The Professional Stranger

An Informal Introduction to Ethnography

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Until now, problems surrounding doing ethnography have been discussed—its role in anthropology, getting connected in an area, presenting yourself as an ethnographer, and so on. But there hasn’t been much talk about its goals and their accomplishment. You’re getting a sense of what ethnography is all about—or, more accurately stated, getting a sense of what I think ethnography is all about—from the discussion of these issues. But it’s time to refine the elusive idea of ethnography. First, it would be useful to discuss it in the context of mainstream social science.

The Mainstream

Most research in the social sciences pivots around the idea of the testing of hypotheses. Hypotheses are logically derived from some theory. Theory has many definitions, but in this case it is usually a loosely connected set of empirical generalizations. That is not the only kind of theory you find in social sciences, but it is no doubt the most frequent.

A hypothesis is a statement that has some predicted truth value, assuming the theory is correct. The point of the testing is to check the actual truth value among some group of people and see if the theory’s predictions are correct or not. If they are, then your confidence in the theory increases a bit; if they are not, then there is something wrong with the theory, or perhaps with the test.

The hypothesis states a relationship among a group of variables. In the simplest case, a statement will conceptually link two variables. The higher
the value of variable A, the higher the value of variable B, for example. To test
the hypothesis, some kind of measurement is necessary so that values can be assigned to
the variables. Defining how values are assigned is called an operational definition. The operationalization may be contained in a sur-
vey questionnaire, a psychological test, some entries in the census, or a game
that is played in a laboratory. Social scientists worry about whether the
operationalization is valid (Does it measure what you think it does?) and
reliable (Does it measure the same way at different times?).

Before the hypothesis can be tested, a group must be identified who will
be the subjects of the research. The social scientist specifies who gets to
participate in the research and how they are chosen—the sampling problem.
Assume that the group to be studied is the population of Houston. First of
all, the social scientist must decide how many people to include to have an
adequate sample, so that she can then confidently generalize to the whole
population.

Next, she must worry about how to choose the sample. If every person in
the city has an equally likely chance of being included in the study, then her
heart soars like an eagle, for she has a true “random” sample (if you ignore
the problems that philosophers have with the concept of “random” in the
first place; can you call something a machine generates a random number?).
She may want to stratify the sample by intentionally including more people
within the freeway loop around the center city. Although proportionately
few people live within the loop, the study focuses on that area, so the social
scientist decides to oversample residents there.

If the sample isn’t random, the social scientist must worry about the biases
represented in the eventual sample she winds up with. Say she runs an ad in
Texas Monthly magazine—her sample will probably be biased more to the
political left than a sample from an ad in Texas Parade. In either case, she
will miss people who don’t read at all, or who don’t read those particular
magazines.

Once the sample is defined and the variables are operationalized, she is
ready to collect the data. Often, hired assistants will actually administer the
interviews or tests. Once the data are in, they are analyzed for the answer—
as variable A increases in value, does B increase as well? No one expects
the results to be perfect. The question is, Does the relationship hold strongly
enough so that the social scientist does not completely reject the prediction
of the theory?

The plan of analysis to test the relationship might be based on a correlation
between the two variables. The correlation coefficient that resulted
would be an index of how well the increases of A and B approximated a
straight line if the values were plotted on a graph. Or we might arbitrarily
divide the values of A and B into high and low. Then there would be four
ways to code the data—high A and high B, high A and low B, low A and
high B, low A and low B. If the statement is true, then most cases should be
in high A and high B, or in low A and low B. There is a simple statistical test
that tells you how strongly the idea is supported by the way the cases fall
into the different quadrants.

Right now, researchers who practice this tradition of hypothesis-testing
are calling my ancestors evil things. This sketch is oversimplified and does
not approach some of the more sophisticated procedures developed in the
last few years. As I said in Chapter 2, there are a variety of sources for you
to go to learn about hypothesis-testing research. But I don’t intend to intro-
duce that approach fully here.

A specific study, however, might help illustrate the tradition. I’ll use part
of a study conducted by my friend of long standing, Richard C. Stephens.
Dick and I arrived at the Lexington hospital at about the same time. Al-
though both of us were graduate students working on our Ph.D. theses,
Dick’s was in the tradition of the sociology of deviance. The amazing thing
about Dick is that he actually came to Lexington because he wanted to do
his thesis there. Most of us were there to avoid Vietnam. Since our Lexington
days, we have been arguing many of the issues discussed in this book. A lot
of ideas in the book are undoubtedly stolen from him, though the best ones
are mine alone.

Dick was working within a theoretical tradition in sociology known as
labeling theory. Labeling theory argues that people, once labeled as deviants,
will remain so partly because “significant others” in their social environment
won’t allow them to be anything else. So, he reasoned, labeling theory would
imply that junkies who left Lexington and returned to their home communi-
ties would have a hard time staying clean. Even if they tried to become
straights, their families and friends would know them mainly as junkies.
They would have a tendency not to allow the returning patient to act like
anything but a junkie. Straight behavior from a junkie wouldn’t make sense
to them.

