitants of one small district to describe becoming possessed in a particular form of trance (*sanghyang*) manifested in that area. I have never heard the word spontaneously produced by a Balinese. More curious still, in providing us with this translation, Geertz shows that he is not aware of the single most important fact a phenomenological study of Balinese trance can produce: that there is no generic term for "trance" in Balinese (although such terms do exist in Bahasa Indonesia, the national language). Balinese terms referring to possession trance are differentiated by such criteria as social rank of the subject; the type of temple or other space, and the type of ceremony, in which the episode occurs; the status of the possessing agent; whether or not the subject has undergone a ritual consecration; the purpose of the trance; and so on. In fact, I find "trance"

Shankman: THE THICK AND THE THIN

to be a "misleading tag" in the Balinese context and prefer to think in terms of a cultural ideology of possession manifest in such realms as religious experience, ideas of self, illness, and healing (e.g., Connor 1982).

The general picture of Balinese trance painted by Geertz in these two papers is spectacular, full of weird feats, mass excitement, and unrestrained emotions—in short, "sheer pandemonium" (1973c:117). In a third article published in 1966, in which different interpretive points have priority, another picture emerges of the same form of trance ceremony that is described in the second article mentioned above (the Rangda-Barong confrontation). Here we find that "fearful witch and foolish dragon combat ends in a state of complete irresolution, a mystical, metaphysical, and moral standoff leaving everything precisely as it was, and the observer—or anyway the foreign observer—with the feeling that something decisive was on the verge of happening but never quite did. Balinese are not the sort to push the moment to its crisis" (1966:60-61). Is this really the same type of ceremony, the same people? In this article too, trance behavior, described in the first-mentioned publication as "a crucial part of every ceremony," is tentatively interpreted as perhaps a "subdominant but nonetheless important theme" in Balinese culture (1966:65).

In Geertz's favor, it should be noted that his considerations of Balinese trance form a small part of his published work on Bali, and perhaps from this it can be inferred that trance phenomena were not high on his list of research priorities. Moreover, anthropologists as a group are fond of overstatement when they present their ethnographic material, and a certain measure may enhance the clarity of an argument. But surely Geertz fails to demonstrate the ethnographic groundedness of his assertions? I share Shankman's doubts about the theoretical program of interpretive anthropology as espoused by Geertz, but I am even more concerned about the ethnographic standards such a subjectivist approach fosters. Geertz sometimes ignores basic (and admittedly rather prosaic) rules for the presentation of ethnographic evidence, and, perhaps because of a predilection for poetics, he does not systematically consider his arguments in relation to the work of anthropologists who have preceded him. To those of us with a more pedestrian concern with such issues, thick description sometimes appears to come out of thin air.

by REGNA DARNELL

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Clifford Geertz is clearly one of the most stimulating anthropologists currently writing. It is less clear, however, that his challenge to "conventional social science" is as revolutionary as either his writing or Shankman's commentary would suggest. Geertz's kind of anthropology has been around since the German idealist philosophers to whom its roots are traced. Franz Boas himself had intellectual roots in that tradition, taking for granted the inseparability of perceiver and perceived. Moreover, Boas conceived anthropology as ultimately psychological, i.e., as a mental science. This is surely part of the heritage of American anthropology!

Certainly, recent anthropology has had its vociferous proponents of the nomothetic approach, but never to the exclusion of the idiographic. In fact, I myself acquired a semiotic and ethnographic perspective on cultural and linguistic anthropology quite independently of Geertz, in whose work I later discovered a lyrical defense of ethnographic detail focusing on context and meaning. I have recently given considerable thought to the perceived contrast in traditions growing out of the major North American departments of anthropology, wondering if it is as clear as participants believe. At the University of Pennsylvania, my mentors were Dell Hymes, Anthony Wallace, and Ward Goodenough, whose more or less shared goals have much

in common with those of Geertz and David Schneider at Chicago. At Columbia, I would have been drawn to Robert Murphy, at Yale to Harold Conklin and Floyd Lounsbury, at Berkeley to John Gumperz. Geertz argues for a normal science of mentalist (or semiotic) research which has been widely valued in anthropology at least since the mid-'50s with the explicit formulation of the principles of componential analysis or ethno-science.

This context does not detract from the importance of Geertz's work. Rather, it underscores what is unique to Geertz—his rhetoric, i.e., the claim that the kind of anthropology he calls for is not only distinctive but superior to and incompatible with its more traditional and "scientific" counterpart. This rhetoric has been crucial to his emergence as a leader in this "elusive paradigm shift" which many commentators believe to be in process.

Paradigm shifts, however they may appear retrospectively, are rarely sudden or clearly delineated. Indeed, Shankman stresses that Geertz does not always practice what he preaches and is, in fact, frequently concerned with matters other than his potential status as harbinger of a "new" anthropology. I conclude that Geertz has not entirely chosen his role as prophet, that in large part it has been thrust upon him. Geertz himself has, in the process of doing ethnography, uncovered various inadequacies in the methods and conceptual apparatus of anthropology which are suggestive of the need for a major restructuring of the discipline. To the extent that he has convinced others, a new normal science is arising that sees itself in essential conflict with "traditional social science."

Shankman seems surprised that Geertz does not develop his theory rigorously, rather suggesting where his approach differs from existing ones. In fact, if he is the initiator of a new paradigm, this is entirely expectable. A paradigm statement and a program for action (research) based upon it necessarily precede formalization and detailed explication. Equally necessarily, there is a continuity with existing research. Many of Shankman's criticisms reflect this transitional stage of potential paradigm shift. Not surprisingly, Geertz finds work not within his framework uninteresting. As the perspective develops, it is predictable that it will turn away from theory and focus on demonstration in particular cases. At this stage, it is a question of involving others in this demonstration, of the emergence of a social network of anthropologists who share Geertz's concerns. Some of these scholars, if not Geertz himself, will no doubt answer Shankman's critique—which is as it should be. Any new paradigm is subject to evaluation by disciplinary peers. When the dust has settled, we may certainly expect to find that anthropology itself has changed, whether or not Geertz succeeds in replacing current social science with something entirely different.

