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## Ethnopsychology and the Prospects for a Cultural Psychology

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The chapters in this volume are "pioneering" in the literal sense of the term. As a group, they open up several new lines of inquiry for psychological anthropologists to follow, and raise important questions concerning theory and methods. Like most pioneering efforts, each essay represents its own form of groping, of seeking to get an intellectual handle on the issues the endeavor brings to the fore. The preparadigmatic nature of work on the topic is much in evidence in the diversity of viewpoints represented. Indeed, at the meetings of the Association of Social Anthropology in Oceania (ASAO) which spawned the project, much discussion was devoted to attempts to define or circumscribe the domain of ethnopsychology— attempts that, not surprisingly, did not reach closure. Still, one senses that something of more than ordinary importance is at stake, that the effort strikes at the heart of some fundamental epistemological issues in the pursuit not only of anthropological research but in the general conduct of inquiry into the human condition.

### **HISTORICAL PERSPECTIVES**

In pondering the project, after having read initial drafts of the papers included in this volume as well as others not included, plus transcripts of discussions and a selection of already published materials on related topics,

I was reminded of an exchange I had as a graduate student at Stanford with Alfred Kroeber, who consented to meet with a group of us while he was visiting the Center for Advanced Study in the Behavioral Sciences. His talk consisted mainly of reminiscences associated with the development of anthropology as a discipline in the United States. He was frail in his dignity and was clearly approaching the end of his distinguished life. I can still remember my sense of self-congratulatory exuberance when I asked him about the future of anthropology—the ill-disguised implication being that he would be an active participant in shaping that future. He wisely deflected the question back to me. “The future,” he said, “will be determined by people like you. What are you interested in?” I answered that I was interested in culture and personality, to which he replied something like, “Oh that’s too bad. Culture and personality is a dead end.” He went on to relate how early in his career he had envisioned the development of a field within anthropology he called “social psychology,” but commented that what he had in mind was very different from the field that had come to be known by that name.

I would like to believe that Kroeber would have been pleased by the chapters in this volume. I suspect that he had in mind a psychology that was truly sensitive to cultural contexts, that was capable of reflecting the diverse patterns of personal experience in a less ethnocentric way. To be sure, the results still leave us far from a satisfactory formulation of such a social psychology, or more properly, a cultural psychology, but at least the challenge has been joined.

Perhaps the main concern that forms a common ground for the contributors to this volume, and unites them in spirit with Kroeber, is a shared dissatisfaction with Western psychology as pretender to a universal analytical framework for personal experience. To begin with, several of the participants explicitly question the conceptualization of personhood in Western psychology, with its strong emphasis on individualism, that is, on isolating the individual as the basic unit of analysis. Dramatizing the deficiencies of this approach stands as one of the more important contributions of this volume. By describing “folk theories” of human conduct in a variety of settings, even though limited to one geographical region, the authors make us aware of the wide array of alternatives available for categorizing human experience and for making sense of it. In particular, they have demonstrated the necessity for framing such efforts in their appropriate cultural contexts.

But as with all efforts to establish culturally sensitive frameworks, the task is fraught with profound obstacles. For one thing, the mere acts of selection and translation require a theory of psychological significance. Thus the very problem of delimiting a domain of ethnopsychology brings

us squarely up against the issues of how *we* define "ethno" and "psychology" which are themselves of a theoretical nature. For the most part such theory has been implicit, and has included as an operationalized premise contrast with idealized (only occasionally documented) Western forms. One reason for this is that Western culture constitutes the common referent for professional anthropologists, and it is by reflection against this template that we establish the bases for our communication with one another. This is a shortcoming that can be overcome, or at least ameliorated with time, as we and our audiences become more familiar with the true range of human variation (so that questions about American experience with the New Guinea Hageners' emotion of *popokl* and the Ifaluk emotion of *fago* can be meaningfully discussed, as Lutz implies). The papers in this book thus provide us with a modest step toward the goal of introducing into the Western frame of reference an expanded range of concepts and theoretical propositions, so that an increasingly inclusive array of experience can be incorporated into its repertoire. The aim is to release "scientific" psychology, which should be universal, from the shackles imposed on it by Western "folk" psychology, which is culturally constricted.

In this respect the authors follow in the time-honored tradition of their anthropological predecessors, begun in earnest by Malinowski, whose field data challenged the universality of the Oedipus complex, and Mead, who challenged received wisdom in American psychology concerning adolescent crises and the linkage between sex and temperament. Appropriately as far as this volume is concerned, the trail was blazed in the Pacific Islands. Contemporary anthropologists, in conjunction with such culturally sensitive psychologists as Michael Cole and his associates, continue to test and correct Western misconceptions about the patterning of human behavior, cognition, affect, and other aspects of experience. For the most part, however, such studies are conducted in a verification mode. They take propositions derived from Western psychology and explore their validity in a variety of cultural contexts, sometimes modifying the form of the proposition in the process. What distinguishes the papers in this volume is the goal of minimizing reliance on Western psychological notions in favor of exploring the cultural premises other people use to explain their experience as sentient human beings to themselves and to one another. In contrast to verification research, which strives toward delimiting acceptable scientific propositions, the immediate aim of ethnopsychology is to expand the repertoire of possibilities. The underlying logic is that only by examining a range of folk models from different societies will we come to see the limitations imposed on academic psychology by our cultural presuppositions.

