3

Who Are You To Do This?

Ethnography is really quite an arrogant enterprise. In a short period of time, an ethnographer moves in among a group of strangers to study and describe their beliefs, document their social life, write about their subsistence strategies, and generally explore the territory right down to their recipes for the evening meal. The task is an impossible one. At best, an ethnography can only be partial.

To some extent, the area covered depends on the ethnographer. On entering the community, an ethnographer carries more baggage than a tape recorder and a toothbrush, having grown up in a particular culture, acquiring many of its sometimes implicit assumptions about the nature of reality. And within that framework, he or she developed personal idiosyncrasies, and later went through some professional training, learning a set of biases about which areas of the human situation were worthy of attention.

As if that were not bad enough, a social category will be assigned to the ethnographer by the group members. The category may change over time, but one will always exist. As the ethnographer's role is defined and redefined, it will guide group members in their dealings with him or her. Their expectations of what the ethnographer wants to learn—and their decisions about what should be told—will derive partly from their sense of who he or she is.

These aspects of "who you are" deserve some careful thought. They raise problems for ethnographers, and for all social scientists. Even at this early stage, they show that ethnography is much more complicated than collecting data, and that "objectivity" is perhaps best seen as a label to hide problems in the social sciences. The problem is not

whether the ethnographer is biased; the problem is what kinds of biases exist—how do they enter into ethnographic work and how can their operation be documented. By bringing as many of them to consciousness as possible, an ethnographer can try to deal with them as part of methodology and can acknowledge them when drawing conclusions during analysis. In this sense, ethnography truly is a personal discipline as well as a professional one.

Personality and Cultural Background

Before psychoanalysts are considered competent to analyze others, they must first go through analysis themselves. If they do not understand their own personalities, the argument goes, they will not be able to understand others. A statement made about a patient could be more a function of the interpretation of the analyst rather than anything the patient expressed. By going through analysis, the analyst can hopefully bring some personal background to consciousness and better control the interpretation of the patient.

Ethnographers, on the other hand, are allowed to go into a situation with no awareness of the biases they bring to it from their own cultures and personalities. This simply does not make good sense, but I am not sure how to correct it. For one thing, what constitutes the ethnographer's "culture"? In the case of an American ethnographer, he or she comes from a diverse society. What is "American culture" anyway, and what relationship does it have to all the different subcultures in U.S. society? Then consider a quotation attributed to Clyde Kluckhohn, that "Each man is like all other men, some other men, and no other man." How do we disentangle the many parts and hand the ethnographer–student some information that says "Here are your cultural biases; for the rest of it, go see a shrink."[1]

[1] There are several comments on the psychoanalysis of ethnographers. Devereaux (1967) and Sullivan (1937) argue strongly that the psychodynamics of the ethnographer are critical in understanding the research both as a process and as a product. There is a saying that I have heard that "all social science is autobiographical." After reading Devereaux, I realize how much truth there is in that statement. He shows how in some ways methodology is a response to the particular anxiety that the research situation elicits in the researcher. Blau (1964) offers a case study to illustrate his argument. Among others, Campbell (1961), Nadel (1953), Langness (1965), and Bell (1955) all call for the psychoanalysis of ethnographers. Powdermaker (1966) notes that her understanding of fieldwork changed for the better as a consequence of her personal analysis. Lewis (1953) notes that two ethnographers personally communicated to him that their fieldwork improved after analysis. On the other hand, Herskovits (1954) states that a comparison of work by students who have had analysis and those who haven't shows no differences. And Pelto and Pelto (1973), in their analysis of 51 questionnaire responses from anthropologists, note that the presence or absence of experience with psychoanalysis seems to make no significant difference in the experience of "moderate or severe" psychological disturbance during fieldwork. The results are obviously sketchy and inconclusive at this point. While I have never been psychoanalyzed, I have worked around practitioners since I began doing drug research. I agree with the discussions in the literature that an ethnographer has much to learn about the notions of "transference" and "countertransference." And Devereaux's book is a good dose of reality therapy for anyone who believes that one human dealing with another is similar to a physicist studying photographic plates from a cloud chamber.

Yet the problem is important. How many times have you heard a person describing someone else, and had the feeling that by substituting "I" for all the "he's" and "she's" in the description, the person would be talking about himself? As an exercise, read some ethnographies and try to analyze what the statements say about the ethnographer rather than the group under discussion. You will have to guess. Important as that information is, seldom do you learn anything about it in the text.

One folk theory in anthropology to explain the differences between Lewis's and Redfield's work has to do with the differences between the two ethnographers. Redfield, so the story goes, romanticized rural peasant life. It was closer to a "natural state" than modern urban life. Therefore, his description of village life, especially when contrasted with urban life, emphasized the more harmonious aspects. Lewis, on the other hand, had a personal view of the world that allowed for, if it did not emphasize, the darker side of life. When he went to the same village, things like hostility, greed, and jealousy were stressed.

This concern with the personality and cultural background of the ethnographer becomes even more critical when you consider that the ethnographer's background is the initial framework against which similarities and differences in the studied group are assessed.[2] Appropriate to Lewis and Redfield, one article argues that the ethnographer's attitude toward his or her own culture conditions the evaluative tone of the description of the studied group. The article suggests that the less the ethnographer likes his or her own culture, the more favorably the alternatives may be viewed.[3] Redfield, living and working in the Chicago area, might have gone into rural Mexico with the idea that "his" culture—urban America—represented a deterioration of the human condition.

The importance of the ethnographer's background is also emphasized in some of the discussions of ethnographers working among their own people. Now the ethnographer's framework is not as different from those of the group members, and the similarities and differences are not as striking as they might be. For example, Joan Ablon, working with middle-class Americans, also

[2] For sample statements about measurement against one's own cultural background, see Den Hollander (1967) and Wolff (1945).