Shankman, like Geertz, addresses his critique not specifically to anthropology but to the social sciences collectively. There is, of course, nothing startling about the continued convergence of social science theory and method growing from common roots and persisting within the same social milieu. Anthropology, however, has a potentially unique role in the emergence of a paradigm focused on context and meaning, simply because the "other" we traditionally study cannot be understood with any approximation to adequacy in terms of our own categories. This is not the first time anthropology has brought ethnographically based caution to the theories of other social sciences. The corollary, of course, is that anthropologists have habitually been more "interpretive" than their colleagues in other social science disciplines.

by ARIE DE RUIJTER

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Shankman exposes the weak spots in Geertz's studies, particularly the lack of explicit procedures for determining the sur-

plus-value of one interpretation compared with another and of grounds for preferring the interpretive to the nomothetic-analytical approach. His article would have been more valuable, however, if, in mapping Geertz's interpretive theoretical program, he had used Lakatos's (1970) model of rational reconstruction of research programs. Lakatos makes a broad distinction between the negative and positive heuristics of a program. The negative heuristics or hard core indicates what is irrefutable and must therefore be considered as the point of departure; it also tells us which research leads us nowhere. The positive heuristics forms a protective belt around this hard core. In this belt we find the rules of argumentation, the exposition of the conceptual apparatus, and the development of auxiliary hypotheses and techniques to safeguard the core from refutation. This positive heuristics is constantly being revised and may even be replaced completely. As the positive heuristics functions as a shock absorber for the hard core, it is generally speaking more flexible than the negative heuristics. Lakatos also gives some guidelines for the presentation of the dynamics of a research program. He identifies five phases of development: (1) the original problem; (2) the development of the negative and positive heuristics; (3) the problems which the program tries to solve consecutively; (4) the period of decline, alternatively called the "point of saturation"; and (5) the program which replaces the original program. Although this periodization idealizes and simplifies reality—events separated in this chronology appear to be combined in practice—and although the distinction between negative and positive heuristics (also called the level of presuppositions and the level of rules and means of argumentation) is not easy to make, I think the use of this model would have resulted in a more systematic, complete, and diachronic description of Geertz's program.

Perhaps, in his discussion of the relative merits of interpretation and explanation, Shankman could have drawn attention to attempts to reconcile the two approaches. A good example can be found in Lévi-Strauss's structuralism. Lévi-Strauss explicitly rejects the opposition adhered to by Geertz between an external, causal (explanatory) conception of science and an internal, interpretive (*verstehend*) one. In his opinion, the originality of anthropology consists not in opposing but in combining causal explanation and understanding. Social anthropology brings "to light an object which may be at the same time objectively very remote and subjectively very concrete, and whose causal explanation may be based on that understanding which is, for us, but a supplementary form of proof" (1966:114). According to Lévi-Strauss, at least three stages in the argumentation can be detected. His starting point is a historical and phenomenological analysis to get acquainted with the phenomena under study. Then he transforms these personal and conscious experiences into meta-empirical models with the help of which one can perform mental experiments. Finally, the various meta-empirical models have to be tested against empirical reality. However, I admit that Lévi-Strauss only gives outlines of a methodology and that he himself does not always follow these global rules in his concrete studies.

by DENIS DUTTON

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In Shankman's view, Geertz's interpretive theory, while it has generated "denser, more detailed ethnographies," has failed to give us a "major theoretical breakthrough." This might be a significant criticism if it were Geertz's intention to advance the prospects for anthropological theorizing. But this is not Geertz's intention. Moreover, the point of view presupposed by Shankman's article is representative of the very problem in anthropology which is addressed by the work of Geertz.

Must anthropological research be assessed according to whether it helps promote anthropology as a *science*? Shankman seems to think so, and he finds Geertz's work wanting in just

this respect. Hence his refrain that Geertz's writings lack "methodological rigor," that they do not yield "theoretical development," that they do not give us much in the way of "theoretical formulations." This is nonsense—not because Geertz *does* give us methodological rigor and theoretical formulations but because he consciously rejects the grand cross-cultural abstractions and the babble of scientific jargon that have afflicted anthropology for the last half-century.

Examples of this sort of stultification in anthropology can, alas, be multiplied endlessly, but one instance, taken directly from Shankman's article, will suffice here. Shankman claims that a "comparative, scientific approach to Balinese trance has a good deal to offer," and he pits Belo and Bourguignon against Geertz, who "describes the situation but is unable to explain it." And what does Shankman serve up to us as a model for a methodologically rigorous and theoretically fruitful example of explanation of the Balinese trance? It is the assertion that in engaging in wild behavior during trance states the Balinese are providing an outlet for their "unfulfilled desires." As Shankman glosses Bourguignon, this view has it that "trance is a widespread, culturally constituted defense mechanism." People slither on the ground, behave like animals, and eat excrement, and we are offered the "methodologically rigorous" explanation that they are acting out their unfulfilled desires. This is what Shankman deems the "scientific approach," though he does add that most stale of anthropological maxims, that "much more work needs to be done" on the subject. Much more indeed! It would be difficult to come up with an "explanation" more tired, more paltry, than one which invokes "unfulfilled desires" and "defense mechanism" to account for something as extraordinary as the trance in Bali.

Of course, there are similar and related phenomena all over the world, from Haiti to the Pentecostal church down the street from where I live, and Geertz has never denied that there is much to be learned from comparisons. But giving some queer mode of human conduct a fancy name ("dissociational state") and presenting it with the window dressing of science (the usual Freudian clichés, in this case) does not yet constitute a scientific explanation of that conduct. I find absolutely no *explanation* of the trance in Bali anywhere in Shankman's article, despite the fact that there is a lot of talk about "relating" trance to "social hierarchies," "socialization anxiety," and so forth.

In Geertz, on the other hand, one can find the imaginative reconstruction and description of cultural reality. He is an insightful observer and an excellent writer of English prose. If his discussions occasionally leave us with unanswered questions, so be it. Better to be left with honest questions than be fed phony answers which persuade only by mimicking the language of science.

by JOHANNES FABIAN

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Some of us who can call ourselves students and, perhaps, friends of Clifford Geertz have often been irritated by his haughty refusal to engage in polemical debates. Shankman's treatment may make us more understanding. I, too, shall refrain from arguing about specific issues and limit myself to a few general remarks intended to be useful.

