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**ETHNOGRAPHY  
AND HUMAN  
DEVELOPMENT**

*Context and Meaning in Social Inquiry*

EDITED BY  
Richard Jessor, Anne Colby,  
and Richard A. Shweder

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Richard Jessor is director of the Institute of Behavioral Science at the University of Colorado, Boulder. Anne Colby is director of the Henry A. Murray Research Center at Radcliffe College. Richard Shweder is chair of the Committee on Human Development at the University of Chicago.

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In memory of our friend and colleague  
Donald T. Campbell  
Whose larger vision inspired us all

## 2

### True Ethnography: The Lore, the Law, and the Lure

RICHARD A. SHWEDER

Anthropologist Sir Evans-Pritchard, in a postscript to his book *Witchcraft, Oracles and Magic among the Azande* (1937), reminisces about his disciplinary training in methodology. As a young student in London about to set off for Central Africa, he sought tips about ethnographic procedure from more experienced hands in the field. He went to his teacher Charles Seligman, who told him to “take 10 grains of quinine every night and keep off women.” He went to Edward Westermarck who told him “don’t converse with an informant for more than twenty minutes because if you aren’t bored by that time, he will be.” He went to the famous Malinowski who told him “not to be a bloody fool.” Evans-Pritchard (E. P., as he was known in the profession) then went off to the field and wrote a classic ethnography.

It is not just the British who offer such a rigorous training in ethnographic method. Most American anthropology departments follow the tradition by refusing to require any courses at all in methodology. There are even significant theoretical trends in the philosophy of science which are explicitly “against method.” Methodological advice in anthropology comes more in the form of “lore” than “laws,” of the sort Evans-Pritchard passed on to his readers after returning from the field and writing about what it is like to hunt for a witch in Central Africa: “You cannot have a remunerative, even intelligent, conversation with people about something they take as self-evident if you give them the impression you regard their belief as an illusion.” Now that is a useful and important piece of advice but not quite the kind of advice that is sought by those who are “for method.”

Many anthropologists are “for ethnography,” which they don’t necessarily think is the same as being “for method.” They harbor the suspicion

that methodological pursuits actually get in the way of participating in the flow of life of a community. They think that if you are really serious and want to get to know a place well then the most important thing you can do is just "muck around" a lot like a good journalist, and follow your nose.

Some anthropologists are neither for method nor against it, but simply innocent of it. Indeed, more than a few Ph.D. candidates in anthropology have had the experience described by Dennis McGilvray in a lecture delivered at the University of Chicago on the occasion of the twenty-fifth anniversary of the university's Committee on South Asian Studies. Reminiscing about his training as an anthropology graduate student at Chicago in the late 1960s, McGilvray recounted how he arrived in a remote area of Sri Lanka only to discover that his theoretically sophisticated and fashionable thesis proposal (Erving Goffman's dramaturgy was the rage of the day) was irrelevant to the mundane realities of the local scene, that he had learned the wrong dialect of Tamil, and that he was not going to be able to select his informants. Instead they were going to select him. He wrote his ethnography relying heavily on conversations with his cook, and his postman, and with a few young unmarried men in the community who were eager to talk to him and had nothing better to do.

McGilvray and I were in graduate school in the same era, although at different institutions. Let me report that at Harvard University in the late 1960s the methodological scene in anthropology was a bit more complicated than at Chicago, or at least a bit more conflicted. I remember how in those days two professors on the admissions committee could never agree on which students to admit to the Harvard anthropology program. The first professor wanted students with a high mathematical IQ and a strong background in formal methods—componential analysis, statistics, experimental design and sampling procedures. He liked people who could count and decompose complex things into simple elements. The second professor wanted students with a high verbal IQ and a strong background in history, philosophy, languages, and literature. He liked people who could write, easily pick up the local language, and synthesize their field experiences into compelling narratives.

The first professor thought that one day there would be a manual or rule book for doing ethnography. I think he believed that anthropology could develop mechanical knowledge production procedures (some people call this the scientific method) and that, armed with an artificial intelligence, anyone should be able to write a true and literal description of another culture, the way the other "really is." As far as I know, he never did write an ethnographic book.

The second professor preferred insight over technique, imagination over procedure. With an educated contempt for methodical inquiry, off to the

field on a vision quest went his literate and well-read students. Invariably they managed to return with a revelation, or a metaphor, about a faraway place. Thinking back I wish less time had been spent in graduate school arguing whether it is scientists or humanists who make the best ethnographers. I wish more time had been spent spelling out the character of a "true ethnography."

Characterizing "true ethnography" is, of course, a hopeless, hazardous, and thankless task. It is sort of like trying to define true love. I am going to try my hand at it in this essay. If the process of getting there is half the fun, and half as much fun as they say, I am willing to settle for that, while braving the anticipated wave of antiessentialist criticism. Along the way I will have some things to say about a series of other issues central to the ethnographic project in anthropology: plural norms (what they are, whether they exist, and how you might go about studying them), the role of authority and power in the maintenance of norms, and the place of mind reading in the construction of an account about what it is like to be differently situated, differently motivated human being. At the very least I hope this essay will encourage others to wonder about the aims of fieldwork, and about the nature of the regulative ideals that we ought to share as a community of scholars committed to the study and representation of "others." So here goes. The lecture I didn't get (or perhaps didn't attend) when I was in graduate school.