So, Dick formulated a hypothesis: The stronger the labeling when the
patient left the hospital, the more likely that he would relapse (return to
using heroin). Dick broke down the labeling by groups, so one of the more
specific hypotheses became this: The stronger the labeling by the family, the
more likely that the patient would relapse.

Now he had two problems. The first was to operationalize the variables.
Relapse was easy. Because he was using patients released from Lexington, all
of them had gone to aftercare facilities in their hometowns. As part of after-
care, they periodically had to give a urine specimen, which was checked for
narcotics use. So he could just check the records of the urine tests and count
the positives for narcotics as a measure of relapse. Of course he had to worry
about no-shows and refusals to give urine, but by and large operationalizing relapse was not a big problem.

Labeling, on the other hand, was more difficult. Dick talked informally to junkies, read ethnographies and junkie autobiographies, and came up with a list of things that a family might do if they suspected that one of their members was using heroin. He came up with a variety of things. He asked how families reacted if they were in the bathroom a long time. The bathroom is a place where junkies frequently shoot up. He asked if family members checked their clothes for cigarette burns—junkies frequently nod off after they shoot heroin, and cigarettes or hot ashes sometimes fall onto shirts, pants, and so on. He asked if the family would trust the individual to go to the store with a $20 bill. It wasn't a bad list.

He then went through the procedure of developing the items into a scale that measured labeling. The procedure is more technical and complicated than I want to deal with here, involving item-item correlations, cluster analyses, and so on. But he eventually wound up with an operationalization of labeling through the construction of a social psychological scale that measured it. His sample was more or less defined for him because he was dealing with junkies who had left the Lexington program for at least 6 months. Although he visited some of the aftercare centers himself, most of the data were collected by people who actually worked there. Dick would mail them the questionnaires and someone at the center would do the interview.

When the data came back, Dick fed them into the jaws of a computer. With all the various kinds of labeling put together, it correlated about .50 with relapse. Statisticians say this suggests that about 25% of the variance has been explained. Roughly translated, the number means that labeling has something to do with relapse, but there are many other things going on as well that weren't taken into account.

Now, there are several things to notice about Dick's study in particular, and the hypothesis-testing approach to social science in general. First, several assumptions are floating around, like the focus on how people stay the same rather than how they change. Then there's a focus on external factors rather than internal factors that keep them the way they are. Next, the assumption is that these external factors are "social others" who are seen to have control over what behaviors the patient is permitted to display. The social other does this through an unarticulated blend of verbal and nonverbal messages that are only a small part of the emergent system that encompasses face-to-face interaction.

For the real Lexington patient returning to the community, the situation is, of course, more complicated. He changes as well as stays the same; internal processes are as relevant as external ones; other pieces of social interaction might be more important to relapse than those isolated by labeling theory. It's not possible to evaluate the simplifying assumptions without an understanding of what has been left behind in the simplification.

Next, where did the operationalizations come from? There is more to being a junkie than just using narcotics. What other things does he do when he returns to the community? We can only wonder if some of those other things might be better indicators of relapse into the junkie lifestyle than narcotics use. Then, the items in the scale tap the specific details of social interaction with family. Where did these details come from? Many of them came from informal interviews Dick held, similar to those used in ethnographic work, as well as from ethnographic studies that were published at the time Dick was developing the scale. Given the simplifying assumptions of the approach, they weren't bad.

Finally, consider the nature of the human relationship between interviewer and respondent. First of all, it is a short-term, highly specific relationship. Dick, or more often his representative in the aftercare agency in the community, met with the client for a short period of time to get some specific information. The relationship existed only for that purpose, and only for that length of time.

If there was more to it than that, we should know about it. If the relationship had other aspects—friendship, kinship, teaching, or counseling—there would undoubtedly be some tangling of the threads influencing the questioning and the responses. In Dick's study, if the interviewer was also the client's aftercare counselor, we should know something about the history of the relationship to fully evaluate the interview.

In addition to being short-term, the relationship was also asymmetrical. The interviewer was the dominant member in the relationship. He had the right to ask questions and the interviewee had the obligation of responding; he defined the appropriate topics of discussion and the linguistic style in which they would be discussed; he had the power to initiate and terminate the interaction. The social science member of the pair, in short, had control.

The control factor is justified by the demands of the method. The social scientist wants the same framework to be provided for each interviewee. By forcing the behavior of different individuals into the same framework, it can be compared on the same yardstick. Any differences in responses can then be attributed to the respondent, not to a change in the framework.

This argument is somewhat delusional. First of all, different people will have different strategies for fitting their behaviors into a framework that many of them will define as alien to the "natural" way they would deal with the same topic. Because the same questions are read in the same sequence, this does not, at any rate, necessarily mean that the framework is the same.
Is the interviewer lumped in with welfare investigators, interested strangers, cops, or just plain nosy, rude people? The categorization will make a difference in the way the person responds.

