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What Psychology Can Do for Anthropology, or Why Anthropology Took Postmodernism on the Chin

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What Psychology Can Do for Anthropology, or Why Anthropology Took Postmodernism on the Chin

The dictates of postmodernism require that I specify my own perspective. Obviously, there are an infinite number of statuses that influence anyone's personal perspective on anything. This is part of the postmodern dilemma. However, there is a restricted set of historical conditions that are relevant to a particular task. What is relevant about my history to the task that I have set myself in this essay is my relation to the disciplines of psychology and anthropology. Therefore, in talking about what psychology has to offer anthropology, I want to make it clear that I am not a psychologist talking about anthropology as someone else's discipline. Although I am in a department of psychology, I received both my degrees from the Department of Social Relations at Harvard, an interdisciplinary mix of social psychology, social anthropology, and sociology. For me, both psychology and anthropology have always been part of my tool kit. In fact, I am revising this essay from the School of American Research in Santa Fe, an institute for advanced study in anthropology. In discussing what psychology has to offer anthropology, I am therefore talking to myself as well as to my colleagues in anthropology.

Until quite recently, I, like Fish (2000), had given considerable thought to what anthropology had to offer psychology (Greenfield 1996). Like many cultural and cross-cultural psychologists (Jessor, Colby and Shweder 1996; Triandis and Berry 1980), I was particularly impressed with the ethnographic method. How to reconcile this admiration from the field of psychology with the breast-beating and self-flagellation going on in cultural anthropology? In thinking about this problem, it suddenly occurred to me that the methodology of psychology had successfully addressed some of the principal problems identified by the postmodern critique of anthropology. I now believe that this may be why psychology has weathered postmodernism better than anthropology. By "weathering postmodernism better" I refer to an optimistic sense that the tradition of empirical research will continue to yield rich rewards in our knowledge of human nature.

Of course, my premise may be instantly rejected by cultural anthropologists, for empiricism itself is of course under attack in the postmodern critique (Geertz 1973). Along

with empiricism, scientific generalization is also an object of derision. In the course of this essay, I hope to convince my readers that the babies of Empiricism and Generalization have been thrown out with the bathwaters of Objectivity, Cultural Homogeneity, Fact, Truth, Otherness, and Science as an Apolitical Enterprise. In short, *to accept these latter six assumptions as valid targets of the postmodern critique does not necessarily entail a turning away from empirical methodology; it does not necessarily entail the redefinition of anthropology as literature rather than science.*

An analysis of psychology's approach to some of the extremely important problems identified by postmodernism may provide ideas for how cultural anthropology can return to itself as an empirical enterprise, stronger and wiser than before the buffets of the postmodern critique. However, before beginning my argument, I must address two major issues that complicate it in interesting ways. The first issue has to do with the fact that some of the potential inputs and insights from the field of psychology have already been integrated into the anthropological subfields of psychological anthropology, linguistic anthropology, biological anthropology, and applied anthropology. Many empirical methods from psychology are well entrenched in psychological anthropology (Bock 1999; Hollan and Wellenkamp 1994, 1996). Linguistic anthropology offers explicit methodology and a new array of techniques that preserve concrete data and subjects' voicing in the face of the postmodern critique (Duranti 1997). In biological anthropology, anthropology makes common cause with psychology, reminding anthropology of the biological substrate of human behavior and challenging the dualism of biology and culture (issues that will not be pursued in this essay). In applied anthropology, a series of books (Schensul and LeCompte 1999) treats ethnography as an empirical methodology that can be described, learned, and taught. Many of these inputs from within anthropology also constitute constructive empirical responses to the postmodern critique.

However, within anthropology these are minority voices, and it is not clear whether these voices have always been heard by the postmodern majority of cultural anthropologists; in addition, even within the subfields of psychological,

linguistic, biological, and applied anthropology, there are many for whom empirical research has been derailed by the postmodern critique (D'Andrade 1999). If the minority voices had been more heeded by cultural anthropology and anthropology as a whole, the postmodern critique might well have done less damage to the empirical, scientific investigation of culture and cultures. In this essay, I hope to give new ammunition to these minority voices within the field of anthropology.

The second issue stems from the fact that psychology has had its own postmodern critiques (Gergen 1990, 1995). Although they have been a minor mode within the field of scientific, empirical psychology (and in this sense psychology has weathered postmodernism better than anthropology), they merit serious consideration. Also, where Gergen's (1985, 1991a, 1991b) postmodernism has been most influential in psychology—in family therapy (Nichols and Schwartz 1995)—his postmodern influence has had a constructive effect on practice, without having any negative impact on the scientific enterprise.

I also recognize what thin ice I am on as a psychologist. In "Thick Description: Toward an Interpretive Theory of Culture," the first building block of postmodernism, Geertz (1973) is not only antiscientific, he is also antipsychological, and unabashedly so. However, Geertz's dichotomy between the "experimental science in search of law" and "an interpretive one in search of meaning" (1973:5) is a false one. Cultural psychology, not to mention linguistic anthropology and psychological anthropology, have shown themselves to be extremely capable of using systematic empirical means to investigate the making and interpretation of meaning as a central theme in human nature (Greenfield 1996).