Lutz alludes to one such presupposition, which is reflected in our preoccupation with scaling and ranking, particularly in trait psychology

The assumption is that all people are containers for the same basic qualities, but in differing degrees. If a quantitative imbalance occurs, such that an individual has too much X and/or too little Y, it makes a qualitative difference, that is, they are labeled differently as psychological types. Many of the categories we use in psychological analysis have this semantic shading.

It remains to be seen whether this perspective is widely shared or whether it merely signals a Western obsession with quantifying and ranking. While some cultures seem to share aspects of this perspective (e.g., portraying individuals in terms of degrees of maleness and femaleness), others seem to be less disposed toward quantifying the "substance" of humanity.

The contrast between Western "scientific" psychology and "ethno" psychology falls along dimensions of current debate that give the volume timely significance. The dimensions to which I refer are those of universalism versus particularism and its corollary (human) nature versus (cultural) nurture. These are, to be sure, ancient debates that seem to be resurrected in each generation and brought to center stage, only to fade again into the background of supposition for the majority of social scientists. Two recent publications have brought these issues squarely into focus within psychological anthropology. I am referring to Melford Spiro's (1982) reanalysis of Malinowski's Trobriand data relating to the Oedipus complex, and Derek Freeman's (1983) disputation of Margaret Mead's interpretation of adolescence in Samoa. Following in the wake of bitter debates focusing on sociobiology, these works are especially important challenges to cultural relativists.

In *Sex and Repression in Savage Society*, Malinowski analyzed relationships with the Trobriand family and concluded that the data did not support Freud's contention that the Oedipus complex was universal, rooted in the biology of psychosexual maturation. Freud appeared to take the European form of nuclear family for granted, which led Malinowski to question its applicability to societies, like the Trobriands, that were matrilineal in organization. According to Malinowski's description, it is the mother's brother who is the disciplinarian in Trobriand families, and it is toward the maternal uncle that hostility is directed. Fathers, in contrast, exert no special authority over children and there is, in Malinowski's view, no significant friction between father and son. Instead of libidinous desires within the family being directed toward the mother, Malinowski finds them to be directed toward sisters. Thus, he writes, "We might say that in the Oedipus complex there is the repressed desire to kill the father and marry the mother, while in the matrilineal society of the Trobriands the wish is to marry the sister and kill the maternal uncle" (1951:80-81). While not completely discounting the Freudian view of instincts—indeed he concedes that his

research confirms the teaching of psychoanalysis on several points—Malinowski concludes that sociological considerations drastically modify the expression of primal impulses. The main sociological forms he mentions in this regard are the regulation of infantile sexuality, the incest taboos, exogamy, apportionment of authority, and the type of household organization (1951:277).

Early criticisms of Malinowski's analysis came from psychoanalysts Jones (1925) and Roheim (1950), but as Spiro (1982:1) points out in the introduction to his critique, the main thesis was generally accepted by interested scholars of every persuasion. On the basis of his reanalysis of the Trobriand data, Spiro argues that not only are there no convincing grounds for Malinowski's contentions, there are grounds for believing that the Oedipus complex is even stronger in the Trobriands than it is in the West. Spiro concludes with a cross-cultural assessment in which he maintains that the evidence supports a view of the Oedipus complex as being universal in "structure" (i.e., consisting of the boy, his mother, and his father in every known society), while variable in "intensity" and ultimate resolution, or "outcomes."

Whereas Malinowski's interest in the Oedipus complex was tangential to his dominant sociological concerns, Mead went to Samoa specifically to address the question of whether the disturbances that vex adolescents in Western society are due to the nature of adolescence itself, derived from the physiological changes that occur at puberty, or are the consequences of particular social and cultural conditions. Following nine months of fieldwork, she concluded that adolescence in Samoa is not characterized by tension, emotional conflict, or rebelliousness. Her book, *Coming of Age in Samoa* (1928), became a key weapon in the arsenal of cultural relativists despite the well-founded skepticism of virtually everyone who knew something about Samoan society.

If any lingering doubts remained about the veracity of Mead's findings, they have been laid to rest by Derek Freeman's devastating critique, *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth* (1983). Using primarily behavioral data, Freeman makes a compelling case for a stressful adolescence in Samoa. He presents the case in part as a counter to cultural relativism, and reasserts the importance of taking into consideration biological universals as an underpinning for ethnographic interpretation. The main lesson to be learned from the case in Freeman's eyes, however, is the danger of taking into the field theoretical dogmas that result in such pronounced data selection that objective conclusions are virtually precluded.