William Labov did a study of the verbal behavior of black schoolchildren that makes this point well. When brought in singly to a formal room with an adult black interviewer, the children behaved just like “verbally deprived” ghetto children should. When the interviewer sat on the floor, brought in kids who were friends, provided snacks, and introduced taboo topics, the same children exhibited a creative use of language. In short, the problems of who you are, discussed in Chapter 4, don’t disappear just because you write down the questions. They might even get worse.

None of this is intended to mean that hypothesis-testing research is necessarily a bad thing. On the contrary, it is an important strategy that can play a role in the overall process of doing ethnography. It has many strengths, not the least of which is its explicitness. When one writes grant proposals or articles, he can lay out the hypotheses, operationalizations, and sample design before or after the fact. With this material explicitly presented, the skeptic can evaluate the procedures and criticize them if he wishes. The dedicated skeptic can even copy the procedures and replicate the study to see if he comes up with the same results.

But the people being researched don’t get into the picture until fairly late in the research game, at the time of data collection. Then they only get in as far as they are allowed by the framework that has been set by the hypothesis-testing social scientist. If they don’t like the framework, they can’t modify it and talk outside of it, except maybe over a beer after the interview is finished. If this were the only way social science research happened, the people studied would never have much of a vote. If they don’t refuse to participate, they get to fit a piece of themselves to a framework selected as significant by the a priori simplifying assumptions and operationalizations of the social scientist. And the piece, more often than not, has to be expressed by the person in some way so that it fits an alien framework for its expression.

And yet, speaking from experience in the drug field, hypothesis-testing research is the fashion, the expensive, high-prestige section of research city. Policy makers turn to it for information, even when the simplifying assumptions of the research design are the same ones that support the policy. Such research sets the standards for the review of research grant applications, and an ethnographic grant application often looks bizarre by such standards. Of course, that’s partly our fault because we have never developed much of a methodology to discuss. But I’m climbing onto my favorite soapbox a bit ahead of the game. First, let’s discuss ethnography and find out where the incompatibilities lie between this tradition and ethnographic research.

Ethnographic Research Differences

When you think informally about ethnographic research, a few things strike you right away. First of all, there is an emphasis on direct personal involvement in the community. As mentioned earlier, research of the hypothesis-testing type often relies on hired hands for direct contact with the people who are actually providing the data. I think most ethnographers would be nervous if they lacked a firsthand “feel” for the people they’re working with. Later, in Chapter 8, I’ll give an example of my own experience with “research assistant” ethnography.

Beyond the first-hand contact, though, there are other aspects to the relationship that ethnographers establish with group members. Recall that the hypothesis-tester was one-up in the asymmetrical relationship; he had to be to maintain scientific control. The ethnographer is also part of an asymmetrical relationship, especially at the beginning of her work. The difference is that she, not the informant, is in the “one-down” position.

This initial one-down position is reflected in two of the metaphors ethnographers sometimes use to explain themselves—child and student. What is being said with such metaphors? Both child and student are learning roles; they are roles whose occupants will make mistakes, which is perfectly acceptable as long as they don’t continue to make the same ones. They can be expected to ask a lot of questions. They need to be taught—both will look to established members of a group for instruction, guidance, and evaluation of their performance.

This discussion of the learning role should already have you nervously wondering, “How do I write a research proposal?” It’s not necessarily that ethnographers don’t want to test hypotheses. It’s just that if they do, the variables and operationalizations and sample specifications must grow from an understanding of the group rather than from being hammered on top of...

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1 The article is by Labov (1969).

2 For a couple of sample statements on the importance of direct personal involvement, see Plotnivov (1973) and Weakland (1951).

3 For example, Crane and Angrosino (1974) mention the student role, whereas Cohen (1970) uses the metaphor of the child role. There are many other examples of the use of these metaphors in the literature. Burnett (1974) points out some interesting theoretical implications of the metaphor, noting that a concern with methods from this point of view also has to do with the study of the acquisition of culture in general. I should mention that much of the discussion in this section is taken from a short methodology paper I wrote for the drug field, cited in the References as Agar (1976).
it no matter how poor the fit. You can’t specify the questions you’re going to ask when you move into the community, you don’t know how to ask questions yet. You can’t define a sample; you don’t know what the range of social types is and which ones are relevant to the topics you’re interested in.

None of this goes over well with hypothesis-testing fanatics. Yet this devotion to the initial learning role is one of the major ingredients that makes ethnography the unique concoction that it is. Later on, we’ll worry more about trying to make the learning process more systematic, and then discuss how what you do later in the ethnography may be something like hypothesis-testing, grounded in that critical early learning period.

To get to that later point, you don’t stay one-down forever. Relationships will change over the time that you do fieldwork. Perhaps some group members will never let you go one-down: You are always in the dominant role as far as they are concerned. On the other hand, some relationships will move to symmetry. Or, to “dejargonize” for a moment, you’ll make friends.

To “dejargonize,” some of these people will become key informants or field assistants. You will rely on them intensely for assistance during fieldwork. Later in the fieldwork, if you do some systematic checks of your newly acquired knowledge, you will of course assume the one-up position. At this point, you’ll look something like a hypothesis-tester, except that your one-up stance will be one moment in a relational history that allows other forms for the relationship as well.

