

2

The Concept of Fieldwork

Somewhere underneath the prose of social science lies some human contact. Before the clatter of the typewriter begins, some person (a social science researcher) collaborates with another person (variously called *subject*, *respondent*, or *informant*) to create a social relationship within which an exchange of information occurs. This book is about those relationships. More specifically, it is about those relationships—fortuitously encountered or consciously constructed—that constitute the core of ethnographic fieldwork.

Ethnography is an ambiguous term, representing both a process and a product. As a product, an ethnography is usually a book. The book almost always focuses on some social group, though it may be guided by any number of theories and methods. Although the book usually has some central point, the discussion of the social group often covers a lot of territory. There will be a dash of history, something about the various environments—physical, biological, and social—and some detail on the things the group does and the beliefs they hold.

Ethnographies differ, of course, and they are not just books. In fact, a teacher of mine once described a vision of the future where ethnographies would not be distributed. With the growth of narrative and visual documentation, he reasoned, and the increase in specimens of things like material culture and plant samples, there would simply be too much information to transmit. Instead, the material would be gathered in one place, and people interested in the ethnography of such-and-such a group would travel there to study it.

At any rate, my concern here is with ethnography as a process. How is it that an ethnographer attempts a comprehensive understanding of some human group? The name for “doing ethnography” is *fieldwork*. There is a tradition in cultural anthropology that one cannot be told how to do fieldwork.¹ A bit of graduate student folklore that I learned at Berkeley goes like this: A graduate student, at the end of her first year, was given a few hundred dollars by the department and told to go and study an Indian group during the summer. Not only had no one told her how to do ethnography, neither had anyone bothered to describe the location of the tribe.

With trembling heart and sweaty palms, she approached the door of Kroeber² himself for some advice. After several passes by the open door, she entered and nervously cleared her throat. Kroeber was typing (naturally) and did not look up for a minute or so. When he did, the student explained her dilemma and asked for advice. “Well,” said Kroeber, returning to his typing, “I suggest you buy a notebook and a pencil.”

It does not matter whether the story is true or false. As folklore always does, it tells us something important about the group that generated it. Fieldwork was not something you could train people for. You just had to do it, and then exchange knowing glances with others who had been initiated. There was a mystique about fieldwork, the emotional aura of a rite of passage into professional adulthood.

In some ways I am sympathetic to this view, and I will write about it more later. I have met few ethnographers who were not personally affected in some profound way by their fieldwork. Besides, there are some areas in which experience is the best teacher—not just classroom assignments, but the whole experience.³ But, it is madness to conclude from this that field training

¹There are numerous references to a lack of fieldwork training in the literature. For example, see the opening comments in Berreman (1962) and Wax (1971). As another example, Adams and Preiss (1960), in their introduction to a collection of methods articles, note that roughly two-thirds of the articles are by sociologists. They then speculate on some reasons why anthropologists are not more methodologically reflective. Freilich (1970a), in his introduction to a collection of personal accounts by anthropologists, discusses the development of a “culture of fieldwork” to account for the lack of a methodological tradition in anthropology. This neglect is made more confusing when you consider that field methods discussions are scattered throughout the literature. There are some classic early positions, such as Boas (1920), Radin (1933), Mead (1933), and Malinowski (1961, originally published in the 1920s). In their histories of fieldwork, Wax (1971) and Pelto and Pelto (1973) show how the tradition of ethnography goes back even further. I can only agree with Gutkind and Sankoff (1967), who, in the introduction to their annotated bibliography on field methods, note the abundance of fieldwork literature and the lack of attempts to systematically discuss it.

²A. L. Kroeber is one of the founding fathers of American anthropology.

³There is a good manual by Crane and Angrosino (1974) that leads students step-by-step through some field projects to train them in important ethnographic tasks.

must be limited to a few concepts and a couple of unstructured ways of recording information.

I do not think many ethnographers would make this argument as a matter of intent, but the argument does represent the consequence of the tradition, or lack of a tradition, of fieldwork training. Though I have not done the study, I bet if you plotted a graph of the proportion of anthropology departments offering a “field methods” course over time, there would in fact be an upward turn in the curve, although quite recently.

Even where such courses are offered, they sometimes deal mainly with research designs of the classic hypothesis-testing sort—statistics, or perhaps survey research or experimental design. There is nothing wrong with learning these things; in fact, they are essential. But they are not the same as ethnography. Good ethnographers draw on such knowledge, but they draw on it as one or two items within a process that includes numerous other activities as well. Many of these “other activities” are part of the mystique of fieldwork. In this book, I hope to contribute to the demystification of at least some of them.

Reasons for Becoming an Ethnographer

People may be attracted to cultural anthropology for a variety of reasons. They might be intellectually stimulated by ecological systems, or they might like the esthetic of structural analysis, or they might enjoy the relevance of alternative technologies. Then again, they might be personally curious about the variety of lifeways practiced on the planet in general. But these same people may or may not like doing ethnography. In fact, for some, the process of acquiring the information that is the bedrock of anthropological theory is downright painful.

So what kind of people enjoy doing ethnography? Probably many different kinds. Around a dinner table not too long ago I sat with a group of ethnographers who were hosted by a clinical psychologist. He asked that question, and since it was his treat, we explored childhood and adolescence as possible sources of our initial interest. From our sample of four, we came up with two theories of origin.