The Objective Perspective

In anthropological ethnography, culture was traditionally treated as an objective whole. Although the ethnographer was a participant-observer, the final ethnography was written as though the observer were omniscient, devoid of any particularities of perspective. A major way in which this came out was in the form of general statements, without any information as to the source of the statements or evidence for them. An example (taken at random) is the following sentence from Guiteras-Holmes's classic ethnography, *Perils of the Soul* (1961:10): "What is today the State of Chiapas belonged to the captaincy general of Guatemala, one of the two administrative subdivisions of the viceroyalty of New Spain." Guiteras-Holmes makes no mention of the source of this information. This is particularly striking because, immediately before, she has informed us that "[h]istorical data are scarce" (p. 10) in the region she is discussing. Yet, because of anthropological convention, she does not feel it necessary to tell us where she found her data.

Just as no source is given for her historical statements, no evidence is given for her contemporaneous conclusions. Thus, when talking about intercultural relations, she states, "Trading and the hiring of labor for the fields are responsible for most relations" (Guiteras-Holmes 1961:17–18), but we are given no indication of the evidence for this statement. This convention of the general statement without historical source or ethnographic evidence is followed throughout the book and is generally true of classic ethnography.

The methodological implications of the general statement without evidence is that methods do not matter because there is an objective truth, homogeneous throughout the culture. The underlying (but never spoken) assumption is that it does not matter how you get your information; the conclusion will always be the same because it is, objectively, true.

Critique of the Objectivity Assumption in Anthropology

This assumption of an objective or outside look at a homogeneous cultural system receives harsh criticism in postmodern anthropology. Clifford (1986:22), in the introduction to a classic work of postmodern anthropology, *Writing Culture*, writes, "There is no longer any place of overview (mountaintop) from which to map human ways of life, no Archimedean point from which to represent the world." Postmodern criticism has drawn attention to gender and political positions as influences on the way data are collected and conclusions drawn, as well as myriad other potential influences. The notion is that all ethnographers have a particular position from which they work; therefore the notion of objectivity as beyond the "bias" of a particular vantage point is simply invalid.

From the perspective of linguistic anthropology, Duranti writes (1997:85–86):

With respect to ethnography, the problems with the term "objectivity" arise from its identification with a form of positivistic writing that was meant to exclude the observer's subjective stance, including emotions, as well as political, moral, and theoretical attitudes. Such an exclusion, in its more extreme or "purist" form, is not only impossible to achieve, it is also a questionable goal, given that it would produce a very poor record of the ethnographer's experience (De Martino 1961). How would one be able to say what people are doing without at least a minimal identification with their point of view? One would end up saying things like "people squat on the floor, grab their food with their hands and bring it to their mouth—and this, they call 'eating.'" As it is obvious from this example, rather than being "objective" and impartial, accounts of this kind can easily be read as implying a negative evaluation of local practices. Equally implausible is a description that completely identifies with the native perspective and does not, in some fashion, reflect the researchers' perception of the described events. . . . A science of people, a human science, cannot but also exploit the researchers' ability to identify,

empathize with the people they are studying. This implies that there exists in ethnography a certain playful element which consists of changing the familiar into the strange and, vice versa, the strange into the familiar (Spiro 1990).

Critique of the Objectivity Assumption in Psychology

Kenneth Gergen, the leading postmodernist in the field of psychology, writes that "if our conventions of writing are, in turn, dependent on social agreements, and these agreements carry with them various ideological biases, then all scientific writing—all our attempts at objectivity—are essentially value saturated products of social agreement" (1990:28). This line of argument leads Gergen to the conclusion that the subject matter of psychology has vanished:

postmodernism raises fundamental questions with the assumption that our language about the world operates as a mirror of that world. Rather discourse about the world operates largely on the basis of social conventions, which in turn are crystallized in terms of various rhetorical rules and options (such as rules of proper storytelling). Thus, to presume the independent existence of a subject matter, reflected by the discourse, would be to engage in an unwarranted objectification of the discourse. [1990:29]

Gergen's critique of objectivity leads to the "marginalization of method" in psychology:

under modernism, methodology underwent a virtual apotheosis. Methodology was the means to truth and light, and thus to salvation. . . . Under postmodernism, however, methodology loses its coveted position. Under postmodernism methods are viewed as a misleading justification device. They misleadingly operate as truth warrants for particular propositions, when propositions are not fundamentally capable of "carrying truth." [1990:30]

Anthropology's Response to the Objectivity Critique

Yes, methodology has been dethroned in anthropology (D'Andrade 1999). Geertz (1973) (based on the psychological notions of an earlier era) dealt a body blow to operational definition and systematic methodology. Thence arise conceptually important ethnographies (such as Tsing 1993) that innovate important ideas (e.g., intercultural interaction), yet have loosened the ties between data and analysis, to the point where data and analysis travel two quite independent paths (Marcus 1998). This disconnection is the natural result of the tenet that methodological techniques and procedures are irrelevant (Geertz 1973).

Still another response is to move from the assumptions of objectivity in a traditional ethnography (e.g., Dumont 1972) to an explicit description of the ethnographer's own perspective and relationship with the subjects of study in the same community (e.g., Dumont [1978]1992). (This

strategy responds not only to the objectivity critique but also to the "otherness" critique, a later topic of discussion.)

Why Psychology Has Been Less Vulnerable to the Objectivity Critique: Operationalization and Description of Methods

The predictions of Gergen notwithstanding, methodology has not been dethroned in psychology. Indeed, it is ironical that the very methodology so harshly criticized by Geertz and Gergen has made psychology less vulnerable to the objectivity critique. This is perhaps one reason why the hegemony of methodology has survived in both undergraduate and graduate education, virtually unscathed by Gergen's critique. The argument for methodology goes back to the history of psychology.