That moves us into another difference in ethnography. Recall that hypothesis-testers established short-term, specific relationships with their respondents. Ethnographic relationships are long-term and diffuse. An ethnographer associates with people over an extensive period of time. Further, she associates with them in a variety of contexts—home, place of work, religious ceremonies, recreational activities, and so on.

There are many reasons why this is stressed. First of all, it takes a while for people to accept your role and begin to trust you. Then to achieve the kind of learning to which ethnographers aspire, much time is necessary. Finally, people have different sides of themselves that they display under different sets of circumstances, making it essential to see group members in different situations, not just during a brief interview.

For the same reasons, there is also an emphasis on the ethnographer going into the groups’ home turf to do the research. People are usually more comfortable in their home territory, compared to bringing them into an office or laboratory, though there are times when an ethnographer needs a quiet place for personal interviews. Then, if one is interested in all the situations that a person ordinarily moves through and deals with, it only make sense to be there when it happens. Finally, because much of ethnography can be trans-

lated as becoming part of a group, living with them is a usual correlate of being a part-member.

New problems have been introduced for the ethnographic proposal in the eyes of hypothesis-testers. Just on a practical level, there will be high-budget figures for travel and living costs, particularly if the group you plan to work with is some distance from your usual home base. Then all of this student-child learning is suspect to a group whose starting point for research is a clearly specified hypothesis. Finally, the intimacy with the community and intensity of involvement often produces a criticism of probable bias resulting from getting “too close.” So, you see the problem. Ethnography is a different sort of research process from hypothesis-testing. From my viewpoint, ethnography is the more general process of understanding another human group; hypothesis-testing is a minor, though potentially significant part of that process.

Let me give an example that helps illustrate some of these differences. A few years ago a social science consulting firm was awarded a contract to do a survey of heroin addicts who had been in treatment programs. After interviewers were located in each of the cities included in the study, they were brought to a central location for training. I was asked to come in as a trainer. I told them I didn’t know much about survey research, but they said they wanted me to talk generally about what “these people” were like. It seemed like a harmless enough way to pick up a little beer money, so I accepted.

My talk centered on the idea that junkies, like all of us, have different aspects of self that are presented in different situations. Sometimes they would present themselves as social failures; sometimes as social successes. I used that as a springboard into the importance of the relationship the interviewer establishes with an addict. I talked about rapport and stressed the importance of spending some time loosening up before interviewing if possible, or loosening up after interviewing and learning if you got the same picture as you did during the more formal interview. Finally, I tried to emphasize that this population was not like suburbanites who are stopped by a survey interviewer in a shopping mall. Junkies usually have been interviewed to death. For this and many other reasons, they are often not enthusiastic about another interview—in fact, they may be downright hostile.

After all the talks were over, speakers were asked to circulate among discussion groups. The groups consisted of interviewer trainees and staff members from the research firm. It didn’t take long for differences to become

Wolff (1960) points out that the influence of the researcher, or “personal equation,” discussed in Chapter 4, is considered a problem to be neutralized in hypothesis testing, whereas ethnographers are more likely to accept it as an inevitable part of the research process.
apparent. I was asked what to do if a junkie—patient were approached on the street and, after you explained the study, he or she seemed reluctant to participate. I immediately advised backing off from the formal interview and suggesting something else that would allow for some informal talk first, like “Let’s go get a beer before we do this thing.”

While some of the group liked the idea, the leader didn’t at all. She said that it was company policy not to drink while conducting interviews. Among other things, it might offend the next person approached for an interview if the interviewer had alcohol on his breath. I had visions of junkies I’ve known saying, “Sorry, man, I don’t talk to anybody who just drank some beer.” Some of the trainees chuckled with me at the absurd image.

Shortly after that another person asked what to do if it became clear that the respondent did not understand the question as phrased. “Paraphrase,” I said, giving some examples from a copy of the questionnaire sitting in front of me. Again, there was a horrified look from the group leader. She disapproved, noting that it was important for the questionnaire to be presented in the same way to each respondent. At that point, all I could think of was a quote from Charles Frake addressing a psychologist, “If you ask a question and people laugh, you say, ‘Come on, this is serious.’ If I ask a question and people laugh, I wonder if there isn’t something wrong with the question.”

The next day, the training interviews started. There were rumblings about how some of the local junkie—patients who were serving as guinea pigs were not showing up as scheduled. Of those who did show, some were downright nasty. One trainee told of his bizarre experience. He had met “his” interviewee at the door, and had chatted amiably as they walked to the interview room. As soon as they sat down and he placed the clipboard on his lap, the interviewee became “sullen.” At the end of the interview, as they walked out, they again had a pleasant talk, loitering on the front steps of the building for 15 minutes or so because they weren’t ready to end their conversation. “Weird,” said the trainee in summary.