1. Even though they are repeatedly qualified and mitigated, statements identifying Geertz as the founding father of "interpretive theory" in anthropology are historically incorrect. A genealogy should begin at least with Boas. If I may serve as an informant for the recent past, I taught a seminar on anthropology and hermeneutics at Northwestern University in 1968 because I had read, among others, Habermas (1967) and Hymes (1964). When I, with many others, opted for an interpretive approach I did so in the context of a critique of positivism and prompted by epistemological considerations regarding the nature of ethnography. To my knowledge, Geertz

never agreed with us that anthropology was in need of a radical critique. "Interpretive" signaled then and continues to do so now an approach commensurate with the nature of our evidence—*texts*, literally, not metaphorically. The (late) fruits of that "interpretive turn" are documented in numerous articles and some recent books (Fabian 1979, Crapanzano 1980, Rosaldo 1980, Boon 1982, Dwyer 1982, Tedlock 1983—a list that is far from complete; see also, for a recent attempt at a review, Marcus and Cushman 1982).

2. Geertz cannot be understood (or discredited) by fabrications regarding his "German idealist" ancestors. Why insinuate illegitimate parents when he often and openly acknowledges his indebtedness to Susanne K. Langer, Kenneth Burke, G. Ryle, and others who are above suspicion of German idealism?

3. Just as a matter of keeping communication lines open, advocates of "conventional science" who choose Geertz as a target might be well advised to ponder his popularity among bona fide theoreticians and historians of science (see, for instance, Elkana 1981:10–12, 70).

4. Shankman thinks that interpretive theory's claim to "superiority" is the "key issue." How can this be if (a) within the interpretive approach, however ill-defined, demonstrated superiority is rejected as naively positivistic and (b) within scientific theorizing—apparently with the exception of "scientific anthropology"—hardly anyone upholds explanatory superiority as the major, let alone the sole legitimate, criterion of choice among competing explanations (need I refer to Popper, Kuhn, and Feyerabend)?

5. Geertz, I think, continues to believe in the possibility of anthropology as a "science of symbols" (an appraisal of his work cannot dismiss as relatively unimportant his essays on ideology, religion, and, more recently, art as cultural *systems*). His unusually candid and intelligent admissions regarding the difficulties of maintaining a scientific stance in view of the nature of our empirical evidence are to his credit. He has always remained aloof from the political aspects of a debate between critical and "conventional-scientific" anthropology, but he provides those who take a stand against scientism with powerful arguments.

by CLAIRE R. FARRER

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Shankman asks us if "social scientists are ready to make the major paradigm shift" that Geertz advocates. It has been made and is continuing—not just in anthropology but in folkloristics as well (Bauman and Abrahams 1981).

Genres are important when one needs artificial categories to facilitate discourse upon their contents. Genres emerge, however, from context and interactions rather than existing a priori. We fix them, applying arbitrary rules of classification, and then seem upset when they shift and meanings change as a result of use, whether the switch is for purposes of strategy, information, play, or whatever. Rather than fussing about genres' blurring, perhaps we gain more by attending to the point of blur. Usage often upsets the categories that, we have now forgotten, were arbitrary in genesis anyway. Kirshenblatt-Gimblett (1975) illustrates elegantly that a parable means one thing in Context A and a very different thing in Context B. Is the genre "parable" at fault? Hardly. Is the context lacking? No. Is the performer utilizing items from the genre, or the genre itself, improperly and so causing blurring? No, the performer's perspective is understood, accepted, and validated through the continuing interaction. But where, then, is *the* reality Shankman wants us to find as the goal of anthropological analysis and understanding?

In the field we find that the genres, structures, institutions, and so on do indeed exist—at one level of analysis. But to

expect them, therefore, to carry all the burden of thought, meaning, and analysis is to stress them beyond the bounds of reason. They are, after all, artificial constructs which facilitate action or thought for the moment.

Shankman contends that interpretive analysis should submit to the procedures of behavioristic or scientific predictability and verification. Yet scientists themselves allow for unpredictability and difficulty of verification, to say nothing of the elusiveness of a reality (cf. Jeans 1933, Hofstadter 1980). Shankman wants us to see light as wave or particle. He asks us to confine cultural analysis to stated rules and predictable outcomes, to espouse the science of Baconian theory and experimentation as the right way to proceed with analysis and understanding. Geertz argues with the new physicists who say light is both wave and particle depending upon one's viewpoint and how the experiment is set up. The context and interpretation, and the content as well, thus become crucial, while rules and prediction fade from interest. The emerging nature of both the data themselves and the understanding of them require a processual orientation rather than a product perspective based upon cause and effect or stimulus and response.

Empiricists are never satisfied with that which cannot be replicated and measured, thus controlled and subjected to manipulations, proofs, and predictions. Interpretivists say control is illusion. Must those of us who are scientific and empirical impugn the intelligence or capability of those of us who are interpretivist and humanistic? Must we insist that only work within one tradition of inquiry is sensible? Why cannot the parable mean different things at different times? And why is it necessary for us to demand that all those meanings be specified and reduced to rules predicting where they are likely to emerge and how?

Why can we not allow both product and process orientations? Each is a productive line of research; both yield insights and understanding, albeit on differing levels of analysis and using different tools to reach the ends. Some few among us may be able to function productively in both paradigms; most of us will opt for one or the other.

Empirical scientism is appropriate and informative. Interpretive explanation is appropriate and informative. Shankman wants us to believe the former is better, more true, than the latter and argues circularly that scientism is better by calling upon validation through empirical scientism. Such eristics serve little purpose. Is empiricism different from interpretation? Certainly. Is empiricism better than interpretation? I remain unconvinced.

by A. D. FISHER

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Shankman's essay begins with a recapitulation of Geertz's justifications for interpretive cultural theory. That this does not help readers grasp what is at issue in "Geertzian" interpretive theory is not Shankman's fault. The problem lies in "Geertzian" theory and its exposition.

For example, Geertz espouses a concept of culture which both is made up of and describes "the webs of significance" created by man. Because just before this he has asserted that his concept of culture is a semiotic one, one does not know how literally one is intended to take these webs of significance.