On a philosophical level, scientific psychology was founded on the principle that a psychological construct does not exist outside the specific way in which it is measured. For example, a classic definition of intelligence within the field of psychology has been "Intelligence is what the intelligence tests measure." This is an intrinsically relativistic notion; the idea is that, if you change your test, you also change your concept of intelligence.

In psychology, researchers are always obliged to describe how they obtained their data and how they went from their data to their conclusions. The obligatory methods section in a psychology article includes a description of the subjects who furnished the data, the operations that were used to elicit the data, the system that was used to interpret or code the data, and the statistics that were used to analyze the data. Although psychology, even more than anthropology, has reified the objective observer, it nonetheless requires explicitness about procedures. Implicitly, the description of procedures does locate the researcher's perspective to some extent. For example, we know if the observer was behind a video camera, was taking notes on a naturally occurring situation, or was carrying on an interview out of the context of daily life.

In essence, the assumption within psychology—that results and conclusions are intrinsically relative to the methods used—has spared psychology from the degree of damage suffered by anthropology at the hands of the objectivity critique. This is not to say that psychology is methodologically invulnerable. Its reification of objectivity is a crack in the armor against postmodern criticism (and leads to the unconscious ethnocentrism so well described by Fish [2000] in his companion article to this one). However, because of its methodological relativity and self-conscious treatment of methods, psychology has, as a discipline, been free to develop new methods to deal with varying subjectivities. A good illustration from cross-cultural/cultural psychology is the collaboration of researchers from each of the cultures being compared in a cross-cultural study (e.g., Stevenson et al. 1985). This technique enables the research

potentially to have both an outsider and an insider perspective on each of the cultures in the comparison.

Potential Application to Anthropology

In the field of education (or perhaps we should call this applied anthropology), a more radical utilization of multiple perspectives has been developed by Tobin, Wu, and Davidson (1989): the multivocal ethnography. The multivocal ethnography is an ethnography composed with many voices, rather than the single voice of the anthropologist. Tobin, Wu, and Davidson's method of multivocal ethnography utilizes videotaped data. Because such data are permanent (unlike the more traditional observations), they can be viewed and interpreted by multiple parties (the multiple voices). Tobin, Wu, and Davidson made tapes of activity in three preschools, one in China, one in Japan, and one in the United States. Teachers and parents from all three cultures saw and commented on tapes from all three countries. Thus, the data from each country were interpreted from both insider and outsider perspectives. Note that, in this method, the typical ethnography in which a Western observer structures the Eastern culture as an object of study is retained, but also turned on its head by the addition of reciprocity. The Eastern observer now has an opportunity not only to interpret his or her own culture but to interpret the Western one as well (plus a second Eastern culture). Although Tobin, Wu, and Davidson developed their method and methodology in the context of a cross-cultural study of educational practice and values, it is an example of a new kind of ethnography that is potentially applicable to any of the traditional arenas of anthropological ethnography.

The preceding example is termed ethnography, even though it utilizes the technology of video. But could anthropology make any use of the more traditional methodological assumptions and conventions from psychology? I believe so. The ethnographer could tell us what he or she did to gain the knowledge that led to a particular conclusion. For example, when Guiteras-Holmes says (1961:24), "He who is away from home expresses his longing to return," a psychological researcher might suggest that the anthropologist tell us whom she talked to or observed and under what circumstances. For example, did the anthropologist draw her conclusion from interviews, or did she learn it as a participant when traveling away from home with her informants?

Within anthropology, Warren (1996) has noted that anthropology needs to add a relationship between field notes and published work. Psychology already has established a parallel distinction between data (often encapsulated quantitatively), coding (where relevant), and discussion of results. The implication from psychology is that a valuable addition to ethnography would be samples of field notes, a description of the guiding principles in taking the notes, and, most important, a description of the method by which

chronological field notes were converted into ethnographic conclusions and writing. Seymour's (1999) fascinating ethnography of long-term fieldwork and social change in India is impeccable in this respect and, interestingly, represents a contribution from the subfield of psychological anthropology.

Another instructive example comes from Bambi Schieffelin, a linguistic anthropologist who has been methodologically influenced by training in psychology (taken under Lois Bloom in the Developmental Psychology Department at Teachers College, Columbia University). In Schieffelin's (1990) ethnography of the language socialization of Kaluli children, she includes a 12-page section on "Method and interpretation" (pp. 24–36). The titles of the subsections themselves provide evidence that she not only has covered all of the territory encompassed by psychological methodology but, in addition, has adapted the methodological categories from psychology of *subjects*, *procedure*, and *data analysis* to her study community and research topic. The titles of her subsections are as follows: "Selecting families and contexts," "Collecting the speech data and preparing the annotated transcripts," "Reading the transcripts and interpreting the examples," "Some thoughts on writing this ethnography." This last section would seem to owe its existence more to Clifford and Marcus's (1986) notion of writing culture than to psychology. However, it is interesting that once the notion of writing culture is integrated with a more self-conscious methodology, it does not lead to the self-flagellation of "How can we ever know anything? We are hopelessly trapped in our limited and biased perspectives." Instead, it leads to an integrated description of the methods that constitute an important aspect of the "perspective" of Schieffelin's study.