Part of the junkie’s reaction was no doubt due to the interview training session itself. I was asked to attend a couple. The trainee and the interviewee sat in the middle of an ordinary motel-type room. They were surrounded by other trainees, invited consultants, company representatives, and sometimes representatives from the university that had the main contract for the research. At the sessions I visited, this didn’t seem to bother the junkie, but the interviewer—trainee didn’t look very comfortable.

In one unforgettable scene, an older female methadone patient sat quietly, a weary look on her face, while the interviewer nervously stumbled through the questions. I felt bad for the interviewer because the sensitivity that she displayed in her nervous reaction to the awful setting meant she might do well once she got out on her own. Finally, the old woman couldn’t stand it any longer either. She leaned forward, and with the kindest of eyes patted the interviewer on the knee and said, “There there, hon, you’re doing just fine.” So oppressive was the situation that there was hardly any reaction. The interviewer just plunged on ahead.

And then there were the questions. A chart was offered that had the preceding 5 years divided up by months. Respondents were supposed to locate events related to drug use, arrest, treatment, criminal activity, and employment. In my observations, they had difficulty doing so. The appointment calendar does not have the sacred value in the street life that it does in the life of survey researchers. It was interesting to listen to those who thought it out loud as they tried to fit their history to what they saw as a bizarre framework. I can’t remember exact samples, but it might have gone something like this: “Let’s see. That would have been about the time Nixon was elected the second time. Right after I started working there, they had that thing about fight-fixing down at the stadium.” People were locating events in relation to significant events in their own personal history. Their memory was not organized by the precision of the yearly calendar. However, their recollections might be transformed into an approximate position on the yearly calendar by locating the event of interest near events that had been reported in the public media. This simple time transformation was not provided, though. The respondent had to do the work, without access to verification of public event dates.

People also had trouble with the time lines that the interviewer was supposed to draw. To draw the line, they needed to know, for example, when the respondent “started” and “stopped” using heroin. But the respondent might say, “Well, I started about March, and then used quite a bit for a week or so, but cut down some, but later I got hooked, even though I tried to control it once in a while. I think I kicked for a couple of weeks, but I went right back to it. Finally, I had to stop again in September when I got busted.” Should that be a continuous line from March to September? If it shouldn’t, it is difficult to know how to break it, especially when the respondent won’t remember when the breaks occurred within that 5- or 6-month period. It’s not that it’s impossible to transform personal histories onto time lines. It’s just that whoever designed the question did not understand the amount of work he was asking the respondent to do; nor was he sensitive, apparently, to the amount of “uncontrolled,” “unstandardized” information processing that was going on (assuming the respondent cared enough about the interview to bother at all).

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5The quote is paraphrased from a transcript of a conference proceeding published in a special issue of the *American Anthropologist*, listed as Romney and D’Andrade (1964).
That story hardly covers all the differences, but it hopefully illustrates some of the problems when ethnography and survey research interact. Many in the literature deal with the fit between hypothesis-testing and ethnography. Before moving on, I would like to discuss a couple of other critiques of hypothesis-testing approaches—one that focuses specifically on operationalization of variables. Using them, with the help of a couple of my own examples, we can make an initial foray into the goal of ethnography.

Two Cases of Ethnographic Critiques

Edmund Leach is a British anthropologist who had done ethnographic work in Sri Lanka. He contributed to an edited volume on field methods by taking a hard ethnographic look at a survey done in that country. His chapter is a detailed critique of the results based on the manner in which certain critical elements of the survey were operationalized.

For example, he notes that a household was defined as persons who cook rice from the same pot. But then he goes on to consider additional information. For one thing, Sinhalese village girls marry young. However, they all have a separate cooking pot. Then he notes a second observation of village life—most property is handed on only when the original owner becomes elderly. The results? A household consisting of a married couple with their three married sons would, by the survey definition, be counted as one landed household and three landless households. So, Leach concludes, we can only wonder how many of the 335 landless households (out of 506) are actually young, recently married adults who are heirs to still living parents.

Let me give you one more example. Landholdings were classified into high-yield and low-yield. But cultivated lands, Leach learned from doing his ethnography, were of two types. One type, traditional lands, was estimated by using a rule of thumb that Leach describes in some detail. But the results, based on his work, usually overestimated actual acreage by about 50%. The other type of land, called acre land, was developed recently after purchase from the crown. These lands were actually surveyed by professional surveyors.

The conclusion is that traditional lands will always look like low-yield lands, because their acreage is overestimated by one-half. When you add the knowledge that acre lands are usually held by the wealthier villagers, it partly explains the apparent relationship between large holdings and high-yield lands.

There are many other examples in Leach's article. The debate is made even more interesting by the political implications of the results. If Leach is correct, then the situation in the communities studied is not as bad in terms of landholdings as the survey concludes. If Leach is incorrect, then he is erroneously contributing to a status quo that supports a few wealthy landowners. The political consequences of this kind of methodological debate, here as in the drug field, are often frightening.

