Abstract  This speech to an NIMH audience is about the process of discovery in ethnography and the hazards of presumptive universals in research on family life practices, emotions, moral development, and representations of the life cycle.

A story from the early days of anthropology illustrates a point that still holds true today: the discussion of ethnographic methods is fraught with hazards. The story is found in the reminiscences of Sir E. E. Evans-Pritchard in the postscript to his monumental book, *Witchcraft, Oracles and Magic among the Azande*. Evans-Pritchard recalled how, as a young student in London, he sought advice about ethnographic method just prior to his first trip to Central Africa. He went to his teacher, Charles Seligman, who told him to “take 10 grains of quinine every night and keep off women.” An additional piece of procedural advice came from Edward Westermarck, who counseled him, “don’t converse with an informant for more than 20 minutes because if you aren’t bored by that time he will be.” The famous Malinowski simply admonished, “not to be a bloody fool,” perhaps for being so unwise as to even broach the topic.

With this wealth of methodological advice in hand, Evans-Pritchard went off to the field and wrote a classic ethnography in which he conveyed to his readers what it is like to hunt for a witch in Central Africa. He communicated his sense of having moved from one perspective-dependent reality (the one to which he was accustomed) to another. He even passed on to his readers some of the guidance about ethnographic method that he had sought in vain. Included in his advice was this: “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” (1976[1937]:244). Or alternatively stated: What makes a good ethnography “work” is the “suspension of disbelief” in

which you do not assume that you know everything in advance and thereby permit yourself to experience the reframing of the “really real.”

There are many hazards to discussing ethnographic methods with either psychologists or anthropologists. Psychologists from the Anglo-American cultural region are typically well-trained in methodologies that have nothing to do with ethnography. When they become interested in cross-cultural research and go on field trips they usually carry with them not only a tool kit of experiments and cognitive tasks but also a grab bag of presumptive universals generated from their own experiences and deep-seated cultural frameworks. Those presumptions about persons, society, and nature are usually left unexamined and their unexamined presence results in a science that is merely an extension of predefined analytic grids. Such a science never sets as its aim the discovery of variability in the way people think about persons, society, or nature, or the discovery of alternative psychologies and psychological processes of one sort or another.

In anthropology, on the other hand, most Ph.D. training programs are big on long-term ethnographic fieldwork but do not require courses in methodology at all. There are even significant trends in epistemology and the philosophy of science that are explicitly against method, as evidenced by the influence of Paul Feyerabend’s book, Against Method. While most anthropologists support ethnography, they do not necessarily see this as being for method. They harbor the suspicion that methodological pursuits 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, the most important thing to do is muck around a lot, somewhat like a good journalist, and follow your nose. This is called participant-observation.

Another hazard of discussing ethnographic method is that anthropologists have not always been reflective about what makes for good ethnography. There is a great deal of valuable ethnographic research, and it is terribly important that the field examine exactly what it is that produces the very fine products it produces.

**FUNDING CRITERIA VERSUS DISCOVERY**

In the early 1980s, I served on a study panel at the NIMH that consisted of talented scholars in social psychology, developmental psychology, sociology, and anthropology. You could always tell which proposals were anthropology proposals because they were the thinnest. A good anthropology proposal would have a rich and sophisticated theoretical section followed by one brief methodological paragraph stating in effect, “I am going there and I am going to do participant-observation.” This contrasted with the sociology proposals where “representative sampling” was
the main issue, and if that section was not right the proposal would be
deal. It contrasted as well with the social psychology proposals where af-
after an illustrative introduction of some clever and middle-level hypothesis
12 "scientific" experiments to "test the hypothesis" would be proposed in
great detail. The discussion of these social psychology proposals would
proceed along these lines. The primary reviewer would say, "This is a very
good researcher who has a nice idea and I like experiments number one,
three, and seven, but number two, four, five, and eight have the following
ten fatal methodological flaws," which would then be enumerated. By the
end of the presentation, the proposal would go down, the victim of an
elaborate methodological fault list created by social psychology to make
itself look sufficiently hard-nosed and scientific.

With that sort of evaluation process in place it sometimes seemed that
the only way to write an effective proposal would be to write about experi-
ments already conducted. And, indeed, it was rumored that in the "hard
sciences" at NIMH it was routine to propose research already done and
then use the new funds to go on from there.