At the same time, such description leads to appropriate modesty about one's work. Gone is the theoretically omniscient ethnographer. In his or her place is the ethnographer who understands and can make explicit the reality of his or her relationship to the culture and the access this relationship afforded. Schieffelin writes (1990:23–24):

As a woman, I was given privileged access to the activities of women and children. No man could have sat in the women's section or gone bathing with small children. As a mother, I was seen as an adult, one who shared some perspectives with other women. Being an impartial observer was neither possible nor desirable. Kaluli incorporated me into their social world and social system, and according to my various relationships I was given kinship or relationship names used by friends.

This paragraph is important because it shows that the particularity of perspective is not necessarily a negative; such negative connotations are contained in the term *bias*. However, the particularity of perspective can be a strong positive, as female gender and motherhood were in Schieffelin's study of the language socialization of children.

Within cultural and psychological anthropology, a new genre has grown up, the individual life history. This genre, as in the beautifully crafted *Translated Woman* by Ruth Behar (1993), makes its methods and sources extremely clear. Behar's whole book is edited quotations from kitchen table conversations with Esperanza, the pseudonym of her subject. This is a way of studying culture through a particular case study that makes a strong connection between data and conclusions.

Culture as a Unitary Whole

Whereas the last section dealt with a critique of the omniscient anthropologist, this section deals with a critique of the omniscient informant. The traditional assumption within anthropology has been that culture is a homogeneous, unitary, and, possibly, superorganic whole. Each member of the culture shares the same cultural knowledge. An assumption, derived largely from Durkheim, "that underlies much of traditional fieldwork practice [is] that anthropologists are not concerned with individuals as such, but merely with their functioning qua carriers of a common culture" (Wassmann 1995:176). Insofar as everyone shares a common culture, informants are both interchangeable and omniscient vis-à-vis their own culture. As Sapir ([1932]1949:509) put it:

It is what all the individuals of a society have in common in their mutual relations which is supposed to constitute the true subject matter of cultural anthropology and sociology. If the testimony of an individual is set down as such, as often happens in our anthropological monographs, it is not because of an interest in the individual himself as a matured and single organism of ideas but in his assumed typicality for the community as a whole.

Critique of the Anthropological Assumption that Culture Is a Unitary Whole

Sapir himself realized the dangers of this approach:

It is true that there are many statements in our ethnological monographs which, for all that they are presented in general terms, really rest on the authority of a few individuals, or even of one individual, who have had to bear testimony for the group as a whole. Information on kinship systems or rituals or technological processes or details of social organization or linguistic forms is not ordinarily evaluated by the cultural anthropologist as a personal document. He always hopes that the individual informant is near enough to the understandings and intentions of his society to report them duly, thereby implicitly eliminating himself as a factor in the method of research. All realistic field workers in native custom and belief are more or less aware of the dangers of such an assumption and, naturally enough, efforts are generally made to "check up" statements received from single individuals. This is not always possible, however, and so our ethnological monographs present a kaleidoscopic picture of varying degrees of generality,

often within the covers of a single volume. [(1932)1949:509–510]

The notion of the omniscient informant continues to be questioned within anthropology. Indeed, an article by Wassmann (1995) is titled "The Final Requiem for the Omniscient Informant?" Lawrence (1995:216), in her reply to Wassmann, argues that all informants have some specialized knowledge, are experts in some field; however, none is omniscient, "all-knowing and all-revealing."

Generalizing this point, Ochs (1994), coming from the disciplinary perspective of anthropological linguistics and discourse analysis, notes that different members of a culture have different pieces of cultural knowledge; no one person has the whole. Wassmann (1995:176) writes that "it becomes necessary to study individuals, or categories of people in their own right rather than merely as some kind of cultural 'subunits.'" This point is particularly applicable to children, who are in the process of being inducted into the culture (Zukow 1989) and so, by definition, have incomplete cultural knowledge.

Indeed, the partiality of the view of the subjects of study parallels the partiality of the view of the ethnographer. Just as Schieffelin went places no male ethnographer could go, so Kaluli women went places no male Kaluli could go. Kaluli women are experts on parts of the culture about which Kaluli men are ignorant, and, of course, vice versa. Other sociological variables besides gender come into play, such as social and economic status. Each of these statuses privileges certain aspects of both behavior and knowledge. Then add to the differences emanating from variables of social stratification, individual differences emanating from temperament and personality variables. All of these factors are sources of within-culture differentiation. In traditional ethnography, all of these factors affect the ethnographer's informants and the knowledge and behavior they are able to display for the anthropologist. Yet, as Clifford points out (1986), it is the rare ethnographer who describes individual informants.

My own field experience in Zinacantán, Chiapas, Mexico, illustrates how the social position of an informant can not only facilitate methodology but actually influence research results. In 1969, I went to the Maya community of Zinacantán as part of the Harvard Chiapas Project. Two anthropologists, Evon Vogt, the director of the project, and George Collier, an alumnus of the project, selected an informant for me. His name was Xun Pavlu. They thought he would be good for me, and he was. My needs were different from those of an ethnographer. As a researcher in cultural, developmental psychology, I needed a lot of subjects for my experiments. Xun did not provide data for me; he provided subjects. He used his political influence and his extensive network of extended family and *compadrazgo* (co-godparents) to persuade parents to let their children participate and to participate themselves. Without his position

of influence, the more than one hundred subjects he recruited (out of a village of about fifteen hundred) would not have been possible. Furthermore, when I returned in 1991 to study the next generation, his social and economic characteristics not only facilitated data collection but also had an important influence on the results of my longitudinal community study.