[3] Braroe and Hicks (1967).

stresses the problem of the ethnographer's attitude toward her "own" culture, and also mentions related problems, like the difficulty in judging significant behavior and her own vulnerability as a member of the same society.[4]

Another kind of discussion also emphasizes the importance of the culture–personality background of the ethnographer. In Chapter 1, the argument about whether insiders or outsiders should study a group was mentioned. In increasing numbers, the "natives" are becoming ethnographers and reviewing the past work of their American and European colleagues. There is a recently formed association of third-world anthropologists that is posing some basic questions about anthropological culture, grounded as it is in Euro–American society.

For example, an African anthropologist recently published a discussion of some previous ethnographic research. His article had the rather unflattering subtitle of "the usefulness of the useless." He argued that some of the classic studies displayed fundamental misunderstandings of the group under study by the ethnographer. Among other reasons for this, he mentioned the colonial context, the loose methodology, and the tendency to rely on a few key informants. But the mainstay of his critique is the inability of the ethnographer to understand and communicate in the vernacular. Because of all these various background factors, the descriptions of African society reviewed are, at best, oversimplifications, and at worst, flat wrong.[5]

After reading all this material, I get the dizzy feeling that an ethnographer (or any social science researcher) is like a drunk pretending to walk a straight line in a dark room with a gale-force wind blowing though it. It's clear that the ethnographer's culture–personality background, though increasingly acknowledged as critical, is a great unknown in ethnographic research. To make things worse, it's not clear how to integrate it into discussions of ethnographic methodology.

Let me give a couple of examples from my own research. Eye contact is something that varies from group to group. There are times when you look people in the eye and times when you do not. I did not know that when I went to South India. I was not aware that that particular area of behavior was culturally variable, and that my rules were only one set of a number of possibilities.

When I arrived in South India, I was horrified. Gopalpur is in an area that was formerly administered as part of the princely state of Hyderabad. The

[4] Ablon (1977). For examples of similar discussions, see Colfax (1966) and Cassell (1977).

[5] Owusu (1978). For an earlier critique of African studies involving the cultural background of the ethnographer, see Maquet (1964).

ruler, called the *Nizam*, signed a treaty with the British, and they, by and large, left internal affairs to him. Unlike other areas of India, then, the villagers were not used to the presence of Caucasians. A young Caucasian, running around in a *dhoti* and *chappli*,[6] without the expected ornaments of a wristwatch and a row of fountain pens, was indeed an unusual sight. Of course, I did not know that; I thought I was "acting normal."

The villager's reaction was a friendly curiosity, so they would come up and stand a few feet away. Then they would clasp their hands together behind their backs, lean slightly forward, and stare. In time I would become annoyed at such obvious rudeness and say, "*eenappa?*" ('What do you want?'). They would change posture, look a bit startled at such an irrelevant question, and say, "*eenilla*" ('Nothing at all'), and then sometimes resume staring.

My rules said such eye contact was rude in the extreme. It was so unsettling in the early days of fieldwork that sometimes I would hide and read or write rather than go out and deal with it . When I did go out, I would try to ignore it, but it usually made me impossibly self-conscious. While struggling to look controlled externally, my mind would be screaming, "Quit the staring. If you want something, what is it? Go away." Actually, my mind was screaming things much more colorful than that.

It took some time to get over the noise introduced by that small piece of my cultural baggage, and that is only one example. Similar problems occurred in New York, my "own" culture. For example, consider the simple area of question-asking. The way I grew up, there were certain things one did not ask questions about, things that were defined as "personal." But there were a great many things you did ask questions about. In fact, you were *supposed* to ask questions—it showed you were "interested" in the other person, and a well-phrased question showed your "intelligence."

For the ethnographer, question-asking is vital. In fact, as we will see later, ethnographic question-asking is a special blend of art and science with a variety of subspecies. As you begin to learn from a group, you need questions—both to add to your knowledge and to check things that you think you understand. Ethnography without questions would be impossible.

In the streets, though, I learned that you don't ask questions. There are at least two reasons for that rule. One is because a person is vulnerable to arrest by the police, or to being cheated or robbed by other street people. Questions about behavior may be asked to find out when you are vulnerable to arrest. Or they may be asked to find out when or in what way you can be

[6] *Chappli* are the sandals that villagers wear, while a *dhoti* is several yards of cloth that men wrap in different styles to cover their lower body.

parted from some money or heroin. Even if one sees no direct connection between the question and those outcomes, it might just be because one has not figured out the questioner's "game" yet.

The second reason for not asking questions is that you should not have to ask. To be accepted in the streets is to be hip; to be hip is to be knowledgeable; to be knowledgeable is to be capable of understanding what is going on on the basis of minimal cues. So to ask a question is to show that you are not acceptable, and this creates problems in a relationship when you have just been introduced to somebody.

No wonder people were hestitant to talk with me, or I should say to answer my questions about what was going on. I had to learn to avoid questions initially, to allow people to watch my behavior for a while. Then I had to carefully explain why I was about to start asking questions. Often I would introduce a question by saying something like, "I know this is a dumb question, but . . .," or "I don't know whether or not it's cool to ask this, but . . .". As people came to accept my role as a curious straight and decided I was not a cop, the problem lessened. Also, as time went on, I could behave knowledgeably, in some areas at least. If I "layed back" first, and then asked questions, it also reduced the problem.

That was another small example of interference from my own cultural background. These two areas—eye contact and question-asking—are behaviorally minor but powerful in their effects on the relationship between ethnographer and informant. Your own culture does not only influence you at this level, it can also guide your interest toward different topics at the expense of others.