It is of some interest that in neither of these instances has significant attention been paid to the indigenous people's perceptions of their own

psychological states. Gerber (chap. 4) gives us a glimpse of the insights into a more complex Samoan psychology to be derived from an ethnopsychological investigation of the ways people themselves construct their social and emotional lives.<sup>1</sup> Even though Malinowski and Mead assumed a theoretical posture of cultural relativism, their psychological frames of reference never shifted from their Western roots. They observed behavior and recorded verbal accounts in order to obtain evidence to "test" propositions explicitly formulated in Western psychological theories. Their critics examined the results and found them unconvincing, but they, too, have presented conclusions within the same psychological frameworks. Why, one is led to ask, have anthropologists, and psychological anthropologists in particular, been so reluctant to explore their subjects' views of such phenomena, whereas we readily recorded their theories of religion, kinship, and other social phenomena? Attempting to answer this question may help us to appreciate the significance of ethnopsychology as an intellectual endeavor. Is it perhaps a reflection of our own view of behavioral causality—a view that postulates mysterious inner forces beyond the awareness of the actors themselves? It seems to me that while we readily accept the notion that a people's religious concepts and beliefs affect their behavior (or at least help to explain ceremonial and ritual practices), and that social and political theories influence forms of social organization, we do not make the same assumptions vis-à-vis our subjects' psychological theories. I am reminded of a dictum I heard as a graduate student in psychological anthropology: the investigator should not ask the natives to explain their own behavior, this was his job as a scientist. At that time, during the late 1950s, the major concerns of both psychodynamic and behaviorist psychology, as well as psychological anthropology, were with explaining *behavior*. Cognition was rarely mentioned, and almost never as a valid object of study in and of itself.

Another reason ethnopsychology may have been delayed is that anthropologists in general were preoccupied during this period with documenting intracultural regularities, which led to ignoring issues of individual variation, a natural focus of psychological inquiry from a Western point of view. The major exception to these generalizations could be found in the writings of A. I. Hallowell, whose articles on the self and world view were read with great interest and admiration. But it was Hallowell's work with projective techniques that really caught our fancy as graduate students, for such techniques—especially the Rorschach and Thematic Apperception Test—would allow us, so we believed, to see beyond the cultural veneer into that "true" psychological domain, hidden from our subjects' own perceptions, of mysterious inner processes and symbolic forms. Thus, after receiving a modicum of training from George Spindler in the use of these techniques, I went to Hawaii in 1957, located a small sample of third

generation Japanese-American (Sansei) women, administered Rorschachs and TATs, and wrote a master's thesis.

I brought Rorschach and TAT cards with me when I began fieldwork in Rotuma in 1959, but my interest in them had waned somewhat and I quickly became disillusioned when Rotumans showed so little interest in playing my games. Besides, I became more and more interested in making sense out of their behavior by attempting to apprehend the logic of their culture. This reflected my exposure, just prior to going into the field, to the developing cognitive approach in cultural anthropology, especially as reflected in the early works of Ward Goodenough and Anthony Wallace. My goal, however, was still to make sense of Rotuman *behavior*, and I paid attention to Rotuman concepts and theories only insofar as they helped me to formulate my own theory of Rotuman culture. To a considerable extent, I was motivated by an aversion to psychiatric concepts, and other formulations, that portrayed subject populations in the same terms used to describe the mentally ill in Western society (e.g., shallow affect), for I was strongly convinced that Rotuman culture was an admirable one, and that the vast majority of Rotumans were models of mental health.

When I undertook my next research project, among Hawaiian-Americans on Oahu, this concern for avoiding ethnocentric misapplication of Western psychological theories was central to my research strategy. Hawaiian-Americans were portrayed by a variety of social agencies as a "culturally deprived" population whose extraordinarily high incidence of social problems derived from failures in proper socialization and other forms of deprivation. It was apparent that the stereotypes of Hawaiian-Americans were cast almost entirely within a framework of deficiency formulations, that is, the ways in which they failed to live up to Middle American value norms of achievement and success. There was virtually no appreciation for the possibility that alternate cultural values might be at work; indeed it was an explicit assumption by most people who advised me prior to entering the field that Hawaiian culture had been "dead" a long time, and that I would be dealing with "just another impoverished minority group."

As was the case with Rotuma, I was motivated to conceptualize Hawaiian-American behavior in other than deficiency terms, to describe it as much as possible in terms consistent with *their* perception of *their* goals and *their* strategies for obtaining them. My reason for rejecting deficiency formulations was the distortion that comes from using constructs that derive their substantive meaning from normative patterns within one group to characterize patterns in a group with quite different norms. By focusing on the ways in which culturally divergent groups deviate from mainstream Western patterns, such accounts generally fail to provide systematic infor-

mation about the normal, everyday aspects of social life and how they *are* organized; indeed, they generally contain far more information about the values and presuppositions of the middle-class American (and/or European) groups within which the constructs were developed, tested, and substantiated. In an article addressed to these issues which I wrote with sociologist Robert A. Scott, we reviewed the social science literature on minority groups and concluded that

a central objective of social science research must be to provide a clear sense of how the social life of a group is ordered. Even though profound frustrations exist, minority group members pursue various goals and sometimes achieve them; they actively engage in interpersonal relations from which they derive satisfaction; and they organize their activities in ways that are meaningful to themselves and those with whom they associate. A major flaw of deficiency formulations is that they neglect to document such behavior and activities and thereby fail to provide a firm basis for understanding the nature of social life among minority populations. (Howard and Scott 1981:114)