I returned to Xun's hamlet of Nabenchauk in 1991 to study the effects of the economic transition from agriculture to commerce and entrepreneurship that had been going on since I had left in 1970 (Greenfield 1999; Greenfield and Childs 1996; Greenfield et al. 1997). I wanted to study the descendants of my old subjects in order to assess the effects of the historical change, uncontaminated by extraneous factors. Xun once again made his network available, as he had two decades earlier. What became clear was that of all the families in Nabenchauk, the Pavlu family was most involved in commerce and entrepreneurship. All of Xun's seven sons and all but one of his three sons-in-law were involved in commerce or entrepreneurship as either truck or van owners or drivers. The remaining son-in-law was quite involved in the consumer aspect of commerce. He had a technical job in a factory in Tuxtla Gutiérrez and therefore had a certain amount of disposable income for consumer products. Because Pavlu family members were commercial leaders in the community, our sample included those families who had been most affected by the historical trends of pertinence to the study. If the hypothesis had merit, this was the sample to demonstrate it.

As we saw in relation to the ethnographer, this example shows how the social position of the informant can have a positive effect on the research if the position is one that is facilitative for the particular problem under study. As in the case of the ethnographer, the nature of the informant's social position both limits and facilitates. Which outcome occurs in a particular case depends on the relationship between the informant's position and the problem under study. If I had wanted to study socialization in the most traditional families in Nabenchauk, for example, Xun Pavlu's position would have been a hindrance rather than a help.

As the Science of Individuals, Psychology Has Not Been Affected by the Critique of the Unitary Whole

Because the unit of analysis in psychology is the individual, psychology has not been susceptible to the critique of the unitary whole. Indeed, the study of individual differences is very much a part of the science and practice of psychology. The study of the influence of social factors such as class and economic status also has a tradition within the discipline (although the origins may come from sociology). In the methods section of a psychology article, the background characteristics of a sample, including the ranges of pertinent descriptor variables, are often presented; the role of gender, social class, and education in behavior is

often analyzed. Nonetheless, I agree with Fish (2000) that the analysis is often superficial; an important question is whether it is possible to combine ethnographic depth with unbiased sampling of within-culture differences, and this issue is pursued in the next section.

Potential Application to Anthropology

The main application of psychology in addressing the critique of the unitary whole is to make it known in one's writings exactly who the informants are in terms of their individual characteristics and social positions. A second application may be to engage in some sort of systematic sampling if the goal of a study is to account for the whole culture rather than the culture as experienced by a few individuals. The methodological notion of sampling (from psychology or sociology) challenges the idea of ethnography, with its classical use of a few informants. However, anthropologist and informant often develop very close relationships. This is not the case for psychologists and their numerous subjects. Consequently, there can be trade-offs of depth and breadth that need to be carefully considered and controlled.

Dasen (a cross-cultural psychologist) and Wassmann (a cultural anthropologist) have recently made some advances in this problem area by consciously integrating psychology and anthropology. They have developed a three-stage approach to their research in cognitive anthropology (Wassmann and Dasen 1994). Stage 1 is ethnographic; it then forms the foundation for observing everyday activity (Stage 2) and for developing culturally relevant experiments administered to many subjects (Stage 3). However, even in the ethnographic phase, Wassmann and Dasen use a sampling technique that bears the mark of psychology. They use not one but multiple informants, and they select their informants systematically in order to sample different social roles and statuses in the community (Wassmann 1995).¹

Fact vs. Interpretation

Ethnographers used to think that they were emerging from their studies with facts. Now they feel belittled by learning that they are emerging with interpretation. Denzin (1996), for example, writes of the representational crisis. This crisis stems from the fact that "researchers can no longer directly capture lived experience; such experience, it is argued, is created in the social text written by the researcher" (Denzin 1996:127). In Clifford's words, "every version of an 'other,' wherever found, is also the construction of a 'self' " (1986:23).

A Contribution from Psychology

These criticisms are discouraging. They imply a researcher who has no escape from his or her own framework. Even as one attempts to understand a new culture, one is

merely building an edifice that is a mirror of the self. This seems like a closed circle. However, because psychology has a long tradition of studying varying subjectivities, this is not necessarily a serious problem. Indeed, the construction of meaning is central to the emerging discipline of cultural psychology (Bruner 1990; Shweder 1990). We can open the closed circle by seeing how subjects interpret us, not merely how we interpret them. Just as we construct ourselves by studying our subjects, our subjects construct themselves through studying us.

There are some wonderful examples of reciprocal interpretation in cultural psychology. The classic one comes from Glick. Cole, Gay, Glick, and Sharp (1971) took an object-sorting task to Liberia, where they presented it to their Kpelle subjects. There were 20 objects that divided evenly into the linguistic categories of foods, implements, food containers, and clothing. Instead of doing the taxonomic (categorical) sorts expected by the researchers, subjects persistently made functional pairings (Glick 1968). For example, rather than sorting objects into groups of tools and foods, subjects would put a potato and a knife together because "you take the knife and cut the potato" (Cole et al. 1971:79). According to Glick (1968), subjects often justified their pairings by stating "that a wise man could only do such and such" (p. 13). In total exasperation, the researchers "finally said, 'How would a fool do it?' The result was a set of nice linguistically ordered categories—four of them with five items each" (p. 13). In short, the researchers' criterion for "intelligent" behavior was the subjects' criterion for "foolish"; the subjects' criterion for "wise" behavior was the researchers' criterion for "stupid." Here, both subject and researcher had a chance to interpret each other. Each interpretation gave as much information about the cultural value system of the interpreter as it did about the nature of the world.