As one example, there is a proliferation of ethnographic studies of women's roles in different societies, and an interest in women's speech in linguistics and its hyphenated relatives. The emergence of this new interest is clearly part of a larger change in American culture, or at least the part of it that self-consciously talks of traditional women's roles in U.S. society and the need to change them.[7] Another example would be drug research. Until not so long ago, ethnographic studies of drug use (excluding alcohol) were almost nonexistent. Now there are a growing number of studies in this area, covering a variety of psychoactive substances in a number of cultural settings.

When I went to South India in 1965, I had never had any personal experience with marijuana. While I was working in the *tanda* ('village') with the Lambardi, they had a ceremony at the nearby Hanuman temple. The

[7] Golde (1970) has collected a series of personal accounts of female ethnographers. In her introduction, she suggests that women in the field have different problems than men. For example, they may be more concerned with protection, less subject to suspicion, and more pressured into conforming within the group studied.

temple was only a remnant of a building that stood in an abandoned village, but the Lambardi used it to make offerings to Hanuman for the continued fertility of the women. The entire *tanda* walked to the temple behind the priest who had come especially for the ceremony, with everyone singing religious songs to Hanuman. As I was moving around trying to watch everything that happened, someone handed me a clay pot full of liquid. It tasted sweet, though it had things floating in it. The clay pot came around more times, and I drank from it each time.

A short time later, I stopped trying to watch everything. I found myself caught up in the music and singing and the smells of the burning incense. The red and yellow decorative powder took on a deeper color, and I stared at the image of Hanuman. I remember this very well because I began wondering if I had gone too far into the culture. As a committed agnostic at the time, the feelings I had made me wonder if I was being converted.

I was stoned. Only after returning to the U.S. did I learn about *bhang* and its use on ceremonial occasions. It did not occur to me to make a connection between the drink and my involvement in the ceremony. I think I attributed it to the hypnotic effect of the music. That may sound naive in retrospect, especially to younger readers of this book, but I'll tell you one that is even worse.

In Gopalpur, sometimes a group of village men would gather for a *bhajani*. One man would play a single string instrument made of a gourd and a bamboo stick, while others would play finger cymbals. The songs were religious in nature. At times, I would notice that one of the men would pack a short, cone-shaped pipe and light it, and then pass it around. There was nothing unusual in this since that is the way men usually smoked tobacco, though they did not often pass it around. I think I remember thinking something like, "Isn't that nice. They're short on tobacco, so they're sharing it."

I didn't get that one straightened out until many years later, when I became a "drug researcher." In conversations with Alan Beals, he reminded me of the marijuana smoking at the *bhajanis*. I had that feeling you get when the elevator stops too fast—I had missed an important part of a common village ceremony. The reason, of course, is related to the lack of ethnographic studies on drugs. Drug use, again excepting alcohol (*our* drug), was not something to pay attention to. I had no background knowledge to lead me to isolate it as a significant topic. I did not know enough to begin asking questions about it, or to make "marijuana use" the basis of interpreting the behavior I was observing. Now, of course, if I saw some Indian villagers smoking something during a religious songfest, I would wonder if it was marijuana. But then things have changed in my culture since 1965.

In addition to your personal and cultural biases, you also have been "programmed" with some powerful professional ones. What have you been taught to look for? Mother's brother and sister's son? Expended calories? Nature versus culture? Or a voluntary association? Have you been told to hang around and keep notes? Conduct a survey? Or ask people to sort things? It is quite likely you have been through a funnel, coming out at the narrow end as you finished your graduate training.

To some extent, the narrowing is impossible to avoid. First of all, there is an obvious information explosion going on. Just recently I received notices of two new journals that I should read, and heard of another by word of mouth. We are getting to the point where you will not pick journals to subscribe to. Instead, you will pick the several library search services that you want to use.

Second, the old disciplinary boundaries are crumbling—except as they survive, and will for centuries, I am sure—as established bureaucratic divisions within the university. Who has time for anthropology journals when there are all those things one should know about in the journals of other disciplines? The increased information flow is approaching overdose proportions. As one professor (with tenure) was heard to say, "Stop publishing or we will all perish."

However it is explained, the narrowing of one's focus will obviously be carried into the field. I can't tell you much about the economic behavior of the Lambardi. I know a bit about sources of cash, the role of reciprocity, and the kinds of crops they grow, but that is all. At that time, I had no training—and no particular interest—in economic anthropology. Professional training further widens the blinders of bias.[8]

This discussion circles around a general point. Whether it is your personality, your rules of social interaction, your cultural bias toward significant topics, your professional training, or something else, you do not go into the field as a passive recorder of objective data. During fieldwork, you are surrounded by a multitude of noises and activities. As you choose what to attend to and how to interpret it, mental doors slam shut on the alternatives. While some of your choices may be consciously made, others are forced by the weight of the personal and professional background that you bring to the field.

First of all, this background is another good reason to worry about methodology. Methodology will not help if you do not know to ask the

[8] Den Hollander (1967) argues that as a consequence of professional training, ethnographers tend to overrate the influences of beliefs on behavior, to overemphasize homogeneity, to overdo order and coherence and underplay exceptions and contradiction, and to emphasize structure at the cost of process.

question. But if you do document your learning with some procedure that publicly displays some of the experiences you had that led to the conclusion, and that potentially might have *falsified* that conclusion, you can at least show that your bias was supported by something somebody did or said.

A second strategy, as hinted at earlier, is contained in the idea of a discipline. Think about what you are doing. Force yourself to look at the same material in a completely different way. Use Descartes's method of systematic doubt and reduce yourself to basic premises. As you gain more experience, you will become conscious of more and more of your biases. As you bring at least some of them into awareness, you can describe them to others, show how they guided the kind of work you did, and suggest alternatives that might emerge if the ethnography had been done with another set of biases.