The first theory held that ethnographers grew up in communities where they felt no personal involvement. While they, of necessity, participated in the flow of social life, they felt detached from activities to which others attached high emotional significance. This corresponds to another item of professional folklore; namely, that anthropologists are people who are alienated from their own culture. As a result, the story goes, they do not share

any attachments that prevent an appreciation of other cultures. In fact, they may seek the experience of other cultures as a quest for an identity that makes sense to them. It may not be an accident that during a period of recent American history that saw an increasing alienation among students there were also dramatic increases in enrollments in anthropology courses. In the late 1970s, this trend was reversing, to the chagrin of university faculty, for whom enrollments are the currency in the academic Kula ring.⁴

The second theory held that ethnographers are products of multicultural environments. They grow up in, and become accustomed to, cultural diversity. Rather than feeling threatened by the different lifeways they encountered, they became fascinated by the differences. As an ethnographer, they find a professional role to justify their move from one way of life to another. Once again, another piece of professional folklore becomes relevant. I have at times heard a belief that anthropologists' children copy their parent's profession more often than is normal in the U.S. population. I have never seen any data on this, but I do know three anthropologists, all excellent ethnographers and all siblings, who are the children of an anthropologist who also relishes fieldwork. When you talk with them, they do say that the experience of "growing up in the field" addicted them to cultural diversity. Short of being independently wealthy, what better role for a "culture-hopper" than a professional ethnographer?

So, some people like ethnography because it justifies their detachment from what others consider important. Others like ethnography because it justifies an interest in exploring different lifeways. I am sure there are many other possible explanations as well, and, like all explanations, the two that fell out of the dinner party were constructed for a host and an occasion. But it raises a question that, to my knowledge, has not been treated much in the literature on ethnography.⁵ You, dear reader, might wonder about yourself,

⁴The Kula ring is described by Malinowski (1961). Briefly, it is a series of trading relationships among Pacific Islanders where certain goods flow in one direction while other goods flow in the opposite direction. In academic situations, enrollment figures go to the administration and departmental resources come back.

⁵The issue of the ethnographer's personality will be discussed in more detail in Chapter 4. Wintrob (1969), comparing a group of seven graduate students with eight anthropologists analyzed by Roe (1952a,b), notes some similarities, such as being firstborn, rebelling against parental authority, and struggling for a highly valued independence. The problem is that there isn't much research, and what there is is based on a few cases. Besides, it's hard to know if the results represent things that preceded, followed, or developed in conjunction with the person's role as an ethnographer. And then we don't know about people with the same characteristics who didn't become ethnographers.

as a person who is interested in doing ethnography. Are we all sociopaths and gadflies? If not, then what, in fact, are we doing here?⁶

Mystique as a Communication Problem

If you read through discussions of field methods, you will find a variety of reasons offered for the mystique of fieldwork—the "culture" of fieldwork, professional face-saving, personal anxieties, a desire to look "scientific," and so on.⁷ Although I can recognize all of these in my own experience, there is another reason that my own biases lead me to consider important—biases resulting from my interest in the study of human communication.

Imagine a trip to another place for a limited period of time. If you are an adventurous type, you move out into the local scene to learn something about it. You are overwhelmed with new sights and sounds. You find yourself acting and thinking in ways different from your ordinary life. Just to liven things up, let's say you also fall in love and spend more time than normal in various states of intoxication. At the end of your 2 weeks, you climb on the plane, your mind filled with images of partly understood, partly connected experiences. As you walk into your home airport terminal, a friend who has come to meet you smiles and says, "So how was the trip? What'd you do?"

Good luck with the answer. Most people confronted with this communication dilemma, resort to the language of intensive affect ("fantastic," "far out," "unbelievable") or go to specific descriptions that will make sense to the questioner ("Well, on Monday morning I . . ."). Only later, with much time and a lot of linguistic work, you might try to give your friend a better

⁶During 1995, the interest in ethnography boomed, and ethnographers now work in a variety of academic disciplines and organizational settings. In light of this growth, the tongue in cheek theory of why people become ethnographers obviously needs more careful thought. For one thing, America's love affair with positivism cools as more and more people realize the difference between aggregate statistical data and what's actually going on in the world. For another, stable homogeneous populations that positivist methods presuppose are increasingly a thing of the past. Also, organizations tend to turn toward ethnography when prior frameworks break down—and organizational breakdowns are now the norm rather than the exception. And finally, as global ideologies shift from hierarchical to participatory structures, ethnography offers the analogous shift in the research arena. For these and many other reasons, the question of why one might become interested in ethnography deserves a theory of its own, something much more thorough than the "dinner table" story in the original *Stranger*.

⁷Freilich's (1970) introduction discusses some of the face-saving motivations. Wintrob (1969) covers some of the personal anxieties arising from doing fieldwork. Examples of critiques of an overconcern with being "scientific" can be found in Honigmann (1976), Redfield (1948), and Weakland (1951). Devereux (1967) shows the relationship between the psychodynamics of the researcher and the kind of "science" that he does.

description—one that approximates more closely the richness, intensity, and variety of experiences that you had.