Earlier still, Geertz has pointed to "the conceptual morass into which the Tylolean kind of *pot-au-feu* theorizing about culture can lead. . . , [a] sort of theoretical diffusion" (1973a:4). It is not clear whether he is alluding here to the diffusion of Tylolean thought from Britain to the U.S.A. or saying that neo-Tylolean concepts of culture are diffuse. One suspects, however, that neither is the case. The historical tone of the discussion seems a distraction. Geertz wants to dazzle us when he pulls "thick description" from under the gaudy handkerchief

of ethnographic description. In performing this trick he is trying to convince us that what we see, interpretive discourse, is what really is—or should be. This, of course, is why it's so hard to grasp interpretive theory.

People like Shankman and me may never be able to learn the illusion well enough to demonstrate it in public, but we recognize it and we think we know what's going on with the hanky and what's under it. A vulgar materialist like me senses more in the trick. By dealing with history as he does Geertz reduces its significance in modern ethnographic theory. Historically it is quite conceivable that "the culture concept, as defined by Tylor and promoted by the Boasians in the United States, makes more sense as ideology than as empirical science. . . . the concept can be better understood as part of a belief system than as a scientific discovery" (Moore 1974:537). American anthropology had to build the concept of culture as a political ideology that would give it some room in which to operate in American universities (Moore 1974:544). Anthropologists had to develop course offerings and scholarly research programmes. They also had to legitimate the use of their informants' views of events as social science, to present this informants' knowledge as something more than merely out of the ordinary while still staying close to what the informants actually said.

"Geertz's approach acknowledges that its descriptions of other cultures are *already* interpretations of their interpretations," says Shankman. Each ethnographer has had to provide some interpretation for his informants' discourse to make it compatible with his (or her) own understandings or observations. This interpretation is not all one-way, however. As Cannizzo (1983:54) has said,

It is surely no accident, not even a peculiarity of his field methods, that "the outstanding feature of Boas' Kwakiutl and Northwest Coast work is the emphasis upon the symbolic aspects of the cultures." While this selection presumably represents Boas' own concerns, mythology, language, and art also constitute the areas in which [George] Hunt would necessarily need to be conversant if he and his heritage were to be presented, recognized, validated and legitimated

Geertz is supremely aware of the complexity of "such structures of meaning," the symbols of culture. However, his interpretive imagery avoids discussion of other sources of complexity, such as the relations between Boas and Hunt, between Boas, the American Museum of Natural History, and the Smithsonian Institution, and between Boas and his university colleagues. There were also the relations between Boas, Hunt, the Kwakiutl, and the Canadian "potlatch law." There is something in the history of the Boasians' concept of culture that you can get your hands around. It is part of institutional political ideology. It is complex, but it is neither "morass" nor "muddle" (as Geertz labels culture theory). Culture makes sense as ideology rather than empirical datum. It makes sense as one outcome of the relations between academic ethnographers and the interpreter/informants whose words they, together, transform into ethnologically respectable discourse. The Geertzian view of culture as webs of significance is not fair exchange for the older belief system.

by L. E. A. HOWE

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Although Shankman is right to criticize much of Geertz's theoretical programme, there are a number of points which remain debatable. To begin with, there is implicit in the author's argument a division between interpretive theory and "thick description," on the one hand, and comparative, scientific explanation, on the other. But "thick description," in the sense of detailed documentation of events and circumstances in small-scale social situations, is the cornerstone of much anthropology

whatever its theoretical framework (network analysis, transactionalism, ethnomethodology, dramaturgy, extended case studies, etc.), and so to set up interpretive theory with its semiotic bent as the only alternative to comparative, so-called scientific explanation is spurious and tendentious. Moreover, Shankman's choice of case studies is interesting, since both (but more especially the *Negara* theme) have been criticized for their lack of supporting evidence. Had he chosen other examples, such as the Balinese cockfight (Geertz 1973*d*) or the Javanese funeral (Geertz 1973*f*), I think he would have been hard pressed to supply a superior "scientific" explanation.

The crucial point is the nature and extent of the evidence provided in support of the arguments advanced, whether the framework adopted is a Geertzian one or cross-cultural and comparative. I have no trouble, therefore, agreeing with Shankman that the lack of material evidence in Geertz's studies of Balinese trance and the theatre state is a major deficiency which calls into question the validity of the analyses. As far as *Negara* is concerned, the ethnography is inadequate and the accompanying analysis, in my opinion, misguided (cf. Howe 1982). Shankman's procedure for this case takes the form of simply substituting a Western political-science model for the interpretive approach, and my own view is that this is the correct thing to do.

His method in the other case study, however, is altogether different, since he attempts to "explain" Balinese trance by locating it in a body of comparative material collected by Bourguignon. The problem here is that Shankman is content to use, quite uncritically, the results of Geertz's other interpretive studies on Balinese symbolism, stratification, and the stylization of social life in order to support his own argument about trance. In fact, much of what Geertz has to say on these subjects is highly contentious. For example, the geographical distribution of trance in Bali is extremely uneven: residents of one village may indulge in it to a considerable extent, those in a contiguous one hardly ever. Also there are many different forms of trance, including possession by spirits and by individuals acting as intermediaries; there is mass trance, individual trance, and explicitly feigned trance. Moreover, rather than trance states' providing a means by which impotent and fearful people can achieve feelings of power and prestige, as Shankman argues, it is usually only those in specialist roles and particular statuses (priests, mediums, virgins, old people, etc.) who are expected to become entranced. Furthermore, the Balinese do not need "ritualized dissociation" to provide them with a set of alternative roles, since social life is not nearly so rigidly stylized and fixed as Geertz maintains (Howe 1980, n.d.). Again, if the general configuration of Balinese trance can be predicted by Bourguignon's cross-cultural propositions, why is there such massive variation within Bali, and why is there so little trance in Java, another highly complex, stratified, and agricultural society? Finally, in the village in which I resided in Bali I managed to document only three cases of trance, involving one young priest and two old women who were wives of priests. If trance is a "widespread, culturally constituted defense mechanism," then the other 1,200 or so inhabitants were profoundly unprotected.