This one example is cited and described in a myriad of articles. It is instantly recognizable as showing something profound about the Kpelle's definition of intelligence, as well as about the cultural relativity of our own definition. Yet the opportunity for reciprocal interpretation is rare in psychology, as it is in anthropology. Nonetheless, it is a method that could be generalized and utilized in both ethnography and psychology. By systematically studying multiple subjectivities in a cross-cultural study, the researcher can escape the hermeneutic circle.

Truth vs. Constructivism

The traditional, or modern, position is that science requires truth and that the ethnographer will discover the "true" culture through the time-honored methods of participant-observation. The general notion about cultural knowledge, like other types of knowledge, is that it "should reflect, depict, or somehow correspond to a world as it

might be without the knower [read: anthropologist]" (von Glasersfeld 1984:3).

Constructivism: The Postmodern Critique of Truth

"Truth" has been radically deconstructed. In anthropology, as in many fields, knowledge became mere "social conventions developed by people with their own biased perspectives and motives" (Nichols and Schwartz 1995:119). Thus, in Geertz's words, "What we call our data are really our own constructions of other people's constructions of what they and their compatriots are up to" (1973:9).

Anthropology's Response to the Critique

The dominant response has been for the researcher to explicate these biases and motives. A well-known example is *In the Realm of the Diamond Queen* by Tsing (1993). The danger here is that the number of pages devoted to the study population can be small, relative to the number of pages devoted to the researcher and his or her culture of origin. Duranti (e-mail to author, June 27, 1998) puts the dilemma in another way: "How do we tell stories about other people without pretending that we weren't there?"

Psychology's Response to the Critique

Constructivism has played a very significant role in psychology, especially developmental psychology and family therapy. For example, "With this postmodern assumption—that there are no realities, only points of view—comes an interest in how the narratives that organize people's lives are generated. Postmodern psychologies concern themselves with how people make meaning in their lives; how they construct reality" (Nichols and Schwartz 1995:119–120). An example of this approach in psychological anthropology lies in Hollan and Wellenkamp's (1994, 1996) explorations of meaning-making in the Toraja community of Indonesia. In other words, instead of emphasizing the meaning-making of the *researcher* (as cultural anthropology does) in response to postmodernism, psychological approaches have emphasized the meaning-making of the *subject* and have taken this activity as an object of study (e.g., Bruner 1990). Indeed, constructivism has, since Piaget (1954), been at the center of the study of cognitive development.

There is another radical difference between the response of psychology and the response of anthropology to constructivism. Whereas anthropology has seen constructivism as undermining anthropology as a science, psychology has recognized that all the sciences, bar none, are narrative constructions. For example, de Shazer asks, "But, don't physicists tell stories about subatomic particles and black holes so that they can let one another know about such

things? Are these stories science or narrative?" (1991:49). If all sciences are narrative constructions, then, from the point of view of psychology as a research field, the human construction of reality provides insufficient reason to declare oneself in the humanities rather than in the social sciences.

Psychology has had another response to constructivism: to move from construction as an individual activity to construction as an interindividual activity (Vygotsky 1978). Indeed, social construction is an important part of both developmental psychology and family therapy (Nichols and Schwartz 1995). The field of family therapy looks to Gergen (1985, 1991a, 1991b) for emphasizing "the power of social interaction in generating meaning for people" (Nichols and Schwartz 1995:120). Social construction is also the basis for the early development of social conventions between mother and child (Bruner 1983) and the later creation of shared norms among children (Piaget [1932] 1965). Again, the empirical study of these developmental constructions has been an important part of the field of developmental psychology.

Application to Anthropology

Geertz (1973:12) notes that "culture consists of socially established structures of meaning." How do these structures get established through interaction? Postmodern anthropology emphasizes the creation of meanings through a process of negotiation. The interactional processes by which meanings are negotiated are a major empirical focus of linguistic anthropology (Duranti 1997) and cultural psychology (Greenfield et al. 1998). These processes could also become an empirical focus for cultural anthropology.

I believe that there has been a barrier to this construal of constructivism in anthropology. This barrier is the conceptual emphasis on individual construction in the form of writing and reading.

The reader brings to the task of reading all of his previous experiences, all previous uses of the words and concepts, which contaminate what he reads. For this, the deconstructionists use the term "misreading." Seen in this way, one cannot read, one can only misread. All texts allow for a host of potential misreadings. [de Shazer 1991:50–51]

Yes, this is generally true for written texts. But spoken discourse, with its interactional component, is often a process in which the interactants constrain and build on each other's meanings. This process is therefore much less solipsistic than the communication between writer and reader. It is ironical that postmodern cultural anthropologists have focused on their own individual constructions rather than studying the social constructions of their subjects.

Anthropology as the Science of the Other

Anthropology was conceived as the science of the Other (Trouillot 1991). "From the early nineteenth hundreds to the second world war the primary agenda of social and cultural anthropology was to document the life of nonliterate peoples" (D'Andrade 1999:2). Clearly the agenda was for literate people from Western societies to get to know and understand nonliterate peoples from non-Western societies.

Critique of Anthropology as the Science of the Other

According to the postmodern critique, there are two problems with this agenda. The first is that it is impossible to know the Other because the Other has his or her unique perspective (Geertz 1983). Given that there is no such thing as an objective perspective and that it is impossible to know another, cultural anthropology's data and objects of study have disappeared. This leads to "epistemological relativism (there is no real foundation for knowledge)" (D'Andrade 1999:8).