People in a branch of sociology known as *ethnomethodology* call this a problem in *indexicality*. *Indexicality* refers to the amount of shared background knowledge necessary to understand a message. If you and your friend go to a party together, then you have shared the experience. When you talk later, you can say “Some party, huh?” and your friend will draw on the same experiences to interpret the message. On the other hand, if your friend wasn’t there, then you have to describe several details before he can understand the point of saying “some party”: “It was a good party because of some people I met”; “It was a good party because everyone got crazy and walked around in lampshades”; “It was a bad party because people only talked about boring things”; “It was a bad party because the host died before it really got going.”

So when you got off the plane from your trip, you brought a resource of experiences that your friend had not shared. To describe the trip, you must describe those complicated experiences, your reactions to them, and do so in a way that makes sense in terms of the experiences your friend has had—a formidable task. Now consider the even more complicated situation of an ethnographer. I haven’t really talked yet about what ethnography is. That will come in detail later. But for now, it always involves long-term association with some group, to some extent in their own territory, with the purpose of learning from them their ways of doing things and viewing reality.

In part, ethnography resembles the common image of social science—questionnaires, tests, censuses, and so forth. But the ethnographer also eats with the group, works with them, relaxes with them, and hopefully comes to understand them. Meanwhile, he or she personally struggles with the interference from his or her own ways of thinking, feeling, and acting. Ethnography, whatever else it is, is an experientially rich social science. Compare it to many other forms of social science data collection—like a study based on questionnaires—and you get something like a comparison between Fort Knox and a quarter.

It is just this experiential wealth that presents a problem in methodology. When you attempt to describe some aspect of a group’s life, you may be drawing from conversations, casual observations, 20 formal interviews, a previous ethnography, two novels, your general idea of the human condition, childhood experiences with your parents, and who knows what else. The problem in ethnographic method is to properly “de-indexicalize” descriptive statements. Throwing up your hands at the difficulty of the task strikes me as a perfectly sane response. Nevertheless, it’s a problem that ethnographers must take seriously.

Reasons for Worrying about Methods

If ethnographers have gotten along so well for so long without being overly concerned about methodology, why worry? Well, first of all, they have not gotten along all that well. Consider the problem of cross-cultural comparisons. At Yale, the Human Relations Area File (HRAF) was developed in the 1940s under the leadership of George Peter Murdock. This file is now available at many libraries around the country. Murdock and his associates attempted to take material from ethnographies and code them into content categories, like socialization practices, subsistence strategies, kinship systems, and so on. A codebook was published that indicates the many categories that were used to classify material.⁸

The idea, then, was that an anthropologist who wanted to compare socialization practices, for example, could look up the appropriate code numbers, go to the microfilmed file, and retrieve all the things that ethnographers have said about socialization. The problem is that ethnographers differed in their interest in socialization, to stay with one example. Sometimes they all but ignored it. If they did discuss it, the discussions differed in the amount of detail. Even if they were similar in detail, they differed in their bias. One ethnographer might discuss socialization from a Freudian point of view. Another might discuss it in terms of learning theory. A third might focus only on the mother–child relationship. Another might emphasize mother and father but ignore sibling relationships. Yet a fifth might only examine birth to 5 years of age, while still another might discuss the socialization process through adolescence.⁹

How can one compare such widely different material? The problem gets worse when you examine the disagreements among ethnographers who work in the same area. Most famous among these disagreements is that of Oscar Lewis and Robert Redfield. Redfield had done a study of a Mexican village, as well as several other areas in that country. Oscar Lewis came along later and decided to do a restudy of the same village.

First of all, Lewis’s behavior was a tremendous breach of etiquette. An ethnographer had “his village,” and it was sacred territory. It was unheard of for another anthropologist to go in and do a restudy. Perhaps this unwritten rule of conduct partly arose as a form of methodological defensiveness.

⁸The codebook is listed in the References as Murdock *et al.* (1950).

⁹Many interested in cross-cultural research have developed ways to measure the quality of data in the HRAF. For example, see Naroll (1970) and other relevant articles in Naroll and Cohen (1970). The relationship between ethnography and cross-cultural research is also taken up in more detail in Chapter 9 of this book.

Such an attitude is certainly contrary to the spirit of scientific inquiry, where replications are routinely done to check the reported results of an individual researcher.

At any rate, Lewis came out with a description of village life much more negative than Redfield's. To oversimplify, Redfield had portrayed village life as harmonious and integrated, especially when contrasted with life in the city. Lewis, on the other hand, described the same villagers as displaying such unharmonious emotions as hostility, jealousy, and greed. In sketchy form, this is the famous Lewis-Redfield debate. The debate is something of a landmark event in the history of American cultural anthropology. Two competent, trained ethnographers produced dramatically different descriptions of the same group of people.