Shankman criticizes Geertz on the grounds of "logical inconsistency and overstatement, argument by assertion rather than clearly demonstrated links between theory and data, and failure to review and consider available, plausible theoretical alternatives." I cannot dispute that, but it seems to apply equally well to much of Shankman's own contribution.

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In their efforts to come to grips with the kind of creatures humans are, should anthropologists look to the physical sci-

ences for predictive models or should they turn to the humanities for interpretive insights? This question, as Shankman points out in his clearly written, important contribution to the debate, is an old one. Despite its age, however, the question is alive and well, with reductionist sociobiology at one end contesting with holistic symbolic anthropology at the other.

The debate goes on unresolved because of its subject matter. The human primate continues to bedazzle us with its contradictions. We cannot decide if humans are a part of nature in the same manner that rocks, trees, and caterpillars are or if they are somehow out of nature and exist in the bodiless realm of the mind. We cannot make this decision and resolve the debate because humans are obviously part of nature and equally obviously not. Humans are, as Miguel de Unamuno cried out, flesh and bone creatures cursed with an appetite for divinity (1972, 1974).

In wrestling with the unresolved question, we would be foolishly myopic to ignore the contributions of what Shankman calls conventional social science. Yet, the interpretive approach does stand apart from the rest in its forthright determination to take culture, a conventional concept, seriously. What is exciting about interpretive anthropology is that it combines the concept of culture as symbol, that which is not available to the physical senses (White 1959), with the sweaty give-and-take of social interaction. In so doing, it produces an image of concrete humans trafficking in ghostly happenings—an image not far removed from Unamuno's god-hungry creature.

With its concept of culture as context, as the web of significance we ourselves spin, interpretive anthropology directly addresses the world in which we humans are. Conventional social science, in its search for underlying causes, explains away that world and in so doing alienates us from it and destroys its magic. When we are possessed, we do not exist within the category of psychological defense mechanisms. Instead, we are in the company of gods, who are all the more real for being human creations.

To reduce this world of contextual webs, of ghostly presences, to the cause-and-effect language of conventional social science is to risk seriously misunderstanding our mode of being. As Geertz (1973*e*) makes clear in one of his most thoughtful articles, without that world of symboling, without culture, we humans would not be another ape, able to do a few ingenious tricks; we would simply not be, at all.

To be sure, interpretive science has its own risks: of being overly precious, of being obscure, of turning a thick interpretation into a dense one. The issue of verification is indeed important. Yet, in such matters lies the strength of interpretive anthropology. It offers the realism of continual assessment in place of the illusion of final proof. Even with its defects, the interpretive approach, by combining the social with the cultural, power with poetry, directs us toward the task of making anthropology, the science of humanity, a truly human science.

by ROBIN RIDINGTON

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Shankman's article is unusual in being about the ideas of a particular living anthropologist rather than about a body of information or area of inquiry associated with a variety of names. As an anthropologist, I wonder why some ideas stand by themselves, while others depend on a single author. Why are some identified topically, while others take the name of their chief advocates? Surely, the answer to this question has something to do with the dynamics of status within our profession. As Shankman points out, interpretive theory is weak in part because its chief proponent, Geertz, indulges in "argument

by assertion rather than clearly demonstrated links between theory and data." Such argument depends more on the status of its author than on its enduring objective value. Theory that seems flashy today may appear dated and ethnocentric in the context of another generation's status symbols.

Why does the writing of Geertz attract some members of our profession and frustrate others? Shankman identifies two main objectives of anthropology that seem to attract people of different persuasions. One is to explain cultural phenomena through comparison and generalization. Inquiry with this objective suggests laws or causes, or at least educated guesses, linking the particular to more general classes of events. The second is to translate meanings from one culture to another. Instead of relating information from another culture to absolute standards built into the anthropologist's culture of science, the anthropologist with this objective relates it to aesthetic values, or at least to a sense of style. The first approach, to use images of a recent academic generation, is *etic*, the second *emic*.

According to Shankman, Geertz finds science uninteresting. If this is true, then his interpretive anthropology must prove itself to be of enduring aesthetic value, rather than of value only as a passing style. Whether he will be read and recommended in years to come, supposing we escape the evolutionary destiny likely to warring sovereign states with nuclear kill devices, depends on his ability to translate meanings vividly and powerfully from one culture to another. I am convinced that clear thought, expressed in powerful prose and based on sound observation, stands the test of time. I still read Fletcher and LaFlesche, Tylor, Morgan and Malinowski, Rasmussen, Speck, Sapir, Hallowell, and even the lean and hungry Marvin Harris, because they write observations of pleasing originality, clarity, and intelligence.

I have more than once put down *The Interpretation of Cultures* in favor of more engaging reading. Perhaps "thick description" itself is more interesting than the argument for it. Geertzian theoretical argument leaves me feeling that the Emperor is naked without being aware of it. Future generations, reading him through different lenses, will know if his "thick description" satisfies its promise to the imagination. If it is more vivid and informative than other work of our generation, it will continue to be read. In the end, Geertz stands or falls on his style of expression rather than on his stylish attraction within a single generation's status hierarchy.

Beyond our divided interests in cause and relative meaning, anthropologists have always sought out "the other" in order to know ourselves. We know ourselves through science, and we know ourselves through our encounters with lives other than our own. The creative interpreter rings harmonies between cultures. Interpretation is intrinsically comparative. I am confident that languages of interpretation will continue to spring from the informed imagination of anthropologists, whether or not the name of Geertz finds a place with those of Boas and Kroeber in the annals of anthropological genealogy.

by STAN WILK

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Shankman is correct in associating Geertz's interpretive approach with the *Geisteswissenschaften* tradition. However, the writings of Geertz should be more immediately linked with the writings of such humanistic anthropologists as Benedict, Sapir, and Boas, who have sought to carry out the project of the "idealists" in anthropology. Perhaps Geertz's own failure to acknowledge, in any meaningful way, his place in the idiographic tradition of American anthropology has caused this oversight. Sapir's (1917) reply to Kroeber on the "superorganic" is largely based on H. Rickert's *Die Grenzen der naturwissenschaftlichen Begriffsbildung: Eine Einleitung in die historischen Wissenschaften*. In it he says that "historical science

. . . differs from natural science, either wholly or as regards relative emphasis, in its adherence to the real world of phenomena, not, like the latter, to the simplified and abstract world of ideal concepts. It strives to value the unique or individual, not the universal" (p. 446). Benedict, in *Patterns of Culture*, takes inspiration from Wilhelm Dilthey and Oswald Spengler and stresses the incommensurability of cultures understood holistically: "Between two wholes there is a discontinuity in kind, and any understanding must take account of their different natures, over and above a recognition of the similar elements that have entered into the two" (1934:52). Thus while Geertz may be the leading contemporary proponent of interpretive theory, he has not come upon the scene *ex nihilo*. If he wishes to ignore in print his intellectual ancestors, Shankman cannot if he wishes to argue convincingly against interpretive theory in anthropology.