#### **Somewhere Between Solipsism and Superficiality**

True ethnography resides somewhere between solipsism and superficiality. Its mission is to gain access to other minds and other ways of life so as to represent what it is like to be a differently situated human being. Its practitioners are people willing to hazard border crossings in pursuit of differences that make a difference for the way lives are lived, developed, and experienced, and for the way competence, excellence, virtue, and personal well-being are defined. Its theories tell us what it means to be differently situated—what it is like to have different preferences (values, goals, tastes, desires, ideals of personal well-being and of developmental competence) and/or what it is like to live with different constraints (information, causal beliefs, abilities, dispositions, resources, technology, systems of domination or control). Its methods make use of the things people say and do to each other in everyday life, as well as the things they strategically and deliberately say and do to us on special "scientific" occasions (for example, when we ask them to answer questions in an interview or to narrate a life history), to construct a plausible and intelligible account about what it is like to be someone else. Yet true ethnography also aims to

deepen our understanding of "otherness" and to move us beyond the cover stories, idealized self-representations, well-rehearsed verbal modes of public image management, and strategic manipulations of those whose lives we seek to understand.

True ethnography confronts us with alternative worlds of value and meaning, with different universes of preference and constraint. It tests the limits of our capacity to suspend our disbelief and to access hidden, unconscious, or marginalized aspects of our own subjectivity. It draws us a picture of moral communities where people have different ideas about the desirable states of functioning for human beings.

True ethnography aims to represent otherness in such a way that "we," who are outside the relevant situation, can imagine what it is like to be in it. It tells us, for example, what it is like to be a midlife Oriya Brahman woman whose culturally categorized childhood ("pila") ended with an arranged marriage, whereupon she took up residence in her mother-in-law's house. It tells us what it is like for her to have lived her entire life within the walls of a patriarchal extended joint family compound in a sanctified temple town in India. It helps us understand why she might look forward to the midlife years with the greatest anticipation of satisfaction, why she is relatively indifferent to the physical signs of aging (including menopause), why she is so finely attuned to matters of honor and social prestige, and why she measures her sense of well-being not in terms of autonomy and the liberty to do the things she wants but rather in direct proportion to the magnitude of the social responsibilities she has shouldered, the intensity of the sacrifices she has endured, and the respect she receives from others in exchange for the service she has done as a dutiful daughter-in-law, mother, and wife. It lets us hear her "voice" and her complaints. It forces us to acknowledge the terms (*her* chosen terms, not ours) by which she deeply values a way of life that we may find difficult to value, at least at first blush (Menon and Shweder 1994; Shweder, Mahapatra, and Miller 1990; and Shweder, Much, Mahapatra, and Park, in press; I have done fieldwork among Oriya Brahmans since 1968 and shall make reference to that community throughout this essay).

Such an ethnography tells us provocatively that within the Brahmanical wards of Hindu temple towns in Orissa, India, moral "goods" or "values" such as duty, hierarchy, interdependency, sanctity, and ritual purity count for more than liberty, autonomy, and equality (Mahapatra 1981; Shweder 1990, 1991; Shweder and Much 1991; Shweder, Much, Mahapatra, and Park, in press; and Menon and Shweder 1994, in press). It tells us that most members of such communities prefer certain constraints on their choices in life, such as a taboo against a "love marriage," against divorce, and against widow remarriage (Shweder, Mahapatra, and Miller 1990).

It tells us that most women in those communities actively constrain their preferences in such a way that they develop a taste for modesty and downcast eyes and idealize the behavior of the "shrinking violet" as a feminine virtue. It informs us that while downcast eyes, "modesty," and the behavior of a shrinking violet are associated in the minds of contemporary middle-class Anglo-Americans with concepts such as *mawk, timid, shy, bashful, mousy, sheepish, shrinking, embarrassed, self-deprecating, not ego-enhanced, ashamed, humiliated, degraded, and weak*, this is not true in all moral communities. In Orissa, India, for example, downcast eyes, modesty, and the behavior of a shrinking violet elicits a quite different set of associations: *imprudentious, unobtrusive, reserved, self-restrained, quiet, having humility, coy, demure, self-effacing, not brazen, decent, elegant, delicate, undefiled, unsullied, powerful, virtuous, and good* (see Shweder 1993; Menon and Shweder, in press; Shweder and Menon 1993). True ethnography challenges our willingness to transcend our own habitual modes of moral judgment and emotional reaction. It dares us to view the world in a different light.

#### True Ethnography: The Culture Concept

A true ethnography is about something called a culture. As everyone knows, there are many definitions of *culture*. The definition I was taught as a "mantra" in 1963 in my first undergraduate course in anthropology was "patterns of behavior that are learned and passed on from generation to generation" (a "pattern of behavior" was meant to include any kind of habit, "standard operating procedure" or symbolic action—shaking hands, addressing your father as "Sir," reading the *New York Times* every morning—that had some history or "tradition" behind it and thus could be thought of as having an identity of its own over and above the intentionality and experience of any one individual).