Still, I do not regard my research among Hawaiian-Americans as ethnopsychological in the sense used by the authors of this volume. In fact neither I nor my associates attempted to systematically explore our subjects' concepts of personhood or theories of behavior. We focused instead on aspects of behavior that were of most interest to *us*, rather than to them, and these in turn were dictated by the interests of such agencies as schools and the Department of Health, and although we did spend a great deal of time doing participant observation, our systematic data were elicited through the use of formal interviews and social psychological experiments. Nevertheless, we did aim our inquiries at identifying coping strategies—a distinctly cognitive concern—and we were explicitly concerned with the patterning of intracultural diversity (see Howard 1974). In these respects I see my Hawaiian research as headed toward an ethnopsychological perspective, although it was still very much tied to a Western psychological framework, albeit one that was far more accommodating to cultural diversity than previous versions.

At the time we began the Hawaiian research I formed a close personal and professional relationship with Robert Levy, who was in the midst of analyzing his Tahitian material. We shared many of the same biases and presuppositions, although I dare say we enjoyed debating finer points. I see his book, *Tahitians* (1973), as a vital link in the historical chain leading toward ethnopsychology. Levy made extensive efforts to elicit indigenous categories of thought and expression, and much of his analysis is based on exploring the implications of these concepts for Tahitian systems of

thought, feelings, and action. Yet his interpretations are also explicitly informed by Western psychological and cultural theories.

I see both my own work in Hawaii and Levy's in Tahiti as attempts to contribute to the formulation of a culturally sensitive, though universally applicable, "scientific" psychology. Neither of us was prepared to surrender those tenets of Western psychology we felt might form the foundation of such a universal framework. What distinguishes ethnopsychology from our approaches is a commitment, in theory at least, to loosening the grip of Western construct on psychological theorizing. From this standpoint ethnopsychology falls much further toward the particularistic end of the universalism-particularistic continuum. It also falls within the broader camp of social science inquiry described by Howard and Scott (1981:143) as "naturalism," which they contrast with the hypothetico-deduction approaches that have dominated social science for the past century. According to Howard and Scott:

When a naturalistic approach is adopted, concepts are derived differently. The commitment of naturalism is to remain as true as possible to phenomena and their nature. Its loyalty is to the experiential world (Matza, 1969, pp. 1-10). The aim of naturalistic accounts is to describe a phenomenon in a manner that maintains the phenomenon's integrity rather than the integrity of a particular theoretical viewpoint. A basic assumption of the naturalistic approach is that human behavior is purposeful, and that persons participate in defining social reality in an active way. For this reason humans are seen as transcending the physical realm in which conceptions of cause, force, and mechanical reactivity are readily applicable. When approaching the study of humans, therefore, naturalism compels the adoption of a subjective view and consequently requires supplementing more rigorous scientific methods with the distinctive tools of humanism—personal experience, intuition, and empathy. The descriptive aim of naturalism is a faithful rendition of human activity, even though only an approximation of that ideal is ever actually possible.

Whether or not they utilize terms employed by the people they describe, social scientists with substantive concerns require a good deal of input from their subjects before arriving at descriptive categories. Their concern is that the categories contain a high density of information, rich in meaning for the people being studied. To be suitable vessels for describing how people manage their lives, such concepts must necessarily take into account the principles by which those persons organize the information they acquire about the world in which they live. It is important for the naturalist to know what contrasts in the overall stream of events are meaningful to those being described, so that an excessive amount of information is not lost at conceptual boundaries. To do this requires intensive interaction with the subjects of study, the use of open-ended questions, and opportunities to observe people in natural settings.

## THE "ETHNO" OF ETHNOPSYCHOLOGY

Despite such commitments, one can legitimately question just how "ethno" ethnopsychology can be. If the criterion for a study to be strictly "ethno" in character is that only those data that are spontaneously produced by our subjects in natural contexts can be considered, the subject matter would be rather barren, it appears, for one of the most striking differences between "the West" and "the rest" is that "they" are generally much less likely to publicly elaborate those areas than are "we."