Let me give you one more quick example. In an article in the same volume, A. J. F. Kobben talks about some problems in gathering census data in Surinam. For example, he notes that informants feared jealousy if they had a large number of kids, so large families lowered the number of children they would report. And mothers feared talking about children who had died because of possible connections with witchcraft. As a final example, the men were mobile, having more than one family in one village. In fact, they even joked among themselves about how hard they were to count.6

Kobben and Leach are both criticizing the interpretation of what would seem to be fairly straightforward "facts"—household composition, land yield, number of children, and so on. These facts were zeroed in on in one context—the informant's—then lifted out and placed in another context—the researcher's. But the meaning of the "fact" changed in the transition. In Kobben's critique, the meaning of "children" was connected to beliefs about jealousy and witchcraft for the informants. For Leach, land ownership was a poor fit with connected ideas about marriage, cooking pots, extended families, and customary inheritance.

As I think about my own experience and the literature I reviewed, I begin to realize how frequent is this ethnographic critique of other social science research. The critique highlights another difference between hypothesis-testers and ethnographers—one that has more to do with a fundamental difference in research worldview. An ethnographer learns something new, and then tries to understand how it connects with other aspects of the situation in which the new learning occurred. As if that weren't difficult enough, he then tries to see if it connects with other things he has learned that are not immediately apparent—things like parts of the belief system, or the history of the group, or the wealth of the informant.

This formidable search for connections, this ethnographic belief that an isolated observation cannot be understood unless you understand its relationships to other aspects of the situation in which it occurred, is called a holistic perspective.7 This perspective, of course, has its own problems, such as the holistic fallacy, when an ethnographer constructs a connection because of his bias to find one without checking it out carefully. And holism does not mean that you can never lift things out of context and talk about them in isolation, though it does mean that this lifting is a more complicated process than most hypothesis-testers think it is.

6 See Leach (1967) and Kobben (1967).
7 For a lucid philosophical discussion of holism, see Phillips (1976). The term holistic fallacy comes from Sieber (1973).
Holism does help to understand, though, why ethnographers are cautious with the idea of a variable. For what is a variable but something that can be measured in a standardized way across situations, across people, across groups, and even across cultures. From a holistic point of view, the very idea of a variable is enough to make one skeptical. Yet in spite of its traditional importance, holism is reported on the decline as more and more anthropologists move towards problem-oriented research and increasingly argue for hypothesis-testing methodologies.\(^8\)

Later in the book this will be discussed at some length. As you have noticed, I have mixed feelings. On the one hand, I am concerned with the development of a more explicit ethnographic methodology; on the other hand, things like the learning role, the long-term extensive personal involvement, and the holistic perspective are what set ethnography apart—they enable us to learn what people are like rather than seeing a minute piece of their behavior in a context we define supports or does not support our ideas of what they are like.

A survey sociologist would no doubt read my account and give a point-by-point rebuttal.\(^9\) His strongest rebuttal would probably come at the end of the argument, when he said, “So what do you have to offer as an alternative? Some self-serving anecdotes that support a conclusion you might have reached before you even started doing your research?” It’s time to start taking that charge seriously. We have to offer something in the way of methodology as an alternative.

### Goals of Ethnography

Methodology is not something to be appreciated solely in terms of its internal aesthetics. Methodology serves some purpose, some higher-order goal. A particular method is a procedure that is a part of the larger process of doing ethnography. What we need, then, is a sense of our goals—just what are we trying to accomplish when we do ethnography? Only then can we properly evaluate specific methodologies. After we use a method, we should be closer to the goal than before we used it. If we are choosing between two methods, and one gets us closer to the goal, other things being equal, we should select it.

All this is obvious, but it is important to remember to keep methodology in perspective. Several years ago I went to a conference on decision making in natural context. Interestingly enough, the conference quickly broke into two groups. One group was fascinated with the methodological techniques available in the mathematical study of decision making. Their guiding question was, “What kind of data do we need to collect to fit this elegant model?” The other group was primarily interested in doing ethnography. Their question was, “How much do we need to hammer and weld this model so that it fits what these people are doing when they make choices?” The difference here is methodology as an end in itself, or in subordination to goals of doing ethnography. There’s nothing wrong with working only with a specific method in isolation. It’s just not what we’re talking about here.

So what is this broader ethnographic goal? If it seems like I’m avoiding the question, it’s because I am. I’m not sure what a precise goal statement would look like. In cognitive anthropology, some have suggested that the goal of ethnography is to be able to “behave appropriately” in a community.\(^10\) You’d also want to include behavior “inappropriately” as well, but that’s not really a problem. Intuitively, I believe there’s a kernel of an explicit set of goals in there, but it’s simply too vague at this point. Where is the boundary of appropriateness, and who in the community is the judge?

What about the notion of student–child discussed earlier? Again, therein lies an implicit goal. The ethnographer’s purpose is to learn—to acquire some knowledge that he previously did not have. But a cursory glance at the psychological literature teaches you what a simple-minded statement that is.