There are other cases of ethnographic disagreement as well. Margaret Mead and Reo Fortune disagreed over the nature of the male role among the Arapesh of New Guinea. An article written in 1946 describes a disagreement over the nature of Pueblo culture. On the one hand, Pueblo culture was described as harmonious and integrated; on the other, as full of covert tension. Then there's a Chinese anthropologist who studied the Zuni, and wrote in an article in 1937 that he was rather embarrassed at all the discrepancies between his experience and the reports of previous work among the Zuni. It's interesting that a common theme in many of these disagreements, including Lewis and Redfield's, is related to a differing emphasis on conflict or harmony.¹⁰

Still another example of ethnographer disagreement is the debate between Ward Goodenough and John Fisher. This one is even more striking because it involves the application of what appeared to be straightforward categories for coding postmarital residence patterns. Ethnographers have long discussed residence using such categories as "matrilocal" (residing with bride's family), "patrilocal" (residing with groom's family), "neolocal" (establishing a new residence), and so on. Yet when Goodenough and Fisher studied the same group on a Pacific atoll, they came up with different percentages of each residence type. Goodenough wrote a paper analyzing the problem.¹¹ He

¹⁰The review of restudies comes from Naroll (1970). A discussion of the disagreement over Pueblo culture is in Bennett (1946). The Chinese anthropologist's article is cited as Li-An-Che (1937). Some examples of general discussions on the importance of restudies as a check on method can be found in Lewis (1953) and Herskovits (1954). On the other hand, Den Hollander (1967) argues that when restudies are done by the same person, or are based on the same original work, there is more agreement. Honigmann (1970) argues that although there have been differences, by and large ethnographers in the same area have reached similar conclusions. Den Hollander (1967) and Honigmann (1976), among others, argue that with the importance of the personal role of the ethnographer, it would be foolish to expect perfect replication at any rate.

¹¹This case is described in Goodenough (1956).

pointed out that in several cases, the residential situation was complicated and subtle, involving several kinds of relatives. One person might say they were "residing together," whereas other persons might say they were not. Because of the complexity, each ethnographer had to fill in some implicit assumptions in order to categorize the cases according to residence type. Since the two ethnographers differed in implicit assumptions, they came up with different percentages of each type. I think *implicit* is a key word here, and it no doubt bears some relationship to the mystique discussed earlier.

All sorts of things happen during fieldwork. Probably some of them will always remain implicit and incapable of articulation. But certainly not all of them need remain so. In attempting to make explicit the ethnographic process, we are contributing to the development of ethnographic method. To the extent we can do so, we might begin to understand how two professionals can be in such disagreement. Perhaps disagreement should be expected, given the complexity and personal involvement in ethnographic work. We'll return to this topic in Chapter 4.

Of course, there is a law that operates here that should be named after somebody. The more we insist that the experiential base of ethnography be made explicit, the fewer the experiences that can be dealt with adequately. Explicitness, in other words, carries with it a danger of trivialization. The strategy is to *selectively* narrow the focus within a previously explored broad field. A couple of sections down the road, you will be introduced to this as the "funnel" approach.

To sum all of this up, I am arguing that ethnographers have *not* gotten along very well by neglecting methodology. In the history of the field, problems in cross-cultural comparison, and the differences between ethnographers studying the same group, both point the damning finger at our failure to be explicit about what it is we do when we do fieldwork. As if these were not problems enough, there is another reason to worry about ethnographic methodology as well. That reason, to put it bluntly, is competition from other disciplines.

In a recent book on cognitive psychology, the author calls for more attention to "ecological validity"—the operation of cognitive processes in natural context. Another recent work offers a new perspective in social psychology, one that is essentially ethnographic. Although some sociologists have always been active in ethnography, it seems to me that the pace has picked up considerably in the last few years.¹² In many cases, these ethnographically oriented authors from other disciplines do a better job articulating ethnography

¹²The cognitive psychology book is Neisser (1976), whereas the social psychology argument can be found in Harre and Secord (1972). A long list of sociological works are given in Note 15 of this chapter.

than we do. To give just one quick example, Glaser and Strauss came up with the elegant statement that in ethnographic research, data collection and analysis are done concurrently rather than being separately scheduled parts of the research.

In many sociological surveys, for example, a questionnaire is designed. Then interviewers go out and "collect the data." The data are then coded and keypunched. Only then does "analysis" begin, with the machine-readable data manipulated according to some statistical procedure. In ethnography, as we will discuss in more detail later, you learn something ("collect some data"), then you try to make sense out of it ("analysis"), then you go back to see if the interpretation makes sense in light of new experience ("collect more data"), then you refine your interpretation ("more analysis"), and so on. The process is dialectic, not linear. Such a simple statement is so important in capturing a key aspect of doing ethnography. An anthropologist should have said it long ago.

In addition to competition from nonanthropological ethnographers, another kind of rivalry is also important. Increasingly, ethnographers live in professional niches where they must compete for attention from granting agencies, publishing houses, and policy makers. Their competition consists of other social scientists who, for the most part, have a tradition of methodologies that are remarkable for their explicitness. These methodologies have their own problems, including their own brand of smuggled implicitness, but more on that later.

The point now is that these social scientists usually have little understanding of what ethnography is and how it is done. They usually find little to instruct them in the scarce methodological discussions in ethnographic research reports. If you are a policy maker and you have a choice between a survey that asked the wrong questions very explicitly, or an ethnography that asked the right questions but you cannot tell how they were asked and of whom, which one would you pick to base policy on? More and more ethnographers are concerned about the policy implications of their research. An increase in methodological sophistication will enhance policy credibility.

At this point, you may be warming up for the conclusion that ethnographers should just use the explicit methodologies already developed in other social sciences. My answer would be to applaud with one hand. Ethnographers need to draw on procedures from other fields as part of the process of fieldwork, but ethnographers who just do surveys or administer psychological tests are not doing ethnography.