To clarify this issue it may be necessary to distinguish propositional levels, the assumption being that certain levels are more readily susceptible to ethnoanalysis than others. In reading the chapters in this book, I found it useful to distinguish between three levels. At the level of least complexity are those propositions that underly conceptual distinctions, that is, that group phenomena as the same or distinguish them as different. Many, if not most of these propositions are encoded in the lexical and semantic structures of the language and can be explored through inquiry into these areas. This is the ethno of ethnosemantics, or more pretentiously, ethno-science. While virtually all such inquiry involves intrusion into normal routines by the investigator, it aims at coaxing informants either to formulate acceptable propositions themselves or to verify our formulations of them. The methodological procedures of ethnosemantics have tended to be formal and prescribed, which is its greatest strength. But this level of theory has proved less than satisfying as a means of gaining insight into other cultures' world views. What is gained in methodological rigor is lost in comparative relevance. Not all domains, or all concepts within any given domain, are equally important to cultural constructions of reality. The ethnographic trick is to pick out those domains and key concepts that are central to a people's theoretical understandings and to elucidate them. This the authors in this volume have attempted, much to their credit. Efforts are made to isolate key constructs, then to relate them to a variety of phenomena for which they are deemed relevant to the people involved. Concepts are thus related to actions, events, thoughts, and feelings, as well as to other concepts. The propositions underlying these perceived relationships are often explicit in statements of association, correlation, causation, and so on, ranging from simple statements such as "x affects y" in some indeterminate way to highly formalized, specific propositions relating multiple variables in precise ways.

At this second, more complex level ethnological research presents a formidable methodological challenge. While people, during the normal course of social life, enunciate commonsense propositions all the time, they generally seem to be so context specific, and so dissociated from one

another, that the logic underlying the relationships between them remains a mystery. It appears, in short, that explicit statements of association, correlation, and causality are but a pale reflection of an implicit set of organized presuppositions that order social behavior. The question then, is just what do we mean by ethnotheory? (I presume ethnopsychology to be a particular kind of ethnotheory; precisely what kind is another thorny issue to be taken up shortly.) If we were to adhere strictly to our subjects' formulations, we would likely be stuck with a rather unsatisfying hodge-podge of propositions, many of them contradictory at least when removed from their contexts. So the question is, what do we do about it? To the extent that we demand a coherent, logically consistent theory from our informants, we restrict the possibilities for a genuine ethnoanalysis. We all know how difficult it is to get our graduate students (or our colleagues), who have been exposed for years to the principles of science, to clearly formulate coherent theories. Indeed, we regard it as an outstanding achievement and reward it accordingly. So, unless we are fortunate enough to come across a most extraordinary native synthesizer, the task of making coherent logical sense of what we have recorded falls on our shoulders. However, to the extent that we do intervene with propositions of our own and force the strands of our observations into a coherent package, we subvert the intent of ethnopsychological analysis.

The endeavor is therefore of a clearly different nature from that of producing an acceptable theory within the Western psychological scientific tradition. Instead of logical consistency and systemic coherence, order must be sought in praxis, in the ways our subjects *do* psychology. A minimum responsibility for an ethnopsychologist is thus to provide an adequate account of the conditions under which propositions are enunciated, the degree to which they are contextualized, and perhaps most important, the specific grounds for the particular interpretation offered, or even better, for competing interpretations.

Ideally ethnoanalysis would be based entirely on data that occurred in natural contexts, but we all know it is unrealistic to expect a richness of data without intrusion. Just how much badgering of our informants is acceptable is an open question. In part the problem is one of distinguishing the effects of the interview context on assertions. (It is clear that informants will sometimes make assertions to anthropologists they would virtually never make to compatriots, while there are others they are loathe to make to outsiders.) In part the problem is one of sampling, since we may be getting idiosyncratic rather than culturally shared views. One of the dangers is that we may set the frames for conceptualizations through elicitation, and may therefore lose important information about the meaning of concepts, since context so often implicates meaning. The problem is made even more

acute when it comes to translating native concepts into English, for it is through the contextualized usage of terms that we gain our best sense of meaning.

When concepts are used metaphorically or metonymically, or otherwise condense a rich symbolic content, we are especially vulnerable to misconstrual if deprived of usage within natural contexts. Certain key concepts (e.g., 'blood') may implicate a broad array of propositions and be powerfully charged with emotion. As is generally the case with such symbols, associated propositions are likely to be implicit (unconscious) and poorly articulated. We therefore run the risk of eliciting only superficial, rationalized assertions about human experience and miss the underlying theories.<sup>2</sup>

Yet another level of complexity has been labeled metatheory, which refers to propositions about the formation of propositions and about their acceptability, truthfulness, and the like. It is necessary to consider this level because ethnotheories are never static. They invariably have a generative aspect to them, as the ranges of possibilities are explored and applied to new circumstances (new, at least, for the individuals experiencing them). They therefore implicate the degree of intracultural variability that occurs in each community. That is, to the extent that assertions are subjected to a rigorous and coherent set of metapropositions before being accepted, variability is likely to be reduced, while lack of a well-specified metalogic breeds diversity. The latter condition complicates the problems of an analyst since he or she may have ethnotheories to contend with, or at least significant variations on the major themes. For this reason it is important to investigate the manner in which a people seek to validate assertions, to understand the grounds on which acceptability is based. Failure to do so removes an analysis one step further from being "ethno," since the investigator must fill in his own assumptions to the extent that he ignores those of his subjects.<sup>3</sup>

An excellent example of the importance of investigating metatheoretical dynamics is provided by Borofsky's (1982) recent work in Pukapuka. Borofsky points out that status rivalry underlies the processes involved in making assertions, asking questions, and providing responses. Whereas deference to those in authority apparently leads to convergent public knowledge in hierarchical Polynesian societies, in egalitarian Pukapuka individuals are concerned that they do not appear deferential to others, and are motivated to question, qualify, or disagree with other's views, at least within the bounds of social propriety. This, combined with a lack of concern for explicit verbal agreement, leads to considerable variability with regard to "knowledge," and gives Pukapukan ethnotheory in all domains a dynamic character.