At the time I did not think to ask why the individuals who carry on a tradition would be motivated to simply replicate patterns of behavior, or why they would care to "pass on" their traditions to others. I did not think to ask whether the perpetrators of a tradition were "rational agents" doing what they did because what they did seemed to them, in light of their causal beliefs, to be an efficient or effective way to accomplish some set of worthy goals. The very idea of worthy goals and other questions about the relationship of tradition to the moral order or to a rational order never really came up. It was simply presupposed that the agents of culture have a taste or a propensity to form habits and do traditional things, and that children are inculcated with culture and readily shaped by the desire

for external rewards or the fear of external punishment to "internalize" whatever a group defines as "traditional."

A bit later, in graduate school, I discovered that Sir Edward Tylor's late nineteenth-century definition of culture (as all the inclinations, skills, values, knowledge, and mental states acquired by an individual by virtue of being a member of a group) was still in vogue, although by the late 1960s, under the influence of the cognitive revolution, Tylor's definition was given an "informational" twist: culture was all the information you needed to know to pass or get along competently as a "native." At least that was the intellectual fashion at Harvard, Stanford, Yale, and Penn (although probably not at Chicago, where the cognitive revolution never did arrive).

In more recent times, after several other "revolutions" (the post-structural revolution, the symbolic revolution, the rational choice revolution) and a few "turns" (the interpretive turn, the deconstructive turn, the turn to pragmatics and toward praxis), I have been teaching my own students that a culture is "a reality lit up by a morally enforceable conceptual scheme composed of values (desirable goals) and causal beliefs (including ideas about means-ends connections) that is exemplified or instantiated in practice." Members of a culture are members of a moral community who work to coconstruct a shared reality and who act as though they were parties to an agreement to behave rationally within the terms of the realities they share. In this essay I am going to develop a case for the truth and usefulness of that particular conception of culture.

To get things started, allow me to suggest that we think about the thing called a culture as nothing other than a plausible hypothetical model, articulated in a mental-state language (intelligible to the ethnographer's interpretive audience), which represents the shared and relatively enduring preferences and constraints exhibited in the observable behavior of a designated network of individuals who are tied to each other as members of some self-sanctioning moral community. Accordingly, the methods of true ethnography are designed to reveal things called preferences and constraints, including those that may be taken for granted by members of a moral community, that may be implicit in their behavior, difficult for them to notice, or socially undesirable for them to articulate or disclose under even the most ideal of communicative circumstances.

True ethnography takes time. It requires some degree of rapport and a good deal of trust. Its methods aim to reveal assessments about the value of life that have a ring of authenticity. Because true ethnography is about the structure of other minds, and because it takes time, it resides, quite naturally, somewhere in between solipsism and superficiality, in the vicinity of the discourse and praxis of members of some designated moral community.

In the context of social science methodologies, superficiality can be principled, sophisticated, and predictively useful, and when it is I shall refer to it as superficialism. Superficialism insists that upon demand and "off the top of their heads" the natives tell what they know, know what they are talking about, and keep their answers short. All of us engage in the practice of "superficialism" (for example, whenever limitations of time, resources, access, or courage lead us to rely on interviews to elicit reports from "native" participants about what they think and do, and why, rather than doing it the hard way, by gaining access to the mental states of others through real time participant observation.)

I shall have more to say about superficialism later, when I discuss the way human beings respond to social science survey probes such as, "How satisfied are you with your life on a ten point scale?" or "How would you rate your overall health (excellent, very good, good, fair, poor)?" or "Do you believe in God (very much, a moderate amount, a little, not at all)?" First, however, I want to examine the doctrine of solipsism and its implications for true ethnography.

#### Solipsism in Ethnography: "Let's Talk about Me for a While"

I learned something about solipsism a few years ago, from a conversation I had with Claude Lévi-Strauss. I met Lévi-Strauss for the first time in Paris in 1990, and part of our conversation went like this. Lévi-Strauss told me that he hears the term *postmodern* all the time but does not understand what it means. He told me he would have preferred to live in the past, before the twentieth century, before the postmodern era. "But where in the world would you have preferred to live in the past," I asked, thinking of Athens in the fifth century B.C., India at the time of the Buddha, and all of those cultures of the world—the Nuer, the Trobriand Islanders, the Samnans, the Ifaluk—studied and thereby made famous by anthropologists. "In France, of course," Lévi-Strauss replied. "Why France?" I asked. "Because French subjectivity is the only subjectivity I can ever understand," he replied. "But what about all those other cultures you have studied all your life? Can't you enter into their subjectivity?" "No," Lévi-Strauss replied, "I can only understand other cultures as objects, not as subjects."

Solipsism is the view that the only mental life you can ever really know is your own. This view can be derived from two premises: that the only way to understand something that has a mental life is by getting "inside its head," and that the only head you can ever really get inside is your own. The solipsistic approach to ethnographic method is well represented by the story being told these days in anthropological circles about the

postmodern ethnographer who arrives at a remote field site, recruits an informant, sits her down by a campfire, and says, "Now let's talk about me for a while." The implication is that the only way you can ever get to know what it is like to be someone else is by understanding his or her mental life, and that is impossible. Ironically, despite Lévi-Strauss's apparent antipathy for postmodern thought, it is precisely the postmodern ethnographer who agrees with him that the only subjectivity you can ever understand is your own.