A third possibility is to have bias-awareness programmed into ethnographic training. In my field methods class, we do an exercise where students ask someone to describe a physical setting. Then they go to that setting and do their own description. They usually come back with a different description, so they go back to the informant and explore the reasons for the differences. Sometimes the differences are trivial, but it makes the point. Different personal biases lead to different descriptions. Perhaps a more sophisticated series of exercises could be assembled to refine the consciousness of professional ethnographers.

Yet another possibility would be to borrow the analyst's training model and require someone to study the ethnographer before he or she studies anybody else. At a minimum, an elaborate life history interview could be done with the prospective ethnographer. The material could be analyzed by anthropologists of both cultural and clinical persuasions. The results might then be fed back to the trainee. Besides, it would be good for the ethnographer to experience the role of informant.

A final strategy is to use more than one ethnographer. If several people examine a similar area, the differences in their biases will generate contradictions in their reports. Contradictions, rather than being viewed as threatening, should be seen as the beginning of a better question, a signpost pointing to a more sensitive understanding. Too many potentially rich contradictions get lost in the politeness rules of academic rhetoric.

Some of your biases will be jolted into awareness; some will only slowly emerge; and some will always lurk unrecognized in dark corners. Fieldwork presupposes an interpretive framework; and an interpretive framework cuts into the world like a jigsaw, leaving much of the wood behind. On the other hand, maybe some frameworks slice more "naturally" than others. That raises an interesting, and hopeful, idea that will be explored in detail in Chapter 9. But while you work on ways to climb out of this hole in the sand,

treat yourself to a little self-indulgent arrogance. Some social scientists think that objectivity is an accomplished goal.

Ethnography and Personality

Besides the background of personal experience discussed so far, there is an elusive "something else" relevant to the makeup of the ethnographer. This "something else" has to do with those features of one's personality that are particularly adapted to ethnographic tasks. This is something different from, yet related to, the background experiences you acquire as a member of a particular society. In fact, certain aspects of personality may be human universals. I mention this because the aspects of the ethnographer I want to discuss are probably things that vary within any group. While they are certainly not totally independent of society and culture, some aspects are probably found in them all. There may be something here to help you identify the best key informants and field assistants.

One of the things ethnographers must deal with is "culture shock." The shock comes from the sudden immersion in the lifeways of a group different from yourself. Suddenly you do not know the rules anymore. You do not know how to interpret the stream of motions and noises that surround you. You have no idea what is expected of you. Many of the assumptions that form the bedrock of your existence are mercilessly ripped out from under you. The more you cling to them, the less you will understand about the people with whom you work.

One of the more interesting discussions of an ethnographer's adaptation to culture shock is an article by Dennison Nash. One way to understand ethnographic research, he argues, is to understand how the ethnographer adapts to the stranger role. As a stranger, he is cut loose from his former significant others. He has a strong sense of increased possibilities, and is overwhelmed by perceptual chaos.

One response to this situation is *authoritarian closure*—when central premises of existence are strongly asserted and contradictory information from the new situation is denied. Like-minded strangers might establish "enclaves" for mutual protection. But another type of response is what Nash calls "autonomous man," one with a high tolerance for ambiguity and uncertainty. He thinks that ethnographers are of this second type.

Nash, like others in the literature, uses the phrase "detached involvement" to characterize the successful ethnographic role. One is, at the same time, part of and distant from the community. One struggles to understand with involvement in the society; at the same time, one stands back critically to

examine what one has learned. However, this detached involvement—this stepping into and out of society—is a strain in its own right.[9]

There are two obvious ways to lessen the strain. Either keep your distance or "go native." You keep your distance at the risk of failing to understand the complexities of a human situation different from your own. You go native, but then stop functioning as a social scientist. Actually, real ethnography represents some of both these strategies as the ethnographer moves around the goal of detached involvement.

Recently, the diary of Bronislaw Malinowski was published. During World War I, he was interred on the Trobriand Islands. While he was there, he did some ethnographic research, subsequently published in several volumes, that is still considered an admirable piece of work. But when his diary came out, many were shocked. In it, he sometimes called the Trobrianders "niggers," and made other comments that were cruel and hostile as well.

How could this be reconciled with the sensitive Malinowski who talked of the magic moment when, just for a few seconds, you saw the world like the natives? I think he was "distancing"—taking a psychic vacation from the intense involvement of living like a Trobriander. He sometimes dipped a bit heavily into the involvement side, and balanced the scale with some detachment. Some ethnographers I have talked with, like me, were not surprised by the contents of his diary.[10]

Once, in the *tanda*, I looked up and saw a villager approaching me. This occurred at a time when I felt particularly unhappy with my work and particularly distant from the Lambardi. I smiled and said (in English) something insulting. At that moment, I hated the Lambardi, hated the *tanda*, hated India, and wanted nothing more than to be on a vinyl seat in a neon-lit, air-conditioned coffee shop eating a hamburger. I could tell similar stories

[9] See Nash (1963). Meintel (1973) also discusses culture shock, characterizing it as a form of self-discovery that should be sought out rather than treated as a problem. She also stresses the "shock" of returning, as does Williams (1967). Andersen (1971) analyzed some of the dreams of American academics travelling in India. She outlines a change in dream content from an initial retreat to earlier life events, followed by the establishment of a "secondary identity" that allows dreams with mixed, but clearly distinct, American and Indian elements. Many others discuss the notion of detached involvement, including Jones (1973), Powdermaker (1966), Freilich (1970a,b), and Pelto and Pelto (1973). The "stepping into and out of society" phrase is taken from Powdermaker's book.