I find Shankman's discussion of interpretive versus scientific approaches unconvincing. He has not shown that, under the specific conditions set down by human consciousness, the pursuit of the "hermeneutic-spiritual knowing" of the German idealist philosophers is not proper science, if we mean by that naturalistic logico-empirical knowledge. A serious discussion of this issue might have caused him to see that interpretive science is worthy of more than just a "special niche" in anthropology. Moreover, his assertion that the issue between positivist natural science models and *Geisteswissenschaften* is not incompatibility or incommensurability, "since Geertz has compared them and found the interpretive approach superior," is ludicrous. Geertz is not to be taken as the final arbiter in such matters, and, beyond this, I do not know of any such theoretical discussion, nor does Shankman cite one. Shankman's own failure to deal directly with this fundamental issue vitiates his own "test" of whether *Geisteswissenschaften* "is superior to conventional social science." Geertz's approach is more conventional than he would have us believe or Shankman realizes, and issues of superiority are sterile in comparison to issues of complementarity.

Reply

by PAUL SHANKMAN

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The enigmatic silence of Clifford Geertz hovers over this commentary like a cloud. Perhaps at some time in the near future he will respond to this article and the comments on it. In the meantime, I would like to thank the commentators and address several points that they raise.

Commentators such as Dutton, Fabian, and Farrer wonder whether it is fair to evaluate Geertz's work by scientific criteria, since it is not his intention to be scientific. In conventional social science, the accuracy of a description and the verification of an explanation can be assessed independently of an author's intentions. Moreover, that interpretive theory and conventional social science are different ways of knowing does not make them exempt from mutual examination. Thus it is no more unfair to evaluate Geertz's work by canons of conventional social science than it is for Geertz to evaluate conventional social science from an interpretive perspective. Questions about the accuracy of a "thick description" or the verification of an interpretation by evidence are relevant for both interpretivists and conventional social scientists, as Connor and Howe point out in their critiques of Geertz's Balinese ethnography. These are not the only questions to be asked, but they are important ones.

Some reviewers note that, while interpretive theory and conventional social science are different ways of knowing, they can be complementary. Geertz, however, does not regard complementarity as either possible or useful. In his latest work,

Local Knowledge (1983), he argues that a return to a conventional conception of social science is "highly improbable" (p. 16), dismissing it as "a haze of forceless generalities and false comforts" (p. 234). Geertz's opinion to the contrary, complementarity is possible under certain conditions. Interpretive theory and "thick description" usually involve "embellished description" and idiographic explanation from which nomothetic explanations can be derived and on which they can be tested (Erasmus and Smith 1967). Hence the two approaches can be complementary, but only as long as interpretivists like Geertz make no claim that their approach is superior to conventional social science or immune from scientific assessment. Such claims require the explicit criteria for comparison and evaluation that Geertz does not present. In the absence of such criteria, his contention that scientific explanations are less appropriate for cultural phenomena than interpretive explanations remains unconvincing.

Farrer discusses the appropriateness of a scientific approach when dealing with different interpretations of reality. I agree with her that science is not the only way of knowing—that there can be different interpretations of reality. Like Farrer, I believe that complete objectivity is impossible; but this does not mean abandoning the search for objective knowledge or conceding that all versions of reality are equally true. What is necessary, according to interpretivist Rosaldo (1982:198), is "ways of moving back and forth between an actor's subjective interpretation and a set of objective determinants." For example, Farrer notes that a parable can mean different things in different contexts, and to her this plurality of meanings suggests that there is no single interpretation of reality. Does this mean that objective conditions have no influence on the interpretation of the parable? Hardly. The context in which the parable is told may predict some of its subjective meanings for those who interpret it. Contextual studies of kinship, myth, and religion indicate that the absence of a single meaning or interpretation does not rule out the scientific study of meanings and interpretations (e.g., Keesing 1982, Harris 1979). Thus Farrer's approach to parables does not refute conventional social science; it may complement it. For Farrer, like Geertz, conventional social science is less interesting than interpretive theory, not necessarily less appropriate.

Other doubts about the applicability of a scientific approach to cultural phenomena are cogently summarized by Richardson, who notes that science can reduce, dehumanize, alienate, misrender, and misunderstand our mode of being. While these possibilities exist, they may be overemphasized in the rush to applaud what Fabian calls Geertz's "stand against scientism" or what Richardson calls the move toward a "truly human science." Since Geertz himself has warned of the multiple hazards of interpretation, it seems that neither approach is more inherently "human." The implication that a scientific approach has not contributed to a truly human science or that it is incapable of contributing further has little foundation. Science was instrumental in refuting racial explanations, in reworking notions about human nature, and in revising ideas about the roles of men and women; scientific method has helped physical anthropologists, archaeologists, and cultural anthropologists place the human career in evolutionary perspective. And these significant contributions to the understanding of humankind were made possible by a reductionist, nomothetic paradigm.

Darnell and Farrer believe that an interpretive paradigm is emerging, but relatively few findings are cited that support this. More commonly cited are Geertz's intentions, his intellectual pedigree, his alleged kinship with the new physics, and his writing ability. Such comments may unintentionally give support to critics who wonder to what extent the emergence of Geertz's program is based on the dynamics of professional socialization and status within anthropology and the social sciences rather than on the findings on which presumably paradigms should rise and fall.