The postmodern "native" seems to agree with Lévi-Strauss as well. I recently had dinner at a storefront restaurant in Chicago, which is owned and run by a Thai family. Prominently displayed on the wall of the restaurant was a poster describing the homeland. It read, "Thailand: The Most Exotic Country in Asia."

Now many of you are probably familiar with the anthropologist's assumption that native peoples experience their own beliefs, desires, and practices as routine, transparent, and commonsensical. That assumption has become old-fashioned. In these postmodern times of cable television, international travel, a global economy, metaphysical jet lag, and hyper-reflexivity, the native peoples of the world are more up-to-date. They know all about the international market in solipsism. They have a canny recognition of the taste of others for impenetrability. They have even come to see themselves, through the eyes of others, as strange and alien. And they are ready to trade on the limits of understanding and to publicize themselves as hidden and mysterious. This keeps the westerners coming. "Thailand: The Most Exotic Country in Asia!"

Perhaps there is nothing new in this self-conscious representation by others of themselves as puzzling and inaccessible, as a black hole beyond the reach of the mental telescope of the West. Perhaps the native peoples of the world have always been a step ahead of their principal investigators. The history of the encounters of the indigenous peoples of the East and South with the indigenous peoples of the West and North (so-called explorers and other anthropological aliens) has been sufficiently bizarre and inhumane that the natives may have (quite understandably) judged themselves to be impervious to the gaze of the westerner and unintelligible to a Northern subjectivity. Perhaps long ago the "natives" of the world decided to turn exoticism to their advantage and to collaborate with Lévi-Strauss on the thesis that French subjectivity is the only subjectivity that a French anthropologist can ever understand.

It is also possible that they read Descartes. For it is Descartes who managed to draw almost everyone's attention to the quite mysterious appearance and nature of consciousness in the natural world. It is no secret that Descartes reasoned that mental states (beliefs and desires, the experi-

ence of seeing red, the feeling of being in pain) are strangers in the natural world. Given that most other things in the world are material, open (in principle) to direct and public sensory inspection and governed by physical laws of causal determination, it is perhaps not too surprising that mental states have the appearance of a singular nonnatural quality: immaterial, inherently private, self-justifying, and bound to a first-person point of view. Whether one refers to this alien spirit lost on a material world as "consciousness," *qualia*, or the phenomenological side of experience, the subjective states that are the *qualia* of life are not open, even in principle, for others to directly observe or inspect. Only one person—you—has potential sensory access to your own experienced states of mind. Your conscious experiences qua states of consciousness are indubitable. They are shielded against criticism and against the external objective standards of the third-person point of view.

There is, for example, no objective neurological or biochemical measure, no "external standard" of pain by which to assess the character of the experience you label "pain" or even to gauge its intensity. In most medically investigated cases of reported headaches, backaches, or chest pains, the brain scan, treadmill test, or X-ray is negative and the clinician has nothing in the natural world to point to as an explanation of, or rationale for, the experience that you named "pain." Typically, the clinician can't even come up with a meager biochemical or neurological (material) correlate of your announced pain. In most cases all the clinician has in his or her hands are your ineffable words uttered in some language ("I have a pain in my chest"), referring to some invisible experience. How can the clinician know whether you use your mental-state words the same way he or she does? What is it you experience? Do you experience a lot of it or only a moderate amount? There is nothing there to grasp or lay hold of except your words, and even they, as words, are immaterial and must be interpreted and translated? Your first-person experience (for example, of pain) is as hermetically sealed as it is self-certifying. Like some mystical experience it provides a sufficient warrant for its own authenticity, but only for the person who experiences it. You can never be wrong that you had the experience you had, and we can never quite be sure that you had an experience, or that we really know what that subjective experience was, if you truly had one. In other words, the only subjectivity that Lévi-Strauss can ever really know is his own. Forget about other Frenchmen. Or so the well-known argument in favor of solipsism goes.

Descartes made us aware of the discrepancy between the essential indubitability of our own experienced states of mind, which no one else can experience, and the essentially contestable character of our speculations and projections about what it is like to be a state in another mind, which

only it can experience. "Cosmic exile" is the phrase used by anthropologist Ernest Gellner to characterize this heightened and somewhat terrifying form of solipsistic self-consciousness in which everyone is exotic to everyone else and separated by a valley of doubt.

If "true ethnography" is possible, then either solipsism is wrong or ethnographic authority is not really threatened by radical doubts about the existence and character of other minds. That is because true ethnography aims to represent the *qualia* of otherness, of other minds, of other ways of life. It aims to make insiders intelligible to outsiders. It tries to remove the "veil of ignorance" that obscures the view between differently situated human beings and makes each seem peculiar and alien to the eyes of the other. A true ethnography does not lead us to gaze upon the other only as a material object or a thing. Nor does it shroud the other in mystery. Nor, quite crucially, does it seek to replace the veil of ignorance with a mirror, turning the other into an all too familiar, all too easily recognized reflection of the self (as the subaltern antiorientalists, who are so hostile to anthropology and are so uniformitarian and monistic in their moral assumptions, would have it). A true ethnography deprives us of neither our differences nor our humanity. It only deprives us of our oddness. Given such goals . . . to represent the other as different but not odd, as intelligible and fully human but human in a different sort of way—it is hardly surprising that some scholars think it is impossible to construct a true ethnography and that the aims of true ethnography are hopelessly incoherent.