One day, after dutifully completing the exercise of finding 25 faults
with experiments number two, four, five, and eight, a social psychologist
reviewer on the panel said: "I want you to understand that this should be
funded and that the discipline of social psychology does not do itself jus-
tice by presenting itself in this scientistic way." He continued, explaining
that, "You should understand that good social psychology is like good eth-
nography in anthropology. This guy is going to go into the lab and engage
in discovery. He is going to muck around until he can find a stable phe-
nomenon and then once he finds a stable phenomenon he is going to start
manipulating and varying things and see what happens. He does not know
in advance what he is going to find or do and it is unfair to evaluate him as
if he does. Treat this researcher on the basis of the richness of his ideas
and have confidence that he will go about things in a systematic and dis-
ciplined way." This was a most illuminating moment for the members of
our study panel. This social psychologist won the day and managed to cre-
ate a new subculture of evaluation focussed on discovery.

**DISCOVERY AND ETHNOGRAPHIC INTUITION**

Ethnography is about discovery. Skillful ethnography is about making
some room for the creative imagination and some disciplined intuition. It
is about entering the field without totally predefining the domain of inter-
est and without presuming that you already know what is universal, be-
cause most of the time those presumptive universals are generated out of
one's own perspective-dependent, context-dependent, and hence local
world.
When researchers do not enter into this process of discovery and do not participate in the flow of life of a community they can get into trouble. One rather humorous example occurred when I was in Orissa, India in the early 1980s. A team of psychologists from Scandinavia were doing comparative research and decided to look at variations in the "universal" family meal. They wired ahead to a prominent local psychologist in this rural area of India and asked him to arrange for a family meal, which they would videotape. Being civil and polite, he did so, without ever telling them that in rural India there is no such thing as a family meal.

The Scandinavian research team spent a few days in the area. The local psychologist convinced some family to sit down at a table together and food was served and they were filmed. But everyone was uncomfortable. Avoidance relationships were being violated. People kept getting up from the table and leaving. No one ever explained to the visitors that family meals should not be presumed to be part of some universal grid. They returned home, coded the material, and made some sort of inference about what was going on, without really ever understanding what was really going on.

This is just one coarse example of the way presumptive universals cause problems, but there are many others. There is not sufficient time for me to go into detail concerning the research I am about to allude to, but these areas provide further examples of the sense of discovery that comes from ethnography.

**PSYCHOLOGY AS ANGLO-AMERICAN CULTURAL STUDIES**

It is curious that there is "Cultural Studies" on the one hand and "Psychology" on the other. In fact, much of what goes on in developmental, social, and personality psychology reflects Anglo-American cultural assumptions and ought to be called "Anglo-American Cultural Studies."

Sleeping arrangements are a good example of this (See Shweder et al. 1995). The ritualized process of separating young children from their parents at night and forcing them to sleep alone is a relatively rare institution outside the Anglo-American cultural region. Yet, we have built up an ethnopsychology, a set of moral values, a set of emotions, and a set of developmental practices tied to separation. Ultimately, this ethnopsychology is lent expert authority in the pediatric literature. Most pediatricians in the United States have strong advice for parents about the emotional damage they would cause by cosleeping with their children.

This ethnopsychology finds its way into the advice columns. Dear Abby tells "concerned aunt" that, yes, she should be distressed about a 40-year old single mother (her niece) who cosleeps with her four-year old son. The mother is accused of being selfish and smothering the child.
Abby's assessment is that the mother needs the cosleeping more than the child. "Concerned" is told to strongly advise the mother to get counseling via a pediatrician's recommendation, as it is desperately needed.

In most other cultures in the world, cosleeping goes on for a very long time, and people mostly turn out normal. We do not know the relationships between these practices, per se, and emotional and mental health outcomes. Important future research could be based on a review of the literature on the effect of these kinds of practices on the long-term things they are supposed to have effects upon. There is an assumption in Anglo-American culture that you will not be independent and self-reliant if you cosleep with your parents. In cultures where cosleeping is done for a long time, often into adolescence, people argue that this creates a sense of intimacy and solidarity in the family.

Is this just cultural ideology? We do not know whether any of these ethnopsychological doctrines on either side of the divide over cosleeping are true, whether the effects claimed by either side are, in fact, true. It is not known if you will turn out healthy by doing what is consensual, regardless of whether it is cosleeping or not cosleeping, by doing what is considered normal by your community, regardless of what that normal practice is.

We also do not know if there are idiosyncratic interactions between temperamental qualities of individuals and the practice those individuals happen to face in their own culture. It is not known if some children are harmed by cosleeping and other children are harmed by not cosleeping. All of this research needs to be done as there is no good data available in this area.