According to the postmodern critique, the second problem with this agenda is that studying the Other exaggerates differences between the people being studied and the researcher. This exaggeration creates what Tsing calls "the fantasized gulf between the West and its Other" (1993:13). In Tsing's view, the depiction of such a gulf has an important political dimension; it expresses a relationship of colonizer to colonized. (I return to the political dimension of the postmodern critique at the end of this essay.)

Why Psychology Has Been Less Vulnerable to This Critique

Psychology arose as the science of the self. One of the original psychological developed in Germany, was introspectionism. While introspectionism was later banished for its lack of "objectivity," psychology remained basically the science of ourselves, not the science of others. Of course, in combination with the universalistic ambitions of psychology (as the science of human beings), this perspective is another factor in psychology's unconscious ethnocentrism (Fish 2000). This ethnocentrism is also an important element in Misra and Gergen's (1993) postmodern critique of psychology (see also Dasen's [1993] critique of ethnocentrism in psychology). *Whereas in anthropology the struggle has been how to understand the perspective of others without assuming essentialistic differences, the struggle in psychology has been how to understand the perspective of others without assuming essentialistic similarities.* These diametrically opposed problems should tell us that the truth lies somewhere in the middle.

Psychology's Response to This Struggle

Many minority and international scholars now fill the ranks of psychology. For the most part they are caring for clients from their own groups in the clinical fields. To some extent they are researching and publishing about the development and social relations of their own groups. The insider's perspective is validated in practice if not in theory. The same struggle remains: after being educated in the field of psychology as it exists, to what extent can these psychologists abandon the ethnocentrically universalistic frameworks of classical psychology and validate the frameworks of those for whom these frameworks do not fit?

It can be done. For example, Triandis (1989, 1993) valorized his Greek heritage in opposing the concept of collectivism to the individualistic assumptions of U.S. psychology. Markus and Kitayama (1991) had a cross-cultural collaboration (U.S.-Japanese) that expanded this concept more squarely into the realm of social psychology with their concept of the interdependent self. With Rodney Cocking, I edited a book called *Cross-Cultural Roots of Minority Child Development* that joined researchers from around the non-Western world (Asia, Africa, Mexico, and Native America) with minority researchers to identify continuities, discontinuities, and change in ancestral and ethnic patterns of socialization and development (Greenfield and Cocking 1994). While insider perspectives purposely dominated, outsider perspectives were also introduced into the discussion. (At the same time, we must acknowledge the bicultural perspectives that occur when members of Third World societies are inducted into the culture of schooling, academia, and the social sciences [Limón 1991].)

Application to Anthropology

Anthropology has begun to travel this same route. Whereas U.S. anthropologists used to have to go to a very "different" culture from their own, they are now doing anthropological research in the United States. However, one more step is necessary. The typical study community is poor, disadvantaged, and an ethnic minority, whereas the usual researcher is middle-class, advantaged, and a member of the dominant majority. The study population is still the Other. But there are signs of change: whereas minority and foreign researchers used to also study Others, it is now much more common for young anthropologists to study their communities of origin (e.g., Limón 1991).

In *Linguistic Anthropology*, Duranti (1997) writes:

As a new generation of students from a wide range of ethnic, racial, and national backgrounds enters the western academic arena, our descriptions are bound to be affected; our discourse of the Other will never be the same. The grandchildren of the "primitives" described by the founding fathers (Boas, Malinowski, Radcliffe-Brown) and mothers (Benedict, Mead, E. C. Parsons) of anthropology are not just reading our books,

they are also sitting in our classes, assessing our descriptions, and, hopefully getting trained to ask new questions and propose new answers. [p. 98]

The last step, suggested by psychology, is for middle-class White researchers to study their own communities from an anthropological perspective. Studies of the dominant cultures in the United States and Europe have been exceptional in the history of anthropology (e.g., Bourdieu 1984; Ortner 1991; Schneider [1968]1980). Whether they are becoming more frequent is unclear.

Instead, the dominant response to the problem of the Other in cultural anthropology is to spend more time writing about yourself and your relationships than about the Others you went to study; this is the *reflexivity* of postmodern anthropology. One of its constructive empirical consequences has been a host of studies that focus on the intersection of "us" and "them"—topics such as globalism, colonialism, and tourism (Appadurai 1991; Ortner 1991). However, this response simultaneously invalidates the classical ethnography: explorations of culture as "sources of value, meaning, and ways of understanding" (Ortner 1991:187). One way to preserve the ethnographic study of culture while eliminating the "Otherness" of the ethnographic subject is to *encourage anthropology students and researchers to study their own communities; this approach gets rid of the problem of the Other in a way that stimulates rather than stymies ethnographic research.*

There appear to be barriers to this plan. The first is the anthropological distrust of empathy (Geertz 1973). Tsing (1993) writes about how feminist anthropologists fear being discredited unless they avoid "any assumptions that women anthropologists have a special rapport with the women of other cultures" (p. 224). Abu-Lughod (1991) speaks of a closely related barrier: anthropology's "conviction that one cannot be objective about one's own society" (p. 139). Thus, cultural anthropology possesses the irony of advocating an interpretive approach yet denigrating the relationships of closeness and familiarity that could maximize correctly interpreting the Other's perspective. The ideal of detached objectivity has not yet been completely banished from cultural anthropology. Nonetheless, the insider perspective appears to be alive and well in linguistic and urban anthropology (e.g., Goodwin 1994; Morgan 1996; Ochs et al. 1989; Vigil 1997); whether or not these models were stimulated by psychology, other cultural anthropologists can derive inspiration from these dynamic and theoretically important examples.