A few years ago, I was part of a symposium called *Ethnography and Criminal Behavior*. During the discussion, a sociologist with ethnographic tendencies stood up and said, "The trouble with you anthropologists is that

as soon as you start working in cities you look like sociologists." His point was that when faced with the large numbers and unbounded groups characteristic of the urban setting, many ethnographers flock to the methodology of survey sociology. In doing so, they abandon the unique features of the ethnographic process, and consequently sacrifice some important contributions they might make to the study of urban life. Although he ignored several important counterexamples, I quote him as a cautionary note. You cannot build a wooden bookshelf out of metal, though you might strengthen it with some steel brackets.

How This Book Was Written

Before I can tell you about this book, I first need to say something about how it was written. During the summer of 1978, I fled Houston for Mexico to study Spanish and escape telephones and memos. I began to think about ethnography for two reasons. First of all, my days in the drug field had taught me the political value of ethnography in humanizing stereotypes. Policy—and the treatment, prevention, education, and law enforcement efforts that it logically implies—is built on assumptions of what "those people" are like. Yet a little ethnography quickly teaches you that the assumptions, at their very best, are oversimplifications. Ethnography, then, offers a social science metaphor within which the richness and variety of group life can be expressed as it is learned from direct involvement with the group itself. I also thought about ethnography because much of my career has been spent discussing it with nonethnographers. I learned that there were some fundamental differences in research worldviews, made more subtle by the fact that they were all included in the category of social sciences.

I began to make some notes, and the notes eventually turned into the first draft of this book. My purpose was to produce something that a person could read to get a perspective of what it's all about—or at least my version of it. When I returned to Houston, I went to the library to see if what I'd written fit into the literature on ethnographic field methods.

By academic rules, you're supposed to go to the library before you write, but I'm glad I didn't. I was able to get my own conclusions straight before I encountered the mass of material I found when I got home. In anthropology we believe that there is not much literature on field methods. That's not quite true. There's an unbelievable amount of literature, but it's scattered widely through time and across many journals. I know there's still more out there. How do you know when you've reviewed enough literature? In these days of the information explosion, it's when you're exhausted, confused, and can no longer see straight.

Reviewing the literature was a humbling experience. I doubt if there are many things in this book that haven't been said somewhere by somebody. Take the title, for example. As far as I know, I invented it. Then I went to the library and found a book called *Stranger and Friend*, an article entitled "The Ethnologist as Stranger," and occasional references to essays dealing with "The Stranger."¹³ I thought my discussion of ethnographic proposals would be unique, but then Pelto, Williams, and Freilich, for example, also discuss them, although not in the same way.¹⁴

On the other hand, the review was gratifying. It was pleasant to see that many of my conclusions were supported independently by the experience of others. It was fascinating to watch the issues grow. The opposition between observation and interview is there at the very beginning with the two founding fathers. Franz Boas stressed the interview as he tried to get descriptions of disappearing American Indian cultures from older informants; Branislav Malinowski pitched his tent among the natives, learning their language and sharing their lives.

I watched the exciting collaboration between sociologists and anthropologists growing out of the community study orientation at the University of Chicago. Then, in 1949, under the editorship of Conrad Arensberg, the journal of the Society for Applied Anthropology (now called *Human Organization*) began regularly featuring methodological articles, a practice that has continued to the present. Finally, book-length treatments of ethnographic methods have begun to appear more recently, together with a growing number of personal accounts of the fieldwork experience.

It was especially gratifying for me personally to notice that two of the better personal accounts, by Powdermaker and Wax, were written by ethnographers who worked in U.S. settings long before the recent interest in U.S. ethnography. It was also interesting to see the continuing commitment of *Human Organization* to methodological discussions. In traditional anthropological circles, and to some extent still today, studying the United States or applied work is considered less prestigious than "pure" research on real "natives." There are some reasons for these biases (discussed later), but I think that work in niches where other professionals wonder what you're up to, and where you yourself are part "native," stimulates methodological reflection.

In fact, from the literature review, I now recognize two more books that should be written. The first would synthesize the voluminous scattered literature as a kind of state-of-the-art review. The second would treat the numer-

¹³ The book is by Powdermaker (1966); the article, by Nash (1963). The two essays are by Simmel (1950) and Schutz (1964). Also, see Meintel (1973).

¹⁴ See Pelto (1970), Freilich (1970), and Williams (1967).

ous personal accounts as personal documents, and analyze them for their similarities and differences.¹⁵ That's not my purpose here, though. So what is my purpose here?

First of all, I hope to give a sense of ethnography as a special approach to understanding the human situation. I want to do this especially by contrasting it with hypothesis-testing approaches—not because I think such approaches are bad, but because to most people (including some anthropologists) hypothesis-testing is the core meaning of social science. If you believe that, it leads you to ask questions that miss the point of what ethnography is all about.