If true ethnography is possible, then either solipsism must be wrong, or else ethnographic authority is compatible with radical doubt. But what is wrong with solipsism? I do not think the problem with solipsism is with the premise that the only head you can ever get inside is your own. That seems true enough. I do not even think it is wrong to argue that the only mental life you will ever really understand is your own, as long as you recognize that your own mental life is very complex, and that major aspects of your mental life are hidden from your view and direct experience, and stand in desperate need of anthropological excavation. The same is true of the mental life of others. The unity of human beings is not to be found in that which makes us common and all the same, but rather in a universal original multiplicity, which makes each of us so variegated that others become accessible and imaginable to us through some aspect of our own complex self. I like to dub this last proposition the "principle of original multiplicity" (Shweder 1991, 1993c). It is the principle of original multiplicity that makes true ethnography possible. An alternative way to phrase the principle, indeed its slogan, is "universalism without the uniformity."

### An Antisolipsistic Mentalism

According to the principle of original multiplicity it is not really worrisome if the only head you can ever get inside is your own, as long as the only head you can get inside is complex enough to contain within it everyone else's head as well. The real problem with solipsism, as I see it, is the premise that the only way to understand the mental life of something that has a mental life is by getting inside *its* head, by experiencing it or observing it directly. I think this point of criticism is what Clifford Geertz (1984) had in his mind when he said that one does not have to be an empath to be a true ethnographer. True ethnography can stand on the assumption that it is possible to understand what it is like to be a differently situated human being precisely because to understand the mental life of a differently situated human being one does not have to get inside his or her head. That is the conclusion drawn by such philosophers as Wittgenstein and Ryle, who are sometimes described as philosophical behaviorists. That is the conclusion drawn by those anthropologists (such as Geertz) who were influenced by Wittgenstein and Ryle.

I hasten to point out that philosophical behaviorism is not the same as psychological behaviorism. There is black magic in the word *behaviorism*. So beware of the illusions produced through associations with the wrong history of the term.

Witness, for example, the difficulty that arose back in 1981 at a planning meeting for a conference on culture theory (Shweder and Levine 1984:7-8) when Roy D'Andrade and Clifford Geertz fooled us all by seeming to be at loggerheads over the role of the study of behavior and conceptual schemes (or ideas) in the analysis of culture. D'Andrade made the following historical and ontogenetic observation:

When I was a graduate student [in the late 1950s] one imagined people *in* a culture; ten years later, culture was all in their heads. The thing went from something out there and very large to something that got placed inside. . . . We went from "Let's try to look at behavior and describe it" to "Let's try to look at ideas." Now how you were to look at ideas was a bit of a problem—and some people said, "Look at language." That notion, that you look at idea systems, was extremely general in the social sciences. Oh, I think, the same afternoon in 1957 you have papers by [Noam] Chomsky and [Georgel] Miller and in anthropology, Ward Goodenough. All signal an end to the era of "Let's look at behavior and see what they do." Before 1957 the definition of culture was primarily a behavioral one—culture was patterns of behavior, action and customs. The same behavioral emphasis was there in



linguistics and psychology. The idea that cognition is where it's at struck all three fields at the same time. . . . I think it was a nice replacement. . . . But the thing is now breaking. . . . and we each have different ideas about how it is breaking up.

Clifford Geertz responded as follows:

At the same time the revolution was going on where people were putting things inside people's heads a counterrevolution was going the other way—criticizing the whole myth of inner reality, the whole myth of private language. The one thing that anthropologists hadn't said about culture is that it is a conceptual structure. . . . it's not a psychological phenomenon in the first place. It's a conceptual structure. . . . and that is what the whole depsychologizing of the concept of sense, of meaning, was all about and still is about.

Looking back on this exchange a decade later it seems to me that D'Andrade and Geertz were talking past each other because they are speaking for and against two different types of behaviorisms. D'Andrade was speaking against the antimentalist doctrine of psychological behaviorism, which tried to circumvent the scientific use of a mental-state language (references to beliefs, desires, plans, wants, emotions, goals, feelings) through the study of the behavior of organisms. Geertz, on the other hand, was speaking in favor of the antipsychic doctrine of philosophical behaviorism, which tried to defend the use of a mental-state language by insisting that mental states are not primarily the inherently inaccessible and private experiences of this or that person's consciousness, but rather derive their meaning from the "external" (and hence intersubjectively accessible) concepts that are exhibited in practices and in the observable "forms of life" that lend meaning to a behavior. For the philosophical behaviorist the study of behavior is not a way of getting rid of mental state concepts. It is rather a way to identify them without having to get inside someone's head. In fact, D'Andrade tacitly accepted this antipsychic philosophical behaviorism with his acknowledgement that perhaps the best way to get at a latent unobservable thing such as an idea or concept is through the observation of linguistic behavior. And Geertz tacitly rejected antimentalist psychological behaviorism when he defined culture as a conceptual scheme.