[10] Malinowski's diary is listed in the References. There is an interesting exchange on the diaries between Geertz (1967) and Powdermaker (1967) in the *New York Review of Books*. Geertz, in his review of the diary, concludes that Malinowski was distant and aloof during his field work. Powdermaker's reply was to balance this conclusion by noting that Malinowski stressed participant observation and understanding the native point of view. As his former student, she remembered that he called everybody names, and she wonders about the translated "nigger" from a diary written in Polish.

about my work with heroin addicts. It was a "harmless outlet" for the personal strain of the moment. I would hate to have my entire field experience judged by that bit of behavior, especially when compared to other moments of warmth and intimacy. Detached involvement is a winding path.

You might think that doing ethnography in one's own society would be less stressful. I find it more so. During World War II, an anthropologist was asked to find out why fighter pilots had more mental health problems than the infantry. He noticed that fighter pilots sit around the officer's club with a drink, then suddenly run to their planes and find themselves in combat. After they land, they are back in the comfortable officer's club.

The infantry, on the other hand, rested for a period of time in a noncombat area. Then they slowly geared up and marched into combat. They remained there for some time, then slowly marched back for more rest and recuperation. So, thought the anthropologist, the dramatic changes for the fighter pilot were probably more stressful; hence, a higher incidence of problems.

In ethnography of the traditional sort, there is a period of travel and adjustment of the field setting, followed by a long period of time in residence. After the fieldwork is over, one travels home and readjusts to the home culture. When you work in your own society, you cross the line between the field and home often and rapidly. While this does have some advantages, it can also produce "fighter pilot" stress.

One day in New York I had a particularly good few hours in the field. By that time, I knew several people in a particular neighborhood in the Lower East Side. My competence in "street talk" had returned. It was a pleasant fall afternoon and evening. I sat on a stoop to drink a can of beer and talk, went into the corner store and exchange friendly insults with the proprietor, and spent some time talking with youth in the local court-referral program.

Later in the evening, I had a dinner engagement with a friend on the Upper East Side. During the short subway ride, I thought continuously about the warm experiences I had just had. When I surfaced near the restaurant, I was jolted by the differences. The differences between the two neighborhoods is best explained in terms of money, with all that that implies. I walked into the restaurant, and it and the people in it were a mid 1970s self-conscious imitation of someone's idea of the 1960s. As I greeted my friend and looked around, the scenes seemed to shift like the jumping frames of an old silent movie. I had to leave and regroup for a couple of minutes before I could sit down and talk comfortably.

Changes were seldom that dramatic, but once in a while they were. Sometimes jumping that boundary once or twice a day is a bit much. While working in your own society, you still have the stress of detached involvement, compounded by the substitution of frequent repeated minidoses of

culture shock in place of the one huge jolt that you usually get in more traditional forms of fieldwork.[11]

An ability to handle this kind of stress is obviously a desirable one for an ethnographer to have. Without it, anthropologists are going to view ethnography as something they are forced to do to get their union card. They will keep their distance or hide in enclaves to avoid culture shock. Then, after going through some kind of minimal adjustment, they will remain well on the detachment side of detached involvement. The folklore has it that this sometimes happens. Such people should not be forced to do ethnography. There is plenty of room, and much need for nonethnographic anthropologists.

There are other aspects of personality that are no doubt adaptive for ethnographers as well. John Price speculates that ethnographers are not "men of action"; rather, they tend to be shy and nongregarious. The Peltos think fieldworkers are flexible, humble, and sensitive, with an ability to meet on the basis of face-to-face human universals. Wax says the most important attributes of an ethnographer are luck and *manvit,* a term meaning "intelligence manifest in common sense, shrewdness, and flexibility." She also notes the importance of the ability to appear the fool. Powdermaker writes that the fieldworker should be outgoing, should like himself, expect others to like him, and be able to communicate easily and directly. In addition, the fieldworker should have an ability to be psychologically mobile among different social categories and be able to handle tension.[12]

That's quite a list, and its only a sample. Further, the authors always carefully qualify their list of attributes, noting that they are just speculating on personality features of ethnographers. As I review the list, and think of my own experiences, some things do clearly appear to me as critical. The ability to tolerate uncertainty is one example. Uncertainty creates anxiety. Yet in field work especially in the beginning, one does not know how to interpret or react to much of what is going on around him. This particular aspect of culture shock is unsettling, to say the least, but to learn a new way of viewing the world you have to rebuild from the gound up.

Another desirable personality feature might be a willingness to make mistakes. I suppose that this is really a tolerance for uncertainty with a personal cost attached. Ethnographers must take new bits and pieces of knowledge and test their understanding of them using their own behaviors. If they err, people will

[11] My experience is contradicted by Pelto and Pelto (1973) who note that less tension was reported by those who worked in North America, including those who worked with American Indians. Perhaps those reports are from ethnographers who entered situations different from "home" and stayed there until work was finished. My description is based on a field situation where the line between fieldwork and life is blurry and traversed frequently.

[12] See Price (1973), Pelto and Pelto (1973), Wax (1971), and Powdermaker (1966).

react with concern, sympathy, embarrassment, or laughter, but the message will be clear. Mistake-making is an important method in informal ethnography, but you must be able to handle the potential threat to your self-esteem.