**BASIC EMOTIONS ACROSS CULTURES**

Emotional functioning in different cultural settings is an area of research that needs to be done by collaboration among anthropologists, biologists, and psychologists. There are now comparative grids that have components of emotional functioning that can be investigated to determine whether there are basic emotions. In fact, the area of basic-emotions research has been reopened (See Menon and Shweder 1993; Shweder 1992).

For example, consider the results of a simple triad test on emotions that consists of listing three emotions—happiness, shame, and anger—and asks which is most different from the other two. In Anglo-American culture, most people will say happiness is most different because emotions are viewed hedonically. The notion is that happiness feels pleasurable or pleasurable whereas shame and anger are unpleasant. Another frequent response is that shame is most different, because shame diminishes the ego while happiness and anger both make one feel full of oneself.
Almost never do Anglo-Americans say that anger is most different than the other two.

If the same test is taken to Orissa, India and translated with the terms that are seen as equivalent for these emotions, the majority respond that anger is most different from the other two. The rationale is that anger is destructive of social relationships and that happiness and what is called shame are the glue of society.

Through the discovery process and the ethnographer's investigation of the narratives and the context in which emotions are displayed, it becomes apparent that translating an emotion as shame is terribly misleading. What is translated as shame is really a way of feeling, of dealing with and being in the world that is seen as powerful and good, which is associated with respectful restraint, civility, and upholding the social order. "Shame" is seen as the antidote to anger.

By engaging in discovery it becomes clear that the predefined grid is terribly misleading. Coming from a society in which pride and shame are seen as opposites, with shame seen as something diminishing the ego, and entering a world in which pride is associated with the degree to which you are full of what is called shame, calls into question the validity of our conceptions. To really understand the meaning of the emotions requires an enormous amount of work on local narratives and context.

**MORAL DEVELOPMENT**

Research in the area of moral development or moral reasoning also points to the need for ethnography in order to fully understand the moral domain. Almost all research in the Anglo-American tradition focuses on the predefined subdomain of justice, rights, physical and psychological harm, or some combination of these elements. Having predefined the domain, researchers in this tradition then create research instruments for assessing it.

Soon after I started doing some Kohlbergian interviewing in Orissa, I discovered an Oriya informant who went through the entire interview without ever mentioning notions of rights or justice. The detailed exegesis of the stated and presupposed meanings in that single interview made an impact on my understanding of the moral domain that was more illuminating than the superficial coding of many cases. All this has been reported in Shweder and Much 1991. The informant elaborated an amazingly rational and sophisticated argument about the irrationality of committing a sin. That such a solid, well-constructed argument could be made outside the moral framework of harm, rights, and justice pointed to the fact that the definition of the moral domain we were working with was too narrow.
Through further investigation, which included the conversations of everyday life concerning transgressions and practices, my associates and I have been able to elaborate what we call the "big three" domains of morality. The moral domain is heterogeneous and consists of more than notions of harm, rights, and justice (See Shweder et al. in press). The Anglo-American cultural region has seized upon and elaborated in great detail an ethics of autonomy, represented by notions of harm, rights, and justice. This is but one part of the broad moral domain that also includes an ethics of community that focuses on issues of community, interdependency, and duty, and an ethics of divinity that focuses on issues of sanctity, sin, pollution, and sacredness.

By recognizing that the predefined framework provided too narrow a vision of the moral domain, we developed a much richer base set of moral goods. If we had not discovered that the initial scheme was too narrow, we would have taken things that people find important and significant in their lives and treated them as either premoral, an earlier stage in the development of our conceptions of morality, or as outside the domain of morality.

By investigating the question ethnographically without predefining morality as harm, rights, and justice (the ethics of autonomy), we discovered that all of the criteria of a moral response (the emotions that are activated, the degree to which one is willing to regulate other people's behavior) end up being associated with the ethics of community and the ethics of divinity as well. In this case, predefinition would have ruled out a full understanding of the moral domain and deprived one of the capacity to be surprised.

THE LIFE CYCLE

Another area is life-cycle conceptions. In the Anglo-American cultural region, life stages tend to be thought of in temporal, chronological, and biological terms. People will talk about when they became an adolescent, when menstruation occurred, then graying, muscle change, menopause, and so on. They will talk about mature adulthood as "middle age."