## THE "PSYCHOLOGY" OF ETHNOPSYCHOLOGY

Let us turn now to the second part of the problem of circumscribing the domain of ethnopsychology, which involves defining the scope of psychology. It is apparent from reading these chapters that psychology is employed to encompass a broad range of possible concerns. It incorporates anything that affects the way persons think, feel, or behave; includes material and symbolic environments; and refers both to processes internal to the organism and those with stimulant value that are external—so how are we to distinguish ethnopsychology from ethnophysiology from ethnosociology, and the like? This question arose during a discussion that followed presentation of the papers at an ASAO meeting. Some participants were inclined to leave the issue open, to allow the parameters of the domain to be defined programmatically, by what scholars interested in the topic researched and reported. Others wanted to allow the contours of ethnopsychology to vary from culture to culture, in line with the formulation of native domains of personhood and the like. But Ward Goodenough, who was in attendance at the session, wisely pointed out that without some consensus about a focal area there would be no basis for comparison. Of course, since the domain of psychology in Western culture is so expansive, any attempt to delimit it for comparative purposes will necessarily be somewhat arbitrary. Nevertheless, there is something to be gained by narrowing the focus somewhat and building out from there.

Since, in my view, the nature of personhood is so central to a useful conception of ethnopsychology, and since it is so problematic, it is a good place to initiate a discussion of the problems of comparative analysis. To begin with, I think it important to recognize that a concept of person is necessary if we are to avoid the risk of merely applying psychological labels to culture rather than actually doing psychological analysis. However, the preceding chapters highlight the fact that defining personhood is no easy task for any given cultural group. The distinctions which some investigators found useful, for example, between "self" and "person," or between personal and social identity, were perceived as being inappropriate by others, given their particular concerns or those of the people they had studied. If every cultural group utilized a singular term in reference to a social (as distinct from physical) entity universally recognized as "person" there would be no problem, but such is not the case. As it is, "person" is an abstract conception everywhere that must be derived through analysis of multiple terms. It invariably has meaning at several contrast levels: human/nonhuman; infant/adult; live human versus dead human, and so forth. Exploring these usages requires deriving semantic content through contextualization, which may require a good deal of investigator intrusion.

Since the presuppositions on which such concepts are normally based are deeply implicit, the investigator may be required to formulate his or her own propositions to make sense of the multiplicity of usage. The problem is made even more acute when terms are used analogically as well as digitally. Thus, while "person" may be readily contrasted with "spirit" in many contexts, there may be intermediate concepts such as "chief" or "dead person," so that what appears to be a polar opposition in one context may be perceived as part of a continuum in another. In other words, personhood may be (and is in most of the societies dealt with in this volume) a matter of more-or-less rather than either-or.

Another problem in dealing with the concept of person has to do with the distinction between persons as individuals and persons as parts of relationships. It is evident, and has been for some time, that American culture is at the extreme end of a scale. Our folk psychology conceptually isolates individuals as actors to a degree that seems in stark contrast to the Pacific peoples studied by the participants in this symposium. The point is made again and again, as it was made by Leenhardt (1979) many years ago, that in these cultures personhood is inextricably woven into the fabric of social life, that the unit is better conceived as persons-in-relationships than as persons as discrete entities. The dissatisfaction with Western "scientific" psychology mentioned earlier is in large part a reflection of precisely this type of bias (see also Geertz 1976; Straus 1977; Rosaldo 1980; and Lutz, chap. 2).

As compelling as this contrast is at first glance, however, I would like to inject a bit of caution into our tendency to rush headlong into making this a cornerstone of comparative ethnopsychology. Thus, I wondered, as I was reading the papers, what kinds of evidence one could come up with to support the proposition that we, too, extend personhood beyond the skin, though perhaps in somewhat different ways. Just a moment's reflection brings to mind a variety of behavioral indicators of personal extension. All those phenomena associated with the concepts of empathy and identification could be included, as well as the more obvious example of personal space extension documented so well by Hall (1966). One could, I am sure, find a good deal of verbal evidence in ordinary discourse to support such an assertion. The other side of the coin could also be made problematic. That is, despite compelling evidence that most Pacific Islanders do not normally distinguish themselves as individualized entities in ordinary discourse, does this mean they do not have a clear conception of themselves as unique individuals? If so, how do they deal with the corporal reality of the body—the fact that it urinates and defecates and experiences hunger, thirst, and sexual urges? It seems to me that we have here an issue as to whether the submersion of individuals within broader, more inclusive categories of