The case of the Balinese trance has drawn a number of comments. My point was that Geertz's description of Balinese trance and Bourguignon's cross-cultural theory of trance were not antithetical or mutually exclusive. Geertz's ethnographic account, however, is challenged by Connor and Howe, whose criticisms are especially telling given the failure of interpretive theory to distinguish between description and interpretation. The weaknesses of Geertz's description, though, do not jeopardize Bourguignon's theory of trance, which was derived independently from and tested on case material other than Geertz's. At the cross-cultural level, Balinese evidence generally supports her theory.

Howe notes the geographical variation in trance within Bali, asks why this variation exists, and criticizes Bourguignon's explanation as inadequate in this regard. Intracultural variation is worth explaining, but cross-cultural and intracultural explanation are different levels of explanation. If Howe has an explanation for intracultural trance variation in Bali, he should provide it; the same is true for the differences he notes between Java and Bali. Merely to note the existence of variation does little to further its understanding.

Dutton finds "no explanation" of Balinese trance in my article, only "phony answers." He is particularly critical of the intervening psychological mechanism that Bourguignon suggests in her explanation of Balinese trance, labeling it a Freudian cliché. Unfortunately, Dutton does not examine the major ecological, structural, and socialization variables central to Bourguignon's explanation. By taking a minor, intervening psychological mechanism out of its explanatory context, he gives it a causal significance that it does not have. One need not agree with Bourguignon about this mechanism to appreciate her broader explanation. If Dutton wishes to challenge her explanation, then he needs to contest the major variables and the results. As for Dutton's observation that in Geertz's work there is "imaginative reconstruction and description of cultural reality," we need only note Connor's and Howe's criticisms of Geertz's basic ethnography. If they are correct, Geertz's description is more "imaginative reconstruction" than cultural reality. There is a difference.

Ágh and Howe suggest that, had I chosen to analyze different cases, such as agricultural involution in Indonesia, the Balinese cockfight, or the Javanese funeral, Geertz's arguments would be less vulnerable to criticism. *Agricultural Involvement*, however, is, as I noted, conventional social science, while the Balinese cockfight and the Javanese funeral have already been reanalyzed by Roseberry (1982) and Gastil (1961:1282).

Even if particular cases were strengthened, the overall interpretive program would remain rather limited. Recently Geertz has reiterated that the goal of interpretive study is "to come to terms with the diversity of the ways human beings construct their lives in the act of leading them" (1983:16) and that its major lesson is cultural relativism (pp. 16, 234). While perfectly acceptable, this program need not encompass anthropology's sole aim or entire message. Although anthropologists are interested in diversity, they may also be interested in uniformity. As Bourguignon comments, we cannot know what is truly unique about a particular culture without some comparative basis. The study of cultural diversity does not rule out the possibility of nomothetic explanations of cultural similarities and differences, and an informed relativism would not be hindered by the systematic analysis and explanation of these similarities and differences. Moreover, while few would disagree with Geertz that it is important to know how humans construct their lives, there are limits to self-knowledge. A whole range of external factors that impinge upon the social construction of reality is not addressed by Geertz (Asad 1983, Harris 1980). An anthropology that included nomothetic as well as idiographic approaches and etic as well as emic factors would be

less narrow. While the interpretive program is often presented as a broadening perspective, it is actually quite limited and may be difficult to reconcile with Geertz's call for a general refiguration of social thought.

As Geertz's interpretive program has evolved, his style seems to have taken on the "late Gothic" quality he describes elsewhere (1963:82), becoming more intricate, with greater internal elaboration of the basic pattern and "unending virtuosity." This involutory style could yield the same diminishing returns on intellectual terrain as it did in the fields of Java. In the 1960s and 1970s—the formative years of interpretive theory—Geertz's style of argument summarily devalued scientific approaches while offering examples to support his interpretive approach. By employing very general criticisms of social science and very particular cases for interpretation, Geertz was able to avoid direct controversy. But much has happened in conventional social science in the past decade that needs to be directly addressed by interpretivists, and criticism leveled at the interpretive program can no longer be avoided. The style of argument that earlier illuminated new ways of thinking about cultural phenomena is no longer as persuasive. Geertz himself admits that it is evasive. He explains that he has no "desire to disguise evasion as some new form of depth or to turn one's back on the claims of reason. It is the result of not knowing, in so uncertain an undertaking, quite where to begin, or, having anyhow begun, which way to move. Argument grows oblique, and language with it, because the more orderly and straightforward a particular course looks, the more it seems ill-advised" (1983:6).

At the risk of being straightforward, I would suggest that Geertz begin by attempting to refute the criticisms of his theory and case material raised in these pages. If the claims he makes for the interpretive program are to be persuasive, interpretive and conventional social science approaches should be compared using the same case material. An open exchange of ideas and evidence of this kind would be neither evasive nor unreasonable.