Pity the poor ethnographer. And we haven't even considered some of the other sources of strain. For example, some note the professional importance of fieldwork. There's a lot of professional reputation riding on these efforts, and the inevitable mistakes can shatter an emergent professional identity. Some discussions of the ethnographic role conclude that there are few set rules. In my opinion, that's a realistic conclusion, but it only adds to the uncertainty. Then ethnographers must often conduct themselves with several reference groups simultaneously in mind—their own society, their study society, and their professional society—and the demands are not always consistent.[13]

At this point, we might review the entire psychological literature and speculate on which parts apply to the practice of ethnography. In fact, much of social science applies to social scientists, something that is not discussed nearly enough in the literature. It would be an interesting exercise to assemble a portrait of the perfectly adapted ethnographic personality (or personalities), and perhaps someday someone will.

Of course, no one person would match it. As I think of a particular individual who is one of the best ethnographers I have ever known or read about, even he is not immune to culture shock or the problems of overdistancing, and even he sometimes suffers the anxieties of uncertainty and the mortification of the unaware fool suddenly exposed.[14] No one is immune, but some are, or become, more adapted to the task than others.

Presentation of Self

When you begin doing ethnography, group members are going to wonder who you are. They will listen to you and watch your behavior, and they will draw on their own repertoire of social categories to find one that fits you. At

[13] Just as examples, Wintrob (1969) discusses the professional importance of fieldwork as a source of strain. Cohen *et al.* (1970), introducing four accounts of entering the field, note that there are no hard and fast rules. And Kloos (1969) discusses the role conflicts of the fieldworker.

[14] The Peltos (1973), after analyzing their 51 questionnaire respondents, reported that about half reported at least "moderate" psychological disturbance. Presence of spouse, prior fieldwork experience, and psychoanalysis seemed to make no difference in presence or absence of stress. Of the 51, they classed 19 as quantifiers, 19 as mixed, and 13 as nonquantifiers. They then noted that field research training was related to a reduction in stress among the nonquantifiers only. Quantifiers, they reported, often reported more tensions.

the beginning, you will offer some explanation of what your interests are and what it is you intend to do. This initial presentation of self leads us right into the issue of research ethics.

Until recently, there was not much of a formal statement of ethics for social science research. Then several things happened. Within anthropology, a scandal erupted at the 1970 meetings of the American Anthropological Association.[15] Some anthropologists were accused of doing "counterinsurgency" research in Thailand on the basis of records stolen from an anthropology professor's office. The business meeting, usually sparsely attended, was mobbed and continued for two nights. There was much heated discussion, followed by committees, investigations, and reports.

In my opinion, there were no heroes. I am still not sure exactly what happened in Thailand, though some individuals did collect information potentially harmful to their informants. At any rate, partly as a result of this event, the association adopted a more elaborate statement of professional ethics and formed a committee to hear complaints of unethical behavior on the part of its members.

At about the same time, the federal government began worrying about the rights of people who served as subjects in research projects. Their concern was also motivated partly by other events, like the scandal of using prisoners in high-risk medical experiments. From this came some guidelines for the protection of human subjects. Evaluating how well protected research subjects are now constitutes a separate part of the grant-reviewing process.

For these reasons, explaining who you are is more than a local methodological problem. It is an act for which you are held accountable by your profession and your funding source. There are many implications for ethnographers in the code of ethics and the guidelines. For now, the implication that concerns us is that people must be informed of your role—who are you and what do you want.

I would think that most ethnographers have always done this anyway. But there are different ways of explaining your role. For example, if you say, "I am an anthropologist and I am here to study your culture," the description, though technically correct, may not be very informative, especially for a group who has no idea of what "anthropology" and "culture" are all about.

On the other hand, suppose you say, "I'm here to write a book about you folks." Though devoid of references to social science, the statement may be more informative for some groups. It implies that you are going to explore

[15] Prior to this event, the Project Camelot disaster had occurred in Latin America, involving anthropology as well as other social sciences. Wax (1978), in his discussion of Deitchman's (1976) book, writes that it represents in social science the analogue of "the fall of man." For other discussions of Camelot, see Beals (1969) and Horowits *et al.* (1967).

different aspects of their life and eventually publish something about them in the public domain. Yet, I can imagine a reviewer who would accept the first statement and reject the second.

Let me give an example from New York. In the beginning I would say, as I was introduced on a street corner, that I worked for the Narcotics Addiction Control Commission. Often, people would discover that they had other things to do, and I would be left talking to a mailbox. Then I began saying, "I'm a social scientist, and I'm interested in the life in the streets." Then if I was asked who I worked for, I would say, "The state pays my salary, but I work on my own research projects." Then if I was asked who specifically paid my salary, I would say, "NACC, but I don't report to anybody on what specific people are doing."

The second method of presenting myself worked much better. Was it unethical? I do not think so. It was a way of dealing with expectations that I learned street junkies had. People from NACC were on the street to spot-check treatment clients or to attempt to locate clients who had disappeared from the program, possibly to return them to the criminal justice system. By just saying, "NACC," I allowed those assumptions to operate. By using my second approach, I countered the assumptions before they could come up.

Let me give you another example that raises more problems. A few years ago, a sociologist named Laud Humphreys published a book called *Tearoom Trade*. "Tearooms," homosexual slang for public restrooms where casual sexual encounters take place, were of interest to Humphreys. He had been counseling male homosexuals but, after reading the then available literature, he did not feel that he understood much about homosexual life. (Perhaps his problems with that literature were similar to mine with the drug literature, as discussed in Chapter 2.)

He then did an ethnographic study of a public park restroom known as a tearoom. However, he did not present himself as an ethnographer. Instead, he took the role of "watch queen," a person who does not actively engage in homosexual acts, but prefers just to observe those encounters. In return, the watch queen is expected to keep an eye out for police in particular or straight males in general.

Humphreys also wrote down the license numbers of the cars of males who stopped at the tearoom for casual sex. Using these numbers, he could then obtain their names and addresses, and ensure that data were collected from them during a large-scale sociological survey that was going on in the city at the same time. Using the survey, he could learn something about their lives outside the tearoom.