Second, I want to contribute to a synthesis of some bothersome oppositions. In the literature, you will run across differences between traditional versus problem-oriented ethnography; between a personalistic versus a standardized approach; between ethnography as a humanistic versus a scientific enterprise. My goal is to try for breadth, but also depth in a few select areas; and to deal not only with the humanity of both ethnographer and group members, but to try to do so in a way that documents the experiences in a manner convincing to a skeptical outsider. Later in the book, this is called a *funnel* approach, with breadth and humanity at the beginning of the funnel, and then, within the context of that beginning, depth, problem-focus, and science at the narrow end. In a way, it's a "Why can't we all be happy?" strategy. But it's critical, I think. Without science, we lose our credibility. Without humanity, we lose our ability to understand others.

The book, however, will not be a compendium of methods that have been used in ethnographic research. Ethnographers have invented numerous methods appropriate to their research settings. They have also begged, borrowed, and stolen methods from most of the other social, biological, and physical sciences, as well as from the humanities. The design or theft of specific methodologies is, as I hope to show later, *your* problem. Rather than attempt a sort of catalogue, I am going to try to focus on the process in general—things that ethnographers worry about whatever their special interests.

¹⁵ Freilich (1970) attempts to analyze the personal accounts presented in his book, suggesting a model of fieldwork based on them. Pelto and Pelto (1973) do a similar analysis of personal accounts plus some questionnaire responses from professional ethnographers. Golde (1970) also reviews the personal accounts contained in her book in an attempt to define problems unique to women fieldworkers. We need more analyses of this type. Two of the best personal accounts, in my opinion, are Powdermaker (1966) and Wax (1971). Examples of others can be found in Henry and Saberwal (1979), Levi-Strauss (1961), Maybury-Lewis (1965), Middleton (1970), Romanucci-Ross (1976), Spindler (1970), Vidich *et al.* (1964), Whyte (1955), Beattie (1965), Casagrande (1960), Dumont (1978), Geer (1964), Golde (1970), Beteille and Madan (1975), and of course the now famous diary of Malinowski (1967). Other personal accounts can often be found as introductions or postscripts to published ethnographers.

Finally, I hope to provide a context within which additional research training will make sense.¹⁶ There is little in the way of statistics here, but hopefully at the end you will have an idea of where statistics might be useful. Mathematical models are only hinted at, although you should see their potential and seek further training in that area. Research design in areas like survey research or psychological experimentation are occasionally mentioned, but it should become clear why you should master them. Likewise, I hope at the end that you will also explore methods from the humanities—techniques drawn from literary criticism, discourse analysis, hermeneutics, and so on. When you set out to do ethnography, the broader your research repertoire, the more creative you can be in developing research approaches to questions you didn't anticipate in a group you're just learning to understand.

A Research Biography

Before we start, I want to offer something of a research biography. Some of my examples will come from "real life," though I am not sure when a person is an ethnographer and when he or she is not an ethnographer. Some other examples will come from short-term consulting work I have done; these can be adequately described as the occasion arises. But a few experiences will be

¹⁶For an introduction to statistics written from an anthropological point of view, see Hurst (1976). Kay's (1971) edited volume illustrates several applications of mathematics in anthropology. A good introductory book on the topic of mathematical models is Lave and March (1975). Johnson (1978) introduces the topic of quantification from a point of view that considers traditional ethnographic concerns. Pelto's (1970) book discusses anthropological research within a general discussion of science. He then goes on to introduce statistics and several measurement techniques from sociology and psychology, as well as to discuss some traditional ethnographic field methods. Brim and Spain (1974) offer a blend of traditional ethnography and control-group research design adapted from Campbell and Stanley's (1966) work. Although not an exhaustive list by any means, these works represent views of ethnography with the goal of furthering standardization and quantification. On the more qualitative side, in my opinion, the sociologists seem to be more active, as evidenced in the philosophical statements of ethnography by Glaser and Strauss (1967) and Bruyn (1966). Other book-length treatments by sociologists include Douglas (1976), Becker (1970), Cicourel (1964), Denzin (1970), Johnson (1975), Schatzman and Strauss (1973), Speier (1973), Junker (1960), and Bogdan and Taylor (1975). Though there are volumes reflecting a primary concern with qualitative analysis by anthropologists [see Edgerton and Langness (1974) and Williams (1967), for example], it strikes me that much recent anthropological writing is concerned with a move towards quantification, whereas many in sociology, coming from a tradition that emphasizes quantification, are concerned with formalizing qualitative analysis.

drawn on so frequently that it is worth giving a background sketch now. Besides, I want to illustrate the different roles an ethnographer can play.

Four of these examples are actual examples of fieldwork. One is from a village in South India; the next from a federal institution for the treatment of narcotics addiction; the third from a village in southern Austria; and finally, the fourth is from treatment centers and the streets of New York City. The fifth experience comes from a position in the "research establishment." For 4 years, I reviewed research grant applications for an agency in what was then called the Department of Health, Education and Welfare. The experiences are presented in chronological order.

South India

In my sophomore year at college, I walked into an introductory anthropology course and decided to stay. In the course, I learned that ethnography was at the heart of it all, so I asked around and learned that Professor Alan Beals was going to India the next year. For reasons that I will never understand, I asked if I could go along, and he said, "Well, maybe." We worked out an arrangement where I would help supervise a team of genealogical interviewers part of the time. The rest of the time I would work in a small community nearby, with Beals supervising and teaching me what ethnography was all about.