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Calendar

1984

- June 26-30. International Conference on Organizational Symbolism and Corporate Culture, Lund, Sweden. Write: P. O. Berg, Department of Business Administration, University of Lund, P.O. Box 5163, S-220 05 Lund, Sweden.
- July 16-20. 5th International Conference on Methods of Dialectology, Victoria, B.C., Canada. Write: H. J. Warken-tyne, Department of Linguistics, University of Victoria, Victoria, B.C., Canada V8W 2Y2.
- July 23-27. International Decade of Research in Singing, 4th conference, Honolulu, Hawaii, U.S.A. Themes: Interdisciplinary Approaches to Voice Research; Instrumentation and Methodologies for Voice Research; Vocal Pedagogy: Cross-cultural Approaches; Aspects of Vocal Style; Vocal Traditions of Asia and the Pacific Region; Social Context of Singing. Write: William Feltz, Culture Learning Institute, East-West Center, Honolulu, Hawaii, 96848, U.S.A., or, for travel/hotel information, Hawaii 2000 Seminars, 1415 Victoria St., Suite 401, Honolulu, Hawaii 96822, U.S.A.
- August 17-19. International Conference and Workshop on Human vs. Natural Bone Modification, Carson City, Nev., U.S.A. Write: Donald R. Tuohy, Local Arrangements Chairman, Nevada State Museum, Capitol City Complex, Carson City, Nev. 89701, U.S.A.
- August 27-September 1. International Association for Mass Communication Research, 14th conference and general assembly, Prague, Czechoslovakia. Theme: Social Communication and Global Problems. Write: James D. Halloran, Centre for Mass Communication Research, University of Leicester, 104 Regent Rd., Leicester LE1 7LT, England.
- August 27-30. 2d International Colloquium on Folklore in Africa Today, Budapest, Hungary. Themes: The Role of Traditional Cultures in the Life of Young People in Africa Today, African Folklore from the Perspective of the Theory of Genres, Problems of Collection of Folklore in Africa, Results and Recent Problems of Oral History Research, Museology in Africa, Peculiarities of Traditional African Culture in the Perspective of Culture Theory. Write: Szilárd Biernaczky, Department of Folklore, Loránd Eötvös University, H-1364 pf. 107 Budapest V, Hungary.
- August 29-September 1. International Association for Cross-cultural Psychology, 7th Congress, Acapulco, Mexico. Write: Isabel Reyes-Lagunes de Carrillo, FC de Cuernavaca 1127, Lomas San Angel Inn, 01790 México, D.F., Mexico.
- September 2-7. 2d International Congress on Traditional Asian Medicine, Surabaya, Indonesia. Write: G. J. Meulenbeld, Institute of Indian Studies, Grote Kruisstraat 2/I, Groningen, The Netherlands.
- September 4-8. International Institute of Sociology, 27th world congress, Seattle, Wash., U.S.A. Theme: The Task of Sociology in the World Crisis. Write: Marie L. Borgatta, IIS World Congress Coordinator, Department of Sociology, DK40, University of Washington, Seattle, Wash. 98195, U.S.A.
- September. European Anthropological Association, 4th Congress, Florence, Italy. Write: European Anthropological Association, % Institute of Anthropology, University of Florence, Villa la Loggia, Via Bolognese, 165, 50139 Florence, Italy.
- October 11-13. 9th European Studies Conference, Omaha, Nebr., U.S.A. Write: Peter Suzuki, Department of Public Administration/Urban Studies, or Patricia Kolasa, Department of Education Foundations, University of Nebraska at Omaha, Omaha, Nebr. 68182, U.S.A.
- November 14-18. American Anthropological Association, 83d annual meeting, Denver, Colo., U.S.A.

1985

- February 3-7. 5th Pacific Science Inter-Congress, Manila, Philippines. Theme: Transportation and Communication in the Pacific. Write: Paulo C. Campos, National Research Council of the Philippines, General Santos Ave., Bicutan, Tagig, Metro Manila, Philippines.
- June 25-29. 4th International Congress of Auxology, Montreal, P.Q., Canada. Theme: Growth and Development—Lifespan Perspective.
- July 8-12. 45th International Congress of Americanists, Bo-

gotá, Colombia. Deadline for approval of symposia October 1, 1984. Write: 45 Congreso Internacional de Americanistas, Universidad de los Andes, Apartado 4976, Bogotá, Colombia.
December 4–8. American Anthropological Association, 84th annual meeting, Washington, D.C., U.S.A. Deadline for all submissions April 1, 1985, on forms to be provided in November 1984 *Anthropological Newsletter*.

1986

August 25–29. 5th International Conference on Archaeozoology,

Bordeaux, France. Write: Pierre Ducos, V^e Conférence ICAZ, CREP, St. André de Cruzières, France.

September 1–7. International Union of Pre- and Protohistoric Sciences, 11th Congress, Southampton and London, England. Major themes: Cultural Attitudes toward Animals, Archaeology and the Very Remote Past, “Objectivity” in Archaeological Interpretation, Interactions between “Central” and “Peripheral” Cultures, Social and Economic Contexts of the Adoption of Similar Technological Elements in Different Parts of the World.

Our Readers Write

Vančata (CA 24:608), commenting on Blumenberg’s article on the evolution of the advanced hominid brain, points to the “marked adaptation to bipedality of the locomotor apparatus, especially the proximal femur and pelvis,” but I don’t find any mention of the obvious consequent major changes *above* the pelvis. Our viscera were meant to hang down from a more or less horizontal backbone and both the digestive system and *blood flow* to be fore-and-aft, while the sense of balance was originally attained at a 90° angle from the line of the backbone. To operate completely upright (eventually) required major adaptations above the pelvis also. I wish someone would someday address the possible causes and advantages and thereby the probable evolution of the *gross* biological distinction between men/apes and the monkey tribe—the total loss and internal reorganization of the tail in all of us in contrast to its great usefulness in them. When I think of the little Barbary apes scrambling up and down the rocky slopes in Gibraltar and North Africa, I am impressed by how far back this radical differentiation took place in the Primates and wonder why.

WILSON W. CROOK, JR.

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On October 20, 1983, our colleague Professor Salomón Nahmad, then director-general of the Instituto Nacional Indigenista, was arrested. The history of the arrest and the implications

of the case are detailed in a series of articles published in *Uno Más Uno*, November 7–11, 1983, by Dr. Arturo Warman, a member of the Committee in Support and Defense of Salomón Nahmad [and see also the open letter from this committee to the administration that appeared in *Excelsior* on October 26—Editor].

Salomón Nahmad has a clean record of indigenist work over a period of 25 years. He was attempting to move ahead with a new indigenist policy oriented toward participation and self-determination for indigenous peoples. These facts have aroused indignation and concern not only in indigenist and academic circles, but also among the indigenous population itself, which has spontaneously and very visibly manifested its support for Salomón Nahmad.

We think that it would be a great help to Salomón Nahmad if indigenists, anthropologists, social scientists, and persons and organizations concerned with the social problems of Latin America would send supportive telegrams requesting his freedom to the President of Mexico, Lic. Miguel de la Madrid, and to the Mexican embassies in their various countries. We will be grateful for readers’ support in this matter and ask that copies of any such action be sent to the Comité de Apoyo y Defensa de Salomón Nahmad at the address indicated below.

FERNANDO BENÍTEZ, MARGARITA NOLASCO,

RODOLFO STAVENHAGEN, and ARTURO WARMAN

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