relationship represents a prior notion of individualized selves, extended outward through socialization, or whether it represents a cultural conception that does not allow for self-differentiation. The point I wish to make is that it may be more fruitful for comparative analysis to accept the proposition that all people extend personhood beyond the skin than to begin with a "they do it and we do not" framework. What would then be problematic—the focus for comparative analysis—would be the ways in which extensions occur and from what conceptual base. Along these lines, I am sympathetic with the suggestion by Poole, made during one of the discussion sessions, that we explore how the inside-the-skin/outside-the-skin distinction is handled in different cultures, and how it relates to various notions of personhood.

Viewed from this perspective, what seems to distinguish Western folk psychology is the degree to which our notions of an inner self are elaborated and made central. For us the "real" self is conceived as that inner core of thought and emotion that is only partially displayed in behavior. It is not that our complete sense of personhood excludes interpersonal relationships, just that they are further removed from this central core. For the islanders described in this volume the reverse seems to be true. They have elaborated the public, relational aspects of their selves and seem to be much less preoccupied with the inner components. In some of these cultures people apparently allocate the inner domain to the realm of private experience and make no effort to account for it, whereas we provide multiple public models for inner experience (through popularized psychology as well as dramatic media) that encourage elaboration and accountability. In other cases, people appear to interrelate their private and public experiences into a shared framework for interpretation and action. That is, they integrate significant situational and/or relational contingencies with subjective experience when conveying their understanding of relevant occurrences.

Another cultural variable that may come into play, and significantly affect the way in which personhood is conceptualized among different cultural groups, has to do with the relative importance of boundaries. It appears that some groups are virtually obsessed with keeping phenomena conceptually distinguished from one another, while others are extremely tolerant of ambiguity and overlap. One need only look at textbooks within the Western academic tradition to gain an appreciation for the degree to which we have expended energy and effort to clearly distinguish one kind of phenomena from another. This preoccupation with isolating units of analysis seems to have reinforced ideological individualism in Western society, resulting in a psychology, of both academic and folk varieties, that isolates individuals as cornerstones for interpretive analysis. Pacific Islanders, as the chapters in this volume clearly show, more readily accept

the interrelatedness of phenomena and incorporate it into their social and psychological perspectives. The point is that natural boundaries (such as skin) can as readily be seen as mediators between domains as separators.

A related issue has to do with degrees of complexity and coherence. Are there notions of persons, or selves, as composed of discrete parts, and if so, how do these parts relate to one another? The division of personhood into corporeal and spiritual components is extremely widespread, if not universal, but elaborations vary. Body parts may or may not be included in conceptions of self, or certain parts (e.g., head, heart) may be considered central while other parts (e.g. feet, hair) may be thought of as marginal. Contextual variation may also be involved, so that on certain occasions particular body parts take center stage in self-conceptions (as when they are injured in our society) while on other occasions they are peripheral. Likewise, we are all familiar with the possibilities for elaborating models of the psyche from the professional psychological literature. The Freudian model comes readily to mind as an example of a differentiated mind whose components (ego, id, and superego) are quasi-independent of one another, and even have conflicting interests. From a comparative standpoint, therefore, we might ask about which areas are elaborated in different cultures, and follow with questions about the reasons they occur in specific ways under particular conditions.

The contributing authors provide a groundwork for comparative analysis by focusing on another type of universal phenomena—the transition into and out of personhood. The primary means of becoming a person is, of course, to be born and socialized. At just what point from conception to adulthood personhood is achieved is variable from culture to culture. In the Roman Catholic view personhood begins at conception, hence abortion is equivalent to murder; in other cultures a child might not be considered a person, and given a name, until well after birth. Infanticide in these societies is equivalent to postpartum abortion. Personhood may be achieved in stages, as Poole so nicely demonstrates for the Bimin-Kuskusmin, and it may be sharply demarcated by ritual acts such as initiations. The point is, however, that by examining the process of becoming a person we have a ready-made framework for comparison. A second way in which a nonperson can become a person is through adoption into a group, as when a stranger, particularly an ethnically distinct stranger, is transformed through socialization. An examination of the conditions under which this takes place should shed further light on comparative aspects of personhood.

Yet another way in which personhood is rendered problematic is through behavioral deviance, including interpersonal conflict as well as individual aberrations. Several of the chapters focus on such disturbances of "normal" social life as a means of illuminating basic cultural premises.