Science as Apolitical

Across fields, the traditional position sees truth as apolitical. This position characterizes traditional psychology as well as traditional ethnographies.

The Postmodern Critique: Anthropology Is Political

The postmodern critique in anthropology sees the differentiation of self and Other, the colonial context in which much traditional ethnography was done, and the nonliterate status of many subjects as representing a form of political oppression. Ortner, in her history of anthropology from the sixties through the eighties, recounts it thus:

In anthropology, the earliest critiques took the form of denouncing the historical links between anthropology on one hand, and colonialism and imperialism on the other. But this merely scratched the surface. The issue quickly moved to the deeper question of the nature of our theoretical frameworks, and especially the degree to which they embody and carry forward the assumptions of bourgeois Western culture. [1984:138]

All generalization began to be seen as oppressive.

Psychology's Response to Politics

Political and social relevance has come to psychology also; however, it has not yet done damage to the empirical imagination. D'Andrade states, "If moral advocates in social psychology do good experimental work, and if this supports their moral positions, so much the better for the discipline. Such work, whatever its animus, because it advances knowledge, promotes rather than threatens the scientific agenda" (1999:8).

Specific Customs vs. Deep Structure of a Culture

The last issue relates to a peculiarity of cultural anthropology that antedates the postmodern critique. This is the fascination with exotic customs and the deep distrust of general cultural principles, principles that might group diverse cultures on the one hand and diverse behaviors and attitudes on the other. Psychologists, in contrast, are always looking for just such principles. It is part of the fascination with universals and the reductionistic desire to explain human beings by a minimum number of principles. Cultural anthropologists, in contrast, are deeply distrustful of reductionism, which is antithetical to first principles of the discipline. However, I would like to tell a story about my own experience in the field. This experience suggests both a heuristic and a theoretical value to the idea of general principles and deep cultural structure.

When I first went to Zinacantán in 1969, I was prepared by experienced members of the Harvard Chiapas Project. They gave much useful information concerning how to act in specific situations. However, I perceived this information as disconnected bits and pieces that I had to memorize individually. When I went back to Zinacantán in 1991, I had just organized a conference on cross-cultural roots of minority child development (Greenfield and Cocking 1994). Its major themes were the constructs of individualism

and collectivism (Triandis 1993) and how immigrants generally brought collectivistic cultural backgrounds with them from their homelands when they came to the United States. I took this conceptual framework with me when I returned to Zinacantán in 1991 for the first time in 21 years. What I found was the following. If I thought of Zinacantec culture as highly collectivistic, the culture as a whole made sense for the first time. Not only that; I could finally figure out how to act in (and understand) new situations—because I had a general principle, collectivism, that could be applied in a multitude of specific situations. I had a deep principle that was generative both for understanding Zinacantec behavior and attitudes and for producing appropriate behavior while I was in the Zinacantec Maya hamlet of Nabenchauk. I was much more successful and confident in integrating into the Zinacantec milieu once I had learned this one very general principle.

From a theoretical perspective, I have concluded that individualism and collectivism are deep principles of cultural interpretation and organization that have tremendous generative value. They do not obliterate specific cultural customs; the customs are simply culturally variable instantiations of the principles (Greenfield 2000). It is much the same as the way that specific languages are culturally variable instantiations of the general language capacity. The implication for anthropology is that it should be open to such general principles as a way of advancing deep understanding of cultures and of avoiding the uninteresting pitfall of ethnographies as collections of exotic customs. Indeed, Fiske (1991), a psychological anthropologist who has taught in a leading department of psychology, has four "structures of social life" that are refinements of individualism and collectivism and are candidates for what I term the "deep structure of culture." Fiske's structures of social life, like individualism and collectivism, are interpretive frameworks. As a consequence, their recognition allows for scientific generalization (important to the discipline of psychology) within the context of the interpretive method (important to anthropology).

Conclusion

In cultural and cross-cultural psychology, we are accustomed to admiring anthropology and considering its contributions to our field, both methodologically and substantively. Anthropologists, in contrast, rarely if ever express admiration for psychology and its amory of methods. However, cultural anthropology in general and ethnographic methodology in particular have, in recent years, been buffeted by the postmodern critique. By and large, the response has been self-flagellation and a movement away from empirical research. Titles such as "The Epistemological Crisis in the Human Disciplines" (Denzin 1996) abound. The point of the present essay is to present another response—a response from the other flank, so to speak. This response

is from the discipline of psychology. Although grounded in a no-longer-tenable principle of objectivity, psychology has some intrinsic epistemological assumptions that have made its empirical enterprise much less vulnerable than anthropology to the postmodern critique.

I used to think that the reason psychology had been left relatively unscathed by postmodernism was that it was simply behind the times. However, I now feel—and hope this essay has shown—that psychology holds the seeds to solving anthropology's dilemmas concerning a number of issues: a single objectivity vs. multiple subjectivities, culture as a homogeneous whole vs. culture as a set of differentiated culture-bearers, fact vs. interpretation, truth vs. construction, the problem of the Other, the politics of research, and specific customs vs. the deep structure of culture. By planting seeds for resolving each of these issues, psychology offers a response to postmodernism opposite to the prevailing one: empirical methodology for investigating the construction of meaning.

Notes

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1. See also Strauss (1999) for an important approach from psychological anthropology to dealing realistically with the nonhomogeneous nature of culture.

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