The two revolutions (antipsychism and promentalism) can be joined by insisting that true ethnography is the interpretive study of the sanctionable behavior (the "normal" discourse and praxis) of members of a moral community. The two revolutions can be joined by insisting that the first and most fundamental interpretive act in the study of sanctionable behavior

(discourse and praxis) is the application of mental-state concepts (beliefs, desires) to render such behavior intelligible. For example, to add some concreteness to the idea of the sanctionable behavior of a moral community, in urban middle-class Anglo-America, exclusive husband-wife sleeping arrangements, the family meal, and saying "thank you" are sanctionable behaviors. In rural Hindu India they are not. In rural Hindu India, menstrual seclusion and husband-wife avoidance in public places are sanctionable behaviors. In urban middle-class Anglo America they are not. The first and foremost interpretive act in the study of such sanctionable behavior is to understand the beliefs about persons, society, and nature, as well as the worthy and unworthy motives, desires, and goals that get conveyed to competent members of a moral community by the things other members of the community say and do to each other in everyday life (D'Andrade and Strauss 1992).

#### The Mind Reading of Mental States

That is not to suggest that the interpretive act called true ethnography is an easy act to follow. It is merely to suggest that the application of mental-state concepts to render the behavior of others intelligible is the only act in town, if we are to understand others as "persons." Precisely how difficult it can be to write a true ethnography is best illustrated with simple materials. Consider, for example, the relatively commonplace anthropological observation that in some cultures (among Tahitians, Oriyas, and Chinese, for example) people can be observed responding to apparent "loss" not (as the anthropologist expects them to respond) with expressions of sadness but rather with various kinds of bodily distress such as fatigue, backaches, or sickness. (See Kleinman 1986, Levy 1984, and Lazarus 1991 for a discussion of this type of case; see also Shweder 1993b, a response to Lazarus from which this discussion is drawn.) How do we represent what it is like to be such a people?

I think the way we do it is not by getting inside the other's head but by mind reading. Mind reading begins with conceptual analysis. We begin with an analysis of the concept of a person, which leads us to the concept of a mental life (symbolic capacity, self, will, belief, desire, rationality, feeling, and so on), which leads us to the concept of emotions (self-relevant feelings), which leads us to the concept of sadness, which leads us to the concept of loss. We observe the death of a child followed by the observation that his father or mother are evincing bodily distress but not evincing sadness. Constrained by a framework of meanings internal to our language for persons, mental states, emotions, and sadness, we go beyond what we have observed to generate a series of interpretive possibilities.

One possible interpretation is that the absence of sadness is only apparent, not real. According to this mind read the other has experienced a serious irreversible loss of something that is highly valued and therefore must be experiencing sadness, but either denies it, defends against it via some hypothesized process called somatization, or does not have a vocabulary for describing it or a nonverbal means of expressing it.

A second possible interpretation is that the loss was only apparent, not real. In the ethnographic literature the appraisal of loss is typically a judgment made by the observing anthropologist who has witnessed a circumstance (for example, death of a newborn infant) which he or she thinks of as loss. According to this mind read the other did not appraise things the way the anthropologist did (Scheper-Hughes 1990).

A third possible interpretation is that the loss was real but the other does not respond to loss emotionally. Several kinds of mind reads are possible here. Perhaps others do not respond to loss emotionally because: (1) they are not persons but rather some kind of being midway between robots and persons who can experience fatigue, sickness, and bodily distress but have no mental life (this is not a mind read that anthropologists are prone to make); (2) they are suffering from some kind of psychopathology that has switched off the causal connection between information processing and emotional experience; or (3) in their society a good way to cope with loss is to switch off the processes that bring emotional experiences online, much the way some people in my subculture are able to become fearless, emotionless, and focused when confronted with a sudden and very real threat to their life. (Perhaps in their society derealization or dissociation is a normal or realized state of mind.) One mind read we are blocked from making is that among others, irreversible loss is a context for the experience of pride or joy. We are blocked from making that interpretation by the limits of coherency established by the logic of our own mental-state concepts and language.

Notice that in constructing a true ethnography the other's self-report is neither a necessary nor sufficient datum for weeding out these interpretive possibilities. If the other should explicitly deny appraising the death of a child as a loss, that is grist for our interpretive mill, but we are also free to discount the testimony. If the other explicitly confirms appraising his or her circumstances as loss but claims to experience no sadness and gives off no evidence of experiencing sadness, again that is grist for our interpretive mill. We are free to either mind read an experience of sadness that is not expressed, or to take the other at his or her word, while either suspecting some form of pathology or some form of mental discipline that keeps the other from emotionalizing encounters with the world. Which-ever interpretation we settle upon, a true ethnography is a mind read in

which we rely on our mental state concepts to interpret the discourse and praxis of members of some moral community. Whatever interpretation we settle upon, we do not treat what people tell us in an interview as an incorrigible representation of their inner life but rather as one more piece of information to be made use of, as we construct a model of the mental state concepts exhibited in their behavior.