The great advantage of focusing on deviance is that it is precisely in such circumstances, in which the rules of cultural order are violated or threatened, that fundamental propositions concerning personhood are frequently made explicit. Inasmuch as ethnopsychology leans heavily toward a naturalistic methodology, and places a heavy emphasis on verbal utterances as primary data, the stimulus value of deviance is considerable. However, to the extent that we rely on such data, we must temper enthusiasm with caution, for the propositions about human experience posed at such times may be specialized and skewed; they may constitute a subset and not accurately reflect underlying conceptions of normal, everyday behavior. The classical psychoanalytic model, which was based on concepts designed to explain pathology and portrayed virtually everyone as deviant from an unobtainable ideal, provides an example of the distortion that can occur when deviance or illness is the center of concern. Despite this caution, it seems clear that we have here an area that will provide ethnopsychology with some of its richest data and most illuminating insights into comparative folk psychology, as the foregoing chapters demonstrate.

The question of what types of phenomena are to be included in the investigation of personhood is itself a thorny issue. On the one hand, it would be possible to relate virtually all of social life (and much else in addition) to concepts of personhood; on the other hand, not everything is as interesting or as important as everything else. In doing analytical work I consider it important to keep indigenous notions of self and/or personhood in focus, lest we drift into a form of description that is indistinguishable from normative social structural analysis, as Kirkpatrick has cautioned. It is this concern that makes Lutz's suggestion, that ethnopsychology focus on indigenous conceptions of personal variation, so appealing. One way to make strategic choices concerning the parameters of study is to let the people being studied determine what is important, either directly, by prescription if they are so inclined, or indirectly, by virtue of how much time they devote to various topics. But while this is always important information, I believe a truly comparative ethnopsychology requires more, and in the end it will be up to us to make informed choices. We cannot expect, of course, an equal density of information from each group that we study. Groups vary with regard to the degree of elaboration they provide in any area, but that in itself may prove grounds for comparison.

## **CONCLUSION**

To summarize, it appears that from both the universalistic and particularistic perspectives ethnopsychology faces a formidable array of theoretical and

methodological problems. Obtaining valid accounts of indigenous theories without significantly altering them by virtue of our intrusion may be an unobtainable goal. Perhaps the best we can do is to arrive at compelling inferences about the ways our intrusions affect the texts we interpret. Sensitivity to the complementarity involved in data collection is doubtlessly more important for ethnopsychological analysts than for nomothetically inclined theorists. We must also be especially alert to the presuppositions we employ in translating texts into *ethnotheory*. The problems of comparison—the only road to a universalistic cultural psychology—are likewise monumental. The Boasian credo, that extensive data collection must precede theory, has proved to be a barren prescription for cumulative understanding. Delimiting domains for comparison will, at the very least, be necessary for generating theories about ethnotheories; evaluating their validity and utility will require *us* to commit ourselves to metatheories (see Lutz, chap. 2, for further discussion of these issues).

Eventually, theoretical sophistication can be expected to emerge through an iterative process between increasingly competent contextualized descriptions of particular cultures on the one hand, and increasingly refined nomothetic formulations on the other. Radical relativism is as unacceptable a framework for the anthropological endeavor as is reliance on parochial “scientific” theories. The ultimate quest must be for an appreciation of the human condition in all its complexity, and this requires comparison. But the human condition cannot be properly understood as long as we resort to a language, couched in universalistic scientific garb, that is ethnocentric, value laden, and often pejorative. An examination of the literature that applies psychological analysis to non-Western peoples, and minority groups within Western societies (see Howard 1978; Howard and Scott 1981), reveals the extent to which such accounts are demeaning and dehumanizing. The worst abuses involve those instances in which cultural context is ignored, for it is precisely context, including the intentions and goals of the actors, that we use to attribute meaning to behavior, and by so doing attribute humanity—personhood—to people. When we ignore context and explain behavior on the basis of psychological abstractions derived from alien cultures, we deprive people of their humanity and reduce them to objects. In so doing we provide a rationale for disregarding their sensibilities and using political power to restructure their lives in ways that we see fit. If anthropology has indeed been handmaiden to colonial oppression in the past, psychology has provided one of its most pernicious tools.

It is to the credit of the authors of the chapters in this book that they aspire to reform scientific psychology so that it provides for cultural context. Perhaps it was an awareness of the difficulties involved in developing a universal cultural psychology that led to Kroeber's pessimism concerning

the future of culture and personality. The challenge is indeed formidable, but it also seems to lead us into the very heart and soul of human experience. These essays are but first shaky steps in what may be an unending quest, but the issue must be joined, and the sooner the better.

## NOTES

1. The value of exploring the native viewpoint is also well demonstrated by Shore's account of Samoan culture in *Sala'ilua* (Shore 1982). His analysis of personhood falls squarely into the domain of ethnopsychology.

2. In his thesis on Pukapukan knowledge, Borofsky provides a specific instance of the consequences of investigator intrusion. Whereas Pukapukans were content to leave a discussion full of ambiguities and unresolved discrepancies, as an anthropologist concerned with providing an intelligible account of Pukapukan culture to Western audiences, he tended to push discussions toward consensus and closure. In the spirit of ethnoanalysis Borofsky analyzes the effects these alternate meta-theoretical approaches have on forms of knowledge (Borofsky 1982).

3. It is in this area, by the way, that anthropologists have much to gain by familiarizing themselves with the achievements of ethnomethodology within the field of sociology.

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