Beals was returning to Gopalpur, a village he had described in an ethnography written from his earlier visit. "My" village was located about 2 miles away. It was populated by the Lambardi, a group of North Indian derivation now residing in the state of Karnataka. Previously, they had been a migrant group who served as the transportation system for goods in India. They would purchase goods at the point of origin, then migrate some distance to sell them at a profit. Partly for this reason, they are called the "gypsies" of India. In fact, they probably are distant relatives of European gypsies.

The settlement, or *tanda*, was located on government-owned wasteland. The Lambardi were poor by local standards. They had no irrigation and few owned much land or many animals. They had their own language from the North, but my work with them was done in Kannada, the language spoken throughout Karnataka. I rented a small hut there and split my time between Gopalpur and the *tanda*.

By the end of the 6 months that I worked there, I was on speaking terms (at least) with most of the *tanda*'s 200 inhabitants. With a few, I had very close and warm relationships. My work there was of the classic ethnographic type, where I tried to cover the entire territory, from crops to kinship to karma. Though this research occurred a while ago, in 1965–1966, it

represents an important source for some of my notions of what ethnography is all about.

"Narco"

The recent history of illicit narcotics use in this country is a stormy one. With the passing of the Harrison Narcotics Act, the subsequent Supreme Court interpretations in the 1920s, and the closing of the morphine clinics at about the same time, narcotics addicts were converted into criminals. In the 1930s the federal government constructed a "hospital" for addicts near Lexington, Kentucky.

The hospital, known as "Narco" locally, served federal prisoners convicted for narcotics-related crimes, as well as volunteer patients who checked in for "the cure." By the time I arrived, in 1968, the name had been changed to the National Institute of Mental Health (NIMH) Clinical Research Center and the "patients" were civil commitments under the provisions of the Narcotics Addict Rehabilitation Act.

As part of its development as a research center, the hospital had opened a Social Science Research section. The director of the section, John O'Donnell, wanted an anthropologist brought on to see what one would do. He turned to the applications for commissioned officer status in the U.S. Public Health Service just as mine crossed a Washington desk. When he contacted me and offered me a commission, I was delighted. It sounded like an unbelievably pleasant alternative to the choices then available to me and my male friends—the army, jail, or Canada.

The hospital, which also had many of the trappings of a prison, was a confusing place for a traditional ethnographer. Here was a group that was both part of and distinct from the rest of American society. They were localized, but only temporarily, and not by choice in most cases. They were all "dope fiends"¹⁷ who differentiated themselves by age, ethnicity, residence, and so on. It was hardly the sort of group ethnographers deal with.

There were some interesting problems right away. For instance, the dope fiends had a name for people about my age who came around asking about life in the streets—"narc." Then I quickly learned that certain cherished pieces of ethnographic jargon, like the term *informant*, had to go. But in spite of such problems, as I compared my work at the hospital/prison with my work in South India, there were more similarities than differences. Even with such a different group, much of what I had learned about doing ethnography in South India worked in Lexington as well.

¹⁷In case anyone finds them offensive, terms like "junkie" and "dope fiend" are those used by street addicts themselves, aggressively so when faced with a straight person with a preference for medical, legal, or other euphemisms.

Lexington was important in another way. For the first time, I had left the family. I was the lone ethnographer among sociologists and psychologists. They often asked delicately phrased questions like, "Just what in the hell is it that you do anyway?" Or, more specifically, "Where is your instrument? What is the sample design? What is your plan of analysis?" No one I knew had worried about such questions, and it was embarrassing to know that somehow those were the wrong questions, but not be capable of explaining why.

Another first-time experience at Lexington was my entry into the world of government bureaucrats and policy makers. I went around telling the hospital staff the things I was learning, hoping to contribute to an improvement of the hospital/prison. But government bureaucrats constitute another culture, the implicit rules of which I did not know. I constantly said the wrong things to the wrong people in the wrong way during the first few months. Although I had experience in doing ethnography, I had no experience in communicating the results of that ethnography to nonanthropologists for nonacademic purposes.

An Austrian Village

In the summer of 1972, I joined a research team, led by John Gumperz, working in a bilingual village in the south of Austria. The study was a comparison case for Gumperz's earlier work in a bilingual village in India. In the Indian community, the two languages were of completely different families—one was Indo-Aryan, the other, Dravidian. Gumperz showed that speakers used one system of syntactic rules to speak both languages, and that the syntax differed from the standard versions of both the Indo-Aryan and the Dravidian language. Two separate syntactic structures, in other words, had been blended together over many years of bilingualism in the community.

The south Austrian village presented a similar case. For many years, the village had been bilingual in Slovenian and German. My job, as one interested in ethnographic semantics, was to do a study in the area of semantic structure. Did the villagers also have a single underlying semantic structure for both languages? I had about 8 weeks to learn something relevant to that question.

The Austrian experience was yet a different kind of ethnography. First of all, I had been an exchange student in Austria for 6 months, so in some ways it was like coming home. I knew German, even spoke it with an Austrian accent. Second, I had never worked in a community occupied by two other ethnographers. Finally, I had never done so narrow a task. My job was not to "range" all over the territory as I had in previous ethnographic work. Rather, I was shown a particular limited task that would fill in a small piece

of the overall portrait. Austria then, was yet another kind of ethnographic experience that contributed to my sense of how ethnography gets done.