His study shatters the stereotype of the male homosexual. Though some of the tearoom homosexuals were "out of the closet," many others were not. In fact, many had all the trappings of straight male life-styles—families, suburban

homes, and so on. Their major homosexual activity seemed to be limited to occasional stops at the tearoom. The book is an important piece of urban American ethnography. It is important, not particularly because of theoretical or methodological innovation, but because it humanizes a stereotype—something a good ethnography should do. However, the work generated a number of exchanges on the ethical issues.[16]

Should Humphreys have disguised himself as a watch queen? His answer would no doubt be that he could not have done the study any other way. The tearoom was nothing if not impersonal, and encounters were brief. Furthermore, the men were understandably nervous about the possibility of discovery and arrest. It was not the sort of situation that an ethnographer could introduce himself into and then work slowly to develop rapport with group members. If you wanted to observe the comings and goings in the tearoom, you had to be part of the situation. There was no time for a stranger to define a place for himself.

Humphreys identified the tearoom visitors without their knowledge and then maintained a list that identified them explicitly. Had it left Humphreys' control, a number of people might have had their lives devastated. Fortunately, it did not. Humphreys was conscious of the importance of the list, and he took great pains to keep it confidential. No one but him ever knew its contents. Anyway, he would argue, there was no other way to get background information on the tearoom men. If you tried to interview them at the tearoom, the possibility of being labeled homosexual would have caused them to refuse and leave.

So there you have it. Put it on the scale and decide what you think. By today's standards, Humphreys' study was clearly unethical. Yet the study was also clearly important—for counselors, for police officers, for policymakers, and, not least of all, for homosexuals. I wonder how a committee would have evaluated the risk to subjects versus benefits of the study? Most likely with too much weight on the risk side.

This discussion is meant to show that there are different ways of presenting yourself, but they probably do not fall into two discrete categories labeled "ethical" and "unethical." There are alternative ways of describing your role, and some are better suited to the situation than others. All this leads to a more general problem. Unfortunately, the guidelines for the protection of human subjects were not designed with ethnography in mind.[17]

[16] For an exhange of views on Humphreys' study, see the essays by Hoffman, Horowitz, and Rainwater in Hoffman *et al.* (1970).

[17] This problem will be discussed in more detail later. Wax (1977) has written a good critique of the guidelines for the protection of human subjects, noting that they are based on a model of biomedical and experimental psychological research that fits poorly into ethnographic research.

This will produce more problems for us later, but for now, notice that the hypothesis-testing social science research that underlies the guidelines is of a particular kind. The data-collecting situation is limited and neatly bounded. It involves something like an interview or a test, given by someone who is a specialist in that role. Ethnography is not so nicely packaged. People drift in and out of situations. The ethnographer is not always collecting data in interviews. Does one need to identify oneself to have a casual conversation with a stranger about the weather? If he is having a conversation with three informants about kinship and a stranger who is related to one of them enters the group, must everything stop until the ethnographic role has been described to the newcomer? If one is drinking in an urban bar and notices something interesting about sociolinguistic variation, must she announce that she is now doing ethnography by attending more carefully to intonation contours?

Those are silly questions, generated by a set of guidelines that do not take ethnography into account. Yet you must deal with them if you apply for a grant or go through a local committee for the protection of human subjects. Dealing with the bureaucracy will be covered when proposals are discussed in Chapter 8. But the bureaucratic procedures shouldn't obscure what is important—the people's right to know who you are. The problem is that whatever your explanation, you will be such an improbable kind of creature that people will have trouble believing you.

When I first began work in the *tanda,* I went through a carefully rehearsed speech about my interest in their customs and language, and how I was doing this as a university student from the U.S. Much later I learned that there were two somewhat similar explanations for my presence. First, I was a spy from Pakistan. Second, I was an agent of the government, there either to check their landholdings or to list all men capable of military service. Hardly a set of role-relationships within which rapport would flourish.[18] As you might imagine, similar kinds of things occurred when I began work in New York. On my first day in the field I was walking down a busy side street. Three young adolescents were sitting on a "stoop" drinking beer from a quart bottle. Their conversation silenced as I approached. One of them looked up as I passed and smiling, said, "Good afternoon, officer."

[18] Berreman (1962), working in North India, reports that a similar set of categories were applied to him initially. Freilich (1970a,b) notes that the "spy" role is frequently applied to ethnographers initially. Less frequently, ethnographers report a more benign initial categorization. For example, Powdermaker (1966), beginning her work in a small southern U.S. town, reports that because of her interest in blacks, a rumor began amongst them that she was really a Negro who was "passing." As you'll see in the text, sometimes I had a similar experience with junkies in New York.

In my experience, in the confusion that your presence initially creates, people restructure it with assumptions of your malevolent intent. I think this occurs because I am a complete stranger requesting the status of an intimate, an insider. What reasonable person would not be suspicious of someone like that? The ethnographer is asking for trust without yet having earned it. Little wonder that initial contact by the ethnographer is so often viewed with suspicion by group members.

The reaction will also depend on the sophistication of the group with whom you are working. Not long ago I did a short pilot study on an alternative-lifestyle rural commune. I was explaining my role to one of the residents, and he quickly interrupted with a question, "Who are you going to use for a control group?" He was a Ph.D. in biochemistry, with a sophisticated background in experimental research design.

Even if the group is not sophisticated in the literature of social scientists, they may have a history of dealing with them. In *Custer Died for Your Sins,* Vine DeLoria defined an Indian family as parents, children, and an anthropologist. A colleague tells a story that a graduate student was doing some linguistic fieldwork with an old Indian in the Southwest. The man was very helpful, finally saying, "You know, you're quite good. I used to work with Edward Sapir." Sapir, of course, is one of the founders of linguistic anthropology.