In Orissa, India and many other places in the world, however, life stages are not conceived as being biologically or chronologically defined. In fact, on a worldwide scale, the chronological conception is quite rare (See Menon and Shweder in press).

In studying this in Orissa, Usha Menon and I started with the most minimal assumptions possible. We drew a dot representing birth and another representing death. We did not even draw a line between them, since lines seem to presuppose so much. We drew the dots and asked that informants tell everything that happened before, after, and in between.
Then the narrative was built. By interviewing a number of people this way, we could work out some kind of image of ideal life stages.

For women in extended joint-family households we found a conception of life stages that almost never mentions chronology or biology. Instead, there are a series of status or role transitions that are tied up with social responsibilities within the family. The biggest break is from life in one's father's house to life in the mother-in-law's house.

In breaking it down further, life in the father's house has two stages. A young child is considered semianimalistic, unruly, and untrainable. The transition from that to being someone who can be coached and trained occurs somewhere between seven and nine, but it is defined by when the child begins praying for itself. At that point, the child is thought to have a conscience and an ability to orient to the divine in a way that makes knowing right from wrong possible.

Marriage defines the next life stage. In early marriage there are great responsibilities to serve the rest of the family and to be reproductively successful. Mature adulthood occurs when household management is taken over from the mother-in-law. The final stage is widowhood.

Patterns of happiness or life satisfaction for women at various stages in the life cycle in Orissa are both similar and different from those in the United States. Paul Cleary and his associates at Harvard Medical School have a data set on reported life satisfaction across the life cycle in the United States that shows that, at every point up through the mid-60s, people feel that they are happier now than they were five years earlier. They also feel optimistic toward the future and are likely to think they will be happier five years hence than they are now. During the mid-60s there is a crossover point where the future is no longer anticipated with optimism.

By contrast, in Orissa, India, women in extended joint-family households experience an increased expectation for happiness in what we would call the midlife period. This is their stage of mature adulthood, but again it has less to do with biological age than the taking over of household management. At the point of taking over household management, a set of goods are available having to do with control and community. This brings more satisfaction in life than the earlier stage, which consists of the demands to be reproductively successful and perform tasks as the household servant. Widowhood, the last stage, is highly marked and consists of perpetual mourning. There is a rapid decline in anticipated life satisfaction at this stage that is unlike anything found in the United States.

From the point of view of health and human development, which is the better predictor of health outcomes: knowing chronological and biological age or knowing social structural positions and culturally defined life stages? The research needs to be done, but I would predict that more substantial correlates would be obtained from the social status conception
of the life stage, elaborated from the indigenous point of view, than the so-called objective grid of chronological age and biological change.

ACKNOWLEDGING THE TRUE NATURE OF QUALIA

Ethnography is a species of qualitative research. In my view, qualitative research is really the study of qualia. To understand the character of ethnography, one must acknowledge the true nature of qualia. This presents the largest hazard in the discussion of methods (see Shweder 1996a).

No doubt the distinction between quantitative and qualitative research has something to do with methodology, with real differences between, on the one hand, intellectual operations such as pointing, sampling, counting, measuring, calculating, and abstracting, which I think are associated with quantitative research, and on the other hand intellectual operations such as empathizing, imagining, interpreting, narrating, contextualizing, and exemplifying, which are associated with qualitative research. Although a great deal of attention has been given to the procedures of quantitative research or qualitative research, much more needs to be done in thinking about the objects of quantitative or qualitative research. The objects in question are quanta and qualia, which have somewhat different characteristics. The contrast between studying objects versus subjects has not been resolved and is one of the reasons there is continual tension in the field. Nevertheless, if the distinction was only a difference in intellectual operations, one would be hard pressed to make sense of the persisting tension between quantitative and qualitative approaches in social research. For, as everyone knows, in science as in life, two hands measuring/abstracting and contextualizing/exemplifying are usually better than one. Good research ought to do both.

I propose that the tension between quantitative and qualitative turns less on methodological issues than it does on one’s answers to questions about how to best study subjectivity and how to best study realities that are perspective and context dependent. Basically it is the difference between studying something that exists regardless of your’s or anyone’s reactions to it, and studying things that come into real existence by virtue of their meaning and the perspective that is taken on them. That is why anthropologists experience the surprise of fieldwork, because they walk into a different world that is historically fabricated but real and is tied to a set of frameworks and perspectives.