New York

In 1973 I took a research position with the state treatment agency in New York. The agency bore the unfortunate title of New York State Narcotics Addict Control Commission (NACC). (It was later changed to Drug Abuse Control Commission, not much of an improvement, and then to Office of Drug Abuse Services.) In the beginning the position was a luxurious one. All I had to do was define research projects of an ethnographic nature, obtain approval from the research office, and do them.

Because I was a NACC employee, I had access to NACC-sponsored treatment centers, many of which were located in community settings around New York. I picked one center in particular in the Lower East Side of Manhattan. I had good rapport with the chief counselor there, and the neighborhood was interesting—a mix of ethnic groups and some remnants of the old East Village days. A second base in the area was a court referral program run by young Puerto Ricans. I told them who I was, that I wanted to learn about the neighborhood by hanging around, and that in return I would tutor kids in the program. No problem.

Using these two facilities as starting points, I eventually was invited into the streets. Both within the centers and in the streets, I became interested in the use of methadone. Since my earlier Lexington work, heroin had declined on the streets and the use of methadone had increased. Through informal conversations and observations, I began to try to learn why the change had occurred.

At about the same time, I met a remarkable individual. When I first arrived, I thought it would be interesting to do an ethnohistory of the heroin street scene in New York. I put out the word that I would like to meet some older heroin addicts, and the word came back that “Jack” (a pseudonym) would talk to me. Jack was something of a tribal elder. He was part of the early “beat” scene that included people like Burroughs, Ginsberg, and Kerouac. He was a mainstay of Kinsey’s work in the Times Square area. Throughout his 60-some-odd years, he had been through almost every drug scene the country had to offer, beginning with his adolescence in Chicago.

When I met him, Jack was on a methadone program in New York. He agreed to be interviewed (little did I know at the time what an expert interviewee he was) and we began doing his life history. Our interviews ran off and on for 18 months. We still sometimes talk on the phone, and we visit whenever I get to New York. Our relationship is an important one; we are, I think, as close as “researcher” and “subject” ever get. Among many other

things, he highlights an important paradox—it is hard to “do science” with people you like; it is also hard to “do science” with people you do not like; if you do not care one way or the other, it is hard to “do science” because you do not understand enough about the people you are doing science with.

The New York work was important in another way. I was able to take material learned in an isolated institutional setting (Lexington) and apply it in the streets. The Lexington material, by and large, held up fairly well, even though it was dated. Also, I learned that many of the descriptions I had recorded were overly optimistic, but more on that in Chapter 6.

Reviewing Grants

The National Institute on Drug Abuse (NIDA) was created in 1973. It was assembled from pieces of the NIMH and from the Special Action Office on Drug Abuse Prevention. One of its divisions is the Division of Research, part of whose mission is to fund, by grant and contract, drug-related research from a variety of perspectives. One of these perspectives is called psychosocial research. So the Psychosocial Branch was born within the institute.

Most funding agencies have some sort of peer review process. When someone applies for research funding, their proposal is sent to reviewers with the expertise to comment on the quality and feasibility of the research design, the competence of the applicant, the appropriateness of the budget, and so on. In some cases, a standing committee, called an IRG (institutional review group) is appointed to conduct these assessments.

An employee at NIDA, the late Eleanor Carroll, had been arguing for ethnographic representation on NIDA’s new IRG. In the “drug field,” there is a bias toward the medical model. After that, in the area of social science, come people concerned with areas such as the sociology of deviance or psychopathology. Ethnography is a newcomer to the field by comparison. Ms. Carroll won her fight, and I was appointed in 1973 as the token ethnographer on the IRG.

During my 4-year tenure as an ad hoc member, I learned much about the bureaucratization and politicization of social science research. I also learned, with an intensity that made Lexington pale by comparison, that nonethnographers have little idea of how ethnography works. Furthermore, the IRG was a rough, sharp crowd. I saw their tests and questionnaires as encoding erroneous policy assumptions about the nature of drug use. They saw my ethnographic approach as so loose and uncontrolled that it could hardly be considered social science. Much heated and instructive discussion went on over those 4 years.

The NIDA experience taught me much about the politics of research—politics in the sense of research and federal policy, and politics in the sense of

judging research proposals by a variety of disciplinary standards. The IRG also taught me something about getting grants, that old academic hustle. In Chapter 9 there will be a detailed presentation on the submission of ethnographic research proposals.

So much for the sketch of my own research experiences. I described them briefly now, partly so that you will have some background when I refer to them later in the book. But I also wanted to introduce you to the variety of forms ethnographic work can take—complete or partial; alone or with a team; with a village, a clique, or a person. Most of the discussion that follows assumes that you will be the primary ethnographer involved, and that you will start from scratch. If your situation differs, then, of course, modifications will be necessary.

Another purpose in stressing my own experience is that this book is written primarily from that experience. What I hope to do is describe a perspective on doing ethnography, one that I have been developing and articulating in discussions with nonethnographers since 1968. I wouldn't pretend even for a minute that it is "the" perspective on doing ethnography. It, hopefully, will contribute to some recent statements by others as to what ethnography is all about. By exploring the similarities and differences in such statements, we can continue assembling a coherent picture of the ethnographic process in its various forms, and begin to define the holes in our understanding of that process so that we can better articulate what we do.