Or consider the case reported by Kendall Blanchard. He found a Navajo informant who had worked with anthropologists since the early 1930s. His associations became so permanent that he apparently added a room to his house for anthropologists to stay in when they came to do research. Blanchard discusses the impact of this long association in economic and psychosocial terms. For example, apparently the man made some economic gains from his associations, but, on the other hand, he followed the anthropological bias to preserve the traditional to the point that he is now considered one of the most conservative residents in the area.[19]

At Lexington, some of the older junkies who came into the hospital used to "game" on the clinicians. As the clinician was asking questions and giving advice, they would interrupt and critique their performance, sometimes pointing out things they did well, sometimes suggesting alternative strategies. The older junkies had been talking to clinicians in hospitals and prisons for years. The Lexington staff, for the most part, was fresh out of school fulfilling their military obligations. The role-reversal flustered the clinicians and amused the other patients.

Sophistication and background experiences with social scientists can make

[19] See Blanchard (1977).

a difference in how you are defined. The ethnographer can also make a difference, if she is not completely new to the group. If she can behave in a way that indicates prior insider status elsewhere, it will change people's interpretation of her role. In New York I met people initially in a community-based treatment setting and had the opportunity to talk with them for a while. I had worked in Lexington and knew something about how to talk from a junkie point of view.

I tried to tell everyone who I was but, as mentioned a bit earlier, in the flow of situations that ethnographers jump into, the message does not always get across to everyone. After I left, I later learned, two stories went around about me. One was that I was an old-time junkie who had gone to graduate school after cleaning up. The other was that I was a representative from Synanon. Both were attempts to label me—I knew more than a straight person should, but was obviously out of date. It was at least an improvement over being called a narc.

I guess I'm something of a fatalist about controlling the perception of your role. Some ethnographers talk of the importance of the early stages of forming your role, or write of the importance of the first days of fieldwork in conditioning the rest of your research. And there are some reports around of disasters that suggest that such advice should not be ignored. But then you read of the strain of maintaining what you think is the correct impression, which may be all wrong anyway, and you recall the earlier discussion of how there are no set rules for entree, and how at any rate everyone makes mistakes early in fieldwork.[20]

My own feeling is to agree with people like William Whyte and Rosalie Wax who conclude that people judged them on how they conducted themselves on a day to day basis rather than on any official explanations they offered. And I agree with Delmos Jones, who argues that there should be more emphasis on directness and honesty, more of a sense that one can be oneself. He gives a couple of examples from his fieldwork to show how changing from role-playing an ethnographer to more of a direct form of behavior was not a liability. It actually improved his fieldwork.[21]

Eventually, people come to accept you for what you are—a strange person who asks many dumb questions. Starting from scratch, it seems to take me about three months until some quantum leap occurs, and I am a functioning, accepted member of the community. While you are becoming adjusted,

[20] For some sample discussions of the importance of early role definition, see Geer (1964), Olesen and Whittaker (1967), Williams (1967), and Kluckhohn (1940). Though many mention the strain of behaving properly in fieldwork, Berreman (1962), Paul (1953), and Delmos Jones (1973) are good examples of such discussions.

[21] See Whyte (1955) and Wax (1977). The Jones article is cited in Note 20.

people watch you and find out (hopefully) that nothing harmful happens as a result of your presence. They notice that you do indeed ask questions about language and customs, write things down in notebooks, and tape record interviews.

When you are accepted, sometimes you are told what to do. After a few months in India, I was sitting in my hut reading a book by lantern, relaxing to the background noises of evening in the *tanda.* Suddenly the door opened, and taking great liberties with the translation, I heard, "Where's your notebook? We're having an important ceremony out here. What's the matter, you're not working tonight?"[22]

Similar things happened at Lexington. Once a patient came into my office and closed the door. "Listen," he said, "we just copped some blues ("numorphan") and we're going to get off. You want to come and watch?" Yes I did, but I declined. It would have put me in too much of a bind. That is another kind of ethical dilemma that we will get to later on.

Whatever you do, though, you are probably going to be regarded with some caution initially. You may be able to reduce the distance more quickly if you have worked with similar groups and can demonstrate that you have enjoyed insider status previously. But however you present yourself and are perceived, you should probably hang question marks on many of the things you learn in the early part of your fieldwork.

During my first month in the *tanda,* I took a census, thinking that was a fairly standard, harmless task. Later, I found out that much of the information was wrong. People laughed about it and that is when I learned that I had been seen as a Pakistani spy and a government agent. I found out that people tended not to tell me about young males living in the household, in case I was going to take them for the army. People also said they had less land than they did, in case I was from the accountant's office.[23] Ever since I have become an ethnographer, I have never been able to keep a straight face when people say things like "a 2-week fact-finding tour," though I should admit I've done a couple.

So, the goal is to begin your work honestly by presenting yourself and your task in some way that will make sense to group members. Then some group interpretations of your role will occur that may present problems in the early stages of your ethnographic work. Try to find out later how group members initially saw you. As time goes on, you will be accepted, at least by some of the group, and you will feel an exhiliration as people decide you really are

[22] Powdermaker (1966) also mentions that her presence with a notebook made an event "official." There are other similar accounts in the literature as well.

[23] For a similar account of problems with census taking, see Kobben (1967).

interested in learning how they think and what they do. Now it is only the baggage in your mind that is preventing you from understanding. That takes us back to the first part of the chapter. Having come full circle, it is time to discuss more formally what ethnography is all about. What are its goals and how does it differ from other kinds of research traditions?