Most survey research has adopted a set of procedures that make it easy to avoid the surprise of ethnography. It has become clear to me from my work with the MacArthur Foundation Research Network on Successful Midlife Development (MIDMAC) that the survey industry wants to have a
rather special kind of conversation with the American public. While working on a national survey on physical health, mental health, happiness, and social responsibility, we have been constrained to devise questions that pass for anyone who might happen to pick up the phone, whether the person lives in rural Iowa or New York City. The questions must be posed so that the person does not hesitate or wonder what it means. Hesitation on the other side of the phone line is a disaster in the phone survey industry. So all the big differences in cultural framework and meaning get filtered out. Differences in "opinion" are then examined, but never at the level of cultural meaning where the frame differences occur.

Ethnography has been included in the MIDMAC research as a complement to survey methods. The aim of ethnography is to make it possible for you to see how different realities and ultimately different aspects of psychological functioning are tied to meaning systems. I think it is easy to lose sight of the special issues that arise when you study things that are really real but only exist from a subjective, historical, or cultural point of view.

Most people are prepared to acknowledge that to understand something is to compare, and that in some sense or another to observe is to count, even if the \( n \) is one. The real and tantalizing difference between quantitative and qualitative lies elsewhere. It has more to do with the nature of subject-dependent realities versus objective realities and not so much on procedures. By focusing only on the procedural or epistemological side of science, the distinction between qualitative and quantitative is either cut and dry or threadbare: cut and dry because obviously not all intellectual operations are the same—numbers are not narratives and measurements are not meanings—and threadbare because anyone concerned with the prevalence or distribution of a meaning, interpretation, or narrative is going to have to count sooner or later.

The distinction may also seem invidious, because there is an underlying logic of causal inference that applies to all scientific research, whether quantitative or qualitative. Thus the tension between quantitative and qualitative research may seem overblown. In part, I believe, the tension has been overblown but we also should not act as if it is not there or has no real basis.

When thinking about funding new research, the process of discovery and the importance of uncertainty need to be taken seriously. While that may look like softening or loosening the criteria of scientific evaluation of a research proposal, it seems to me that one can and ought to marshal good arguments for allowing people to engage in the discovery process without having worked out everything in advance. I do not think we want to set up the funding process so that it reproduces social psychology's vast and stifling checklist of potential methodological faults.
A FINAL NOTE

Participant-observation has a very important feature that I discovered in 1982–83 while in Orissa, India. That is, sometimes dormant or unknown emotional and cognitive structures within oneself are activated through participation. When they are activated, all of a sudden understanding occurs in a far more profound way.

After working closely with three informants of very different backgrounds in Orissa, I decided to have a seminar in my home with the three. Food would need to be served, and as they had various and different food restrictions, the only solution was to serve “prasad” from the local temple. Prasad is the food offered to the deity who resides in the temple, and who is thought to absorb the essence of the food. Anyone, regardless of background, can eat the god’s leftovers, although Christians would refuse to do so since it is Hindu food. In this case, the god was Lingaraj, a form of Shiva, and the food was vegetarian.

After the seminar, there was a lot of leftover prasad. Although my wife gave much of it to the informants to take with them, there were still leftovers. One of the informants overheard her comment that she thought she might take what was still left and mix it with some chicken that she planned to cook for dinner. Upon hearing this, he cautioned me, saying that the rice and vegetables were prasad, not just leftover food. Since Lingaraj, the god, was vegetarian, he would be very upset. “Do not do this,” he warned.

Later, working in her own culinary and metaphysical framework, my wife cooked the chicken, mixed it with the prasad, and there it was for dinner. I looked at it and said, “I can’t eat this. Lingaraj will be very upset.” Not only that, but I experienced a sense of dread. At that point, I had activated an emotional cognitive scheme that my informants had talked about. It was there in me, waiting to manifest itself once I had the proper set of concepts and experiences from long-term fieldwork in this community.

It seems to me that this is one of the outcomes of participant-observation. People have within themselves an enormous complexity of cognitive and emotional structures. Understanding others is a process by which you will find something within yourself that will be the bridge to understanding difference. It seems to me that without fieldwork it is impossible to reach that depth.

NOTES

Acknowledgment. This commentary is an edited transcript of a tape recording of an oral presentation delivered on October 30, 1995 at the workshop “Ethnography and the Illumi-
nation of Ethnographic Processes.” It summarizes, draws on, and recapitulates material that can be found in Menon and Shweder 1993; Menon and Shweder in press; Shweder 1992, 1996a, 1996b; Shweder and Much 1991; Shweder et al. 1995; and Shweder et al. in press.

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