### Building Conceptual Models

What does it mean for a concept or an idea to be exhibited in behavior and for an ethnographer to spell out the concept? Minimally what it means is that ethnography is the attempt to establish a correspondence between behaviors and something exhibited in those behaviors that is *analytically* external to and separable from the behaviors themselves. I say *analytically* external to the behaviors themselves because it is in the nature of cultural things in everyday life that often no analytic separation is made between a behavior and its meaning. The interpretation goes through rapidly and *unself-consciously*, and the behavior is apprehended as though it were ready-made. We see *in* the behavior (shaking hands) that which it exhibits (politeness). The vehicle of meaning becomes its meaning and it seems as though there is nothing to explain.

To clarify this point, let us assume for a moment that behaviors can be analyzed as alternatives in some choice set. Consider a person who is faced with two choice sets,  $(x, y)$  and  $(x, y, z)$ , and chooses  $x$  from the first set and  $y$  from the second set. Arguing against the idea of "revealed preferences," economist Amartya Sen (1993:498-99) has pointed out that in and of themselves, acts of choice are not like statements, "which can contradict, or be consistent with, each other." He argues that the choice of  $x$  from the first set and  $y$  from the second set are not contradictory or inconsistent choices. They only become contradictory when we try to understand these choices in the light of some idea of what the person is trying to do, which means we invoke a frame of reference (motives, goals, concepts, principles) external to the choice. The choices only become contradictory or inconsistent choices once we interpret them as though they implied two statements of a particular kind, with the first choice entailing the statement " $x$  is a better alternative than  $y$ ," and the second choice entailing the statement " $y$  is a better alternative than  $x$ ." It is Sen's point that "being consistent or not consistent is not the kind of thing that can happen to choice functions without interpretation—without a presumption about the context that takes us beyond the choices themselves."

Indeed, the context may take us far beyond the behavior itself into the pragmatic meanings of the event. Thus Sen notes that while the statement

"x is a better alternative than y" is inconsistent with the statement "y is a better alternative than x," the pragmatic action of actually *saying* or *writing* both statements "may not be really inconsistent in the way the two statements themselves are. . . . For example, the person making the statements may want to be taken as mentally unsound to establish diminished responsibility, or be taken as unfit to stand trial. Or simply want to confront the observer. Or check how people react to apparently contradictory statements." The aims of true ethnography are to construct a model of those statements and purposes that are exhibited in behavior and to turn that behavior into an action that must be read and interpreted, however rapidly, routinely, or unconsciously that reading is done.

#### Building a Conceptual Model: The Case of "Who Sleeps by Whom?"

Consider for example the universal system of behavior known as sleeping and the praxis of arranging the locations where members of a family sleep at night. "Who sleeps by whom" is not only a classic, if under-studied, problem for ethnographers. It is also a universal moral issue that is resolved in quite different ways around the world. Caudill and Plath's (1966) seminal research on Japanese sleeping arrangements revealed that from birth to death members of a Japanese family rarely sleep in a location or room of their own, and that they prefer not to do so, even when space is available. The praxis is an expression of their preferences and not a by-product of a resource constraint. Solitary sleeping (the kind of sleeping pattern forced upon young children by middle-class Anglo-American adults) is emotionally and mentally disturbing to the Japanese.

Sleeping arrangements are one of many practices of concern in my ongoing research on the moral basis of family life and life-span development in Bhutanese war, Orissa, India (Shweder, Jensen and Goldstein 1995; Shweder 1991; Shweder, Mahapatra, and Miller 1990; Shweder, Much, Mahapatra, and Park 1994, in press). It is not feasible or permissible for outsiders to enter the interior spaces of Hindu family compounds to photograph who sleeps by whom, or to directly witness in what order, like peas in a pod, family members arrange themselves on cots or mats. What one can do is collect reports from members of households in which informants are asked in detail about where everyone had slept the night before. The ethnographic goal, however, is not only to document the praxis but to spell out the concepts or principles implicit in the choices being made about how to arrange family members in a common sleeping space.

We have recently constructed a cultural model of the concepts and principles implicit in Oriya sleeping practices in the temple town of Bhubaneswar (Shweder, Jensen, and Goldstein 1995). Although a record of

actual sleeping patterns was at hand, we did not initially try to induce the cultural model from records of behavior. Actual families differ in size and in their age and sex structure, which makes the task of induction quite complex. It proved more fruitful to select a representative hypothetical family consisting of seven members (father, mother, son 15 years of age, son 11, son 8, daughter 14, daughter 3) and to ask insiders (Oriyas) and outsiders (Anglo-Americans in Hyde Park, the Chicago neighborhood in which the University of Chicago is located) to arrange and rearrange members of the family into sleeping spaces under different hypothetical resource constraints. You have one sleeping space. How would you arrange the seven family members? You have two sleeping spaces. You have three sleeping spaces. And so forth through seven sleeping spaces. The number of the sleeping spaces available is the resource constraint.

Although I do not have space to discuss the details of the Shweder, Jensen, and Goldstein (1995) analysis, I would make the following summary methodological remarks. First, it turns out upon logical analysis that there is a large number of ways to sort seven family members into up to seven sleeping spaces. Of course, there is only one way to sort seven people in one sleeping space, and only one way to sort them into seven sleeping spaces. But there are sixty-three ways to sort them into two sleeping spaces. A particular three-sleeping-space solution, for example—father, mother, son 15, son 11, son 8, daughter 14, daughter 3—is only one of 301 possible ways to sort the members of the family into three sleeping spaces. Overall there are 877 logically possible ways to sort the seven family members into one to seven sleeping spaces.