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\(^8\)For example, Johnson (1978) notes that the “contradiction” between holism and detailed measurement, which used to be resolved in favor of holism, is now beginning to be resolved more in favor of detail. Similar arguments can be found in Naron and Cohen’s (1970) introduction to their edited volume, and in Kaplan and Manners (1971). As pointed out in Note 15 in Chapter 2, many of the recent methods books in anthropology emphasize quantification, standardization, and hypothesis-testing, although they do so in a perspective that includes at least some concern with the general idea of ethnography. On the other hand, Edgerton (1970), after reviewing a variety of methods used in psychological anthropology, notes that one problem is that such methods force us to “atomize” and lose context. He suggests that we develop our own methods that retain a sense of context, complexity, and interaction. That pretty well summarizes what I had in mind when I started work on this book.

In 1996, it’s clear that what I targeted was moving in the direction of hypothesis-testing turns out to have been half-right and half-wrong. The worst part lies in the fact that the academic wing of American anthropology has pretty much rejected this approach completely, for several different reasons discussed in Chapter 1. At the same time, other anthropologists who work in settings where positivism still holds sway argue that such a rejection is self-defeating. These different positions have sometimes turned professional meetings and academic departments into war zones. Chapter 1 attempts to untangle the contentious arguments and aim towards a more productive synthesis.

\(^9\)He could make an even stronger rebuttal by pointing out that surveys have been used very productively in some studies. Although I’ll talk more about how ethnography can use surveys later, there are several good sources in the literature. Powdrell (1987) shows how a survey was useful in her work in South Africa. Bennett and Thaisis (1970) review the anthropological literature and show many cases where surveys were productively used. Vidich and Shapiro (1955) also describe how a survey and ethnography interacted.

\(^10\)The classic statement of this goal is in Goode (1957).
What kind of learning? What kind of knowledge? The structure of knowledge or the process of using it? What if the knowledge is not new for the ethnographer? And, as in the preceding paragraph, whose knowledge are you trying to learn?

If these are two of the clearer goal statements, we are in trouble. Perhaps with a bit of strategic oversimplification, we can establish a more explicit goal for now. It won’t be broad enough, but it may serve as a beginning. The goal will hinge on the idea of *paraphrase*, which will stress the importance of interpreting the perceivable world of sound and motion.

Let’s take an example of paraphrasing from my work in India. You stand with an informant in the *tanda*. You are about to leave, and someone is preparing a snack for you to eat on the trail. Just before he wraps the food in a cloth, he places a small piece of charcoal in the bundle. Now, you and the informant both saw the same stream of motion. If someone asked you what just happened, you might say, “Hell if I know. He just put a lump of charcoal in with the food. Maybe it’s supposed to flavor it.”

Suppose the same person now turned to your informant and asked what happened. The informant says, “It is midday, a time when spirits are especially active. People alone are particularly susceptible to spirits, especially when they are carrying food. Spirits are repulsed by charcoal. That’s why he put the lump of charcoal in there—to protect you.”

Consider another example. In New York I see a person going into a methadone clinic, as does an informant who is also a clinic patient. Someone asks me what he is doing, and I say “He’s probably a patient in the program. He’s an outpatient, so he must go into the clinic to obtain his methadone.” He might also stay for a group session or a meeting with his counselor.” Now the same person asks my informant. His reply is much briefer: “He’s going to cop.” Again, we both saw the same motions and heard the same sounds, but my statements were an official interpretation of methadone clinics, while the informant’s statements interpreted the same behavior from a street point of view.

In both cases, the informant and I are giving different accounts of what we have seen and heard. We each took our own sense perceptions, decoded them to assign some kind of meaning to those parts of the sound and motion that we attended to, and then on the basis of that meaning, we produced some statements that were our accounts.

Hence the relationship between giving accounts and paraphrasing. The paraphrase is something that ethnographers have always used, whether or not they were primarily interested in linguistic matters. Paraphrasing is a powerful test of comprehension. If you hear a sentence, can you properly decode it? Do you understand it in one of the possible ways that the listener does? If you do, you should be able to encode it into a new sentence or group of sentences, such that the people around will say, “Yes, that’s what I (he/she) just said, all right.”

“It’s hot in here” can be intended, and interpreted, as a request to turn down the heat, or a hint that going out onto the open air balcony might be a better place for an intimate conversation than a crowded party. So what happens when our ethnographer tries a paraphrase and says, “Oh, you mean the temperature is higher than you normally prefer?” Everyone collapses in hysterical laughter. In fact, sometimes I think the only reason ethnographers are tolerated at all is for their entertainment value.

This goal should serve to test our understanding and invite group members to comment if we are wrong. We will rely initially on sharpening the ability to paraphrase as a working ethnographic goal. But recall that we are using *paraphrase* in a broader sense than its traditional use in linguistics. We are talking about the ability to decode rather involved sequences of verbal and nonverbal behavior, and then encode our understanding of the meanings of that sequence into some utterances to check whether or not we understood what just occurred. It is in this special sense that I speak of giving an account.

In the examples given earlier from the *tanda* and from New York, my account differed from that of a group member. My immediate goal is to reduce the difference between the two accounts, so that mine better approximates a group member’s. Anything that helps me do this is a valuable ethnographic method.