Second, of the 877 logically possible solutions very few, less than forty, are ever selected by any single Oriya or Anglo-American informant. Fewer than fifteen are selected by more than one informant. The solutions are highly constrained, and the task of the ethnographer is to explain all the missing cells by reference to as small a set of moral principles as possible. This is one way to get at "values"—not by asking directly (although direct questioning can sometimes be instructive), but by positing principles to make sense of actual choices.

Third, Shweder, Jensen, and Goldstein (1995) discover that a small set of moral concepts help explain both the missing cells in the hypothetical seven-person sleeping arrangements task, and the arrangements of family members (who slept by whom) in actual families in the temple town. For Oriyas there are four moral concepts implicit in their praxis, which can be listed in a precedence order as follows: incest avoidance, care of the dependent young (children do not sleep alone), female chastity anxiety (sexualized women do not sleep alone), hierarchical deference for males (mature males do not sleep with other males who must defer to them because there

is an implication of disrespectful familiarity; status and familiarity drive each other out). For the Anglo-Americans there are three moral concepts implicit in their praxis, which can be listed in precedence order as follows: incest avoidance, the sacred couple, and autonomy.

Fourth, Shweder, Jensen, and Goldstein discover both dramatic similarities and dramatic differences in the moral concepts and principles implicit in this praxis in rural Hindu India and urban middle-class Anglo-America. For example, the single most important principle in both cultures is the same, incest avoidance. Thus, sexualized family members of different genders (father, daughter 14, son 15, daughter 14; mother, son 15) do not sleep together. All the other moral concepts implicit in their sleeping practices differentiate the cultures. Thus the second most important principle for the Anglo-Americans can be dubbed "the sacred couple." According to this principle, the husband and wife must sleep together and alone. This principle plays no part whatsoever in the choices made in Oriya culture about where people sleep, and it is a principle that is rarely acknowledged on a worldwide scale. The sacred couple principle places great constraints on possible solutions to the seven-person family problem (it rules out 92 percent of the possibilities), and indeed Anglo-Americans can conceive of fewer solutions than the Oriyas. For example, many Oriyas are willing to accept a two-sleeping-space solution that divides the males from the females and honors the incest avoidance rule. But this arrangement violates the sacred couple principle for Anglo-Americans and most of them find this "solution" entirely unacceptable. Many Anglo-Americans can come up with no solution under a two-sleeping-space constraint.

Finally, the research makes it easy to see why in constructing a true ethnography it is imperative to analytically distinguish preferences from constraints. On the ground, under particular resource constraints, the behavior of two communities may look more similar than an analysis of their cultural preferences would reveal. Thus, for example, rural Hindus and urban Anglo-Americans tend to converge in their solutions to the three-sleeping-space problem, despite the fact that their choices are regulated by somewhat different moral concepts and principles. Under a three-sleeping-space constraint Anglo-Americans strongly favor father and mother in one sleeping space; son 15, son 11, and son 8 in another; and daughter 14 and daughter 3 in the third. This is consistent with the two most important Anglo-American principles (incest avoidance and the sacred couple). Under the three-sleeping-space constraint, most Anglo-Americans are willing to compromise on the autonomy of the children. It is noteworthy that a plurality of Oriyas find this solution acceptable as

well. This is because this solution is consistent with their top three principles (incest avoidance, care of the dependent young, chastity anxiety). Under the three-sleeping-space constraint, Oriyas are willing to compromise the hierarchical deference of males. It is true that even under the three-sleeping-space constraint, Oriyas do generate solutions that Anglo-Americans reject (for example, mother, daughter 14, daughter 3, father, son 8; son 15, son 11). Nevertheless, if one were to only observe the behavior of the two cultures at that (three sleeping spaces) resource level, one might be misled into thinking the two cultures were more or less the same. Only when one looks at behavior across a variety of resource constraints are true differences in cultural preferences (values, goals, and so on) revealed.

#### Evaluative Discourse and Praxis

In a recent essay called "A Theory of Culture for Demography," Eugene Hammel (1990) takes up the cause of true ethnography. Hammel, an anthropologist and a demographer, has the following things to say about a "greater reliance on comparative studies of relatively small social units, however large and complex may be the societies within which these are embedded." He suggests that more information about social processes can be gathered by intensive investigation of a few cases than by the superficial examination of many. He suggests that cultural communities need not conform to interest groups or even geographical units.

Hammel suggests that members of a cultural community acquire their culture through praxis; in other words, they resonate to, activate, or absorb unarticulated concepts and principles through exposure to behavior.<sup>2</sup> Hammel does suggest that ethnographers can make progress by collecting commentaries about actual behavioral events from focal participants. He suggests that members of the same culture share not only a language and specific customary behaviors but also an "evaluative discourse," and that the "value of culture for social analysis is not so much that the informants speak to the investigator, but that they speak to one another and can be told us about their behavior is what they tell one another (again the need for participant observation). Most importantly, Hammel suggests a definition of culture as the principles exhibited in the everyday evaluative discourse and behavior of members of some small-scale moral community (for example, a household).<sup>3</sup>