Several objections to this goal came to mind. First, why the emphasis on language? The emphasis is on language as *metalinguage*, as far as the goal is concerned. One of the confusing things about human language is that it is both object language and metalinguage. As object language, it is part of the flow of behavior that occurs as group members do the activities they do. But it is also a metalinguage, used to talk about that flow of verbal and nonverbal behavior. An account is given using the group’s language as metalinguage.

An ethnographer’s ability to give an account might be modeled by some procedures that take as input the observation of behavior and provide as output a set of metalinguage statements that group members judge to be a correct interpretation of what is going on. Of course, there are problems here. For example, there may be more than one correct account, and group members may disagree in any case on which account or accounts are correct.

It strikes me, though, that such problems are informative, and it is a strength of the focus on the goal that they occur. The ethnographer wants to know about alternatives and disagreements within the group. Variation like that is only a threat to a social scientist committed to a monolithic portrait of group life. Unfortunately, the quest for “the normative order,” deeply ingrained in many social science traditions, has sometimes blinded us to the
many important lessons for the ethnographer when confronted with variability as well as uniformity.

But what if the ethnographer comes up with an account that no one ever thought of before? All kinds of interesting things can happen here. You may elicit an “aha” reaction from informants, showing them a connection they feel is true but which they never previously consciously articulated. In fact, you may contribute to account-giving strategies among the very people you are studying. That’s a situation that some call “training” informants.

And what if the ethnographer has ideas about what is going on that differ from group members’ accounts? Good question, but wrong place to ask it. At this point, I am just worried about defining a low-level ethnographic goal—learning to give accounts of an event like community members do. Later on, higher-level analyses of accounts will be discussed—like the relationship between accounts of different events, and the problems when an ethnographer learns about and checks out accounts that may be contradictory to the accounts of informants. But these larger-level analyses presuppose an ability to give accounts, so right now we’ll stay with that.

Another serious objection might be the passive role of the ethnographer implied by the goal. Is he to become sort of a commentator, sitting on the sidelines and giving a play-by-play account to a panel of listening and dissenting judges? This role would infuriate people like Richard Nelson, who advocates a method of “direct participation.” In his work in the Arctic with Eskimo and Indian groups, his philosophy was, “If you think you can describe seal hunting, then you yourself should be able to successfully hunt a seal.”

This direct participation goal makes me personally a bit nervous, since I can easily imagine myself putting a spear through my right foot. There are other problems. If I think I can give an account of a religious ceremony as group members do, does that mean I must be capable of conducting it? Group members aren’t; the ceremony is done by a specialist. If I can give an account of a wedding ceremony, does that mean I must be capable of going through it? I hope not.

In spite of the problems, the sentiment is an important one, but I’d hate to make the ethnographer’s object language and behavior in the stream of community life the ultimate test of ethnography. For one thing, he’s had less practice and must deal with interference with his own culture, unlike informants. For another, he may have problems for a host of personal reasons—self-consciousness, shyness, or he may just generally be what in the jargon of the social sciences is known as a klutz.

At the same time, the goal of accounting is by no means intended to isolate the ethnographer. If for no other reason, he should be part of the flow of community life to learn to ask better questions. And nothing in the goal prevents an ethnographer from jumping in. If you want to hunt seals, more power to you. I’d made a lousy seal hunter, but that doesn’t mean I’m not interested in understanding folks who do.

So, let’s begin with the goal of giving accounts. It’s not perfect, and it’s oversimplified, but at least it’s a start. We can’t really discuss ethnographic methods until we have at least a tentative sense of what those methods are supposed to accomplish. To begin, this is what those methods are supposed to accomplish: They should add to the procedures used by the ethnographer to transfer observations into accounts that group members say are possible interpretations of what is going on.

Before plunging into specific ethnographic experiences again, some comment needs to be made about theory. Some of this section sounds like the beginning of some critical theoretical issues. What are those “procedures” that transform observations into accounts? Does an “account” have certain features that distinguish it from other informant utterances, and are these features more general than just what you find in your study of a specific group?

For the second time, we are confronting the relationship between methodology and theory. Can we sensibly treat them separately, or are they so intertwined that we separate them at the risk of obscuring rather than enhancing our understanding? Again the issue appears, and again we put it off until later. For now, let’s accept the broad goal of accounting and return to the field. Now we can take up some specific problems that you will face as you begin doing your work.

10If I were writing the book in 1996, I’d shift the discussion of “giving accounts” over to the concept of “communicative competence”. Beginning in the 1980s and continuing into the present, I’ve done a lot of language-based work in Austria, some of which is referred to in Chapter 1. Communicative competence, a concept introduced by Dell Hymes in a work cited in the first edition of Strangers, takes the ability of a person to enter into the flow of situations that make up group life. Communicative competence does include the ability to give accounts, but also goes well beyond it. My work in Austria convinced me that the acquisition of communicative competence was what ethnography was all about. In fact, the ethnographic goal wasn’t all that different, I decided, from the goal of an immigrant to the country in question, a situation that is now much more common than when the original Stranger was written. If I were writing the book now, I might call it The Professional Immigrant.