The Professional Stranger

An Informal Introduction to Ethnography

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Beginning Fieldwork

You arrive, tape recorder in hand, with a grin rigidly planted on your face. You probably realize that you have no idea how the grin is being interpreted, so you stop and nervously attempt a relaxed pose. Then you realize that you have no idea how *that* is being interpreted. Soon you work yourself into the paralysis of the psychiatrist in a strip joint—she knows she can't react, but she also knows she can't *not* react. It is little wonder that people sometimes hide in a hotel room and read mysteries.

This, of course, assumes the worst—that the group is a complete unknown to you. You might have read some ethnographies of such groups, but those sometimes have a disappointing lack of relevance when you're actually faced with a representative of the group that supposedly has been described. This early phase is, I think, the source of the mystique of fieldwork. You are adrift, trusted by none, and unsure of what is going on around you.

In this chapter, I want to consider some of the problems of the early phase of fieldwork. Several problems are relevant here. First of all, some people will be more likely to approach you than others. Then you have to learn to steer conversations in directions that cover areas you need to begin to learn about. You must also make some sort of decision on how and how much of these early experiences to record. Finally, as you begin to integrate into the community, or perhaps even before, you may want to add some community members to the research team.

Who Talks First

Often an ethnographer bases much of her work on intensive discussions with a very few people. Rather than looking at the distribution of a few predefined variables in a large population, she is trying to learn the interrelationships of a large number of discovered variables among a few people. The ethnographer often comes up with a statement like, "There *exists* a pattern such that . . ." But the reader gets no sense of how it is distributed in the larger population.

A good analogy can be found in linguistics. When the field linguist (as opposed to those who "introspect") works on a new language, he sits down with a few informants. He gets some data, and then comes up with some tentative rules that articulate the implicit structure in those statements. Then he gets more data, further tests the statements, gets more data, and so on.

Then along came the sociolinguistic arguments of people like John Gumperz and William Labov.[1] People speak differently from each other, something that becomes striking as soon as you leave your four informants and listen to the other 5000 people in the town. Traditionally, linguists often called this "free variation," meaning that things could be said in different ways, but that they didn't want to deal with it. They had enough problems just trying to account for a few informants.

So Labov, to take just one quick example, studied the way that the pronunciation of the *r* varied in New York City. He found that the more formal the situation, and the higher the socioeconomic status of the speaker, the more likely the *r* was to be pronounced. By studying distribution in a large population, Labov cut a piece of understanding out of the "free variation" wastebasket and began the process of adding it to linguistic theory.

The catch, of course, is that he would never have known which variable to look at without the blood, sweat, and tears of previous linguists who had worked with a few informants and identified problems in the linguistic structure of American English. All of which finally brings us to the point of this example—traditional ethnography struggles mightily with the existence of pattern among a few. It's a worthy, necessary, first-priority struggle, but it's not enough. A couple of chapters down the road, the problem of *distribution* of these patterns will be discussed. For now, the moral of the story is that when you do your initial work with a few people, you had better worry about who they are. If you had done linguistic work among New York's elite at museum benefits, you would come up with a phonological description

[1] A recent collection of Gumperz's work can be found in Gumperz (1971). The example of Labov's work is listed in the References as Labov (1966).

that said the *r* is always pronounced. The reviews of your work in the *Daily News* would be awful,[2] and you probably wouldn't get tenure.

In my own fieldwork, I find that usually the first people to approach are "deviants" and what, for lack of a better term, I'll call "professional stranger-handlers." Most groups have official or unofficial stranger-handlers to deal with outsiders. Such stranger-handlers are natural public relations experts. They can find out what the outsider is after and quickly improvise some information that satisfies him without representing anything potentially harmful to the group.

Among the Lambardi, the professional stranger-handler was an older man named Sakrya. He was the first who came up to talk with me when I entered the *tanda*. He pleasantly explained, for example, that the *tanda* was overcrowded. Therefore, I had to understand that it would be impossible for me to live there. It was Sakrya who suddenly appeared whenever I began doing something bizarre in the early days of fieldwork, like drawing a map or measuring the dimensions of a *tanda* hut. After a couple of months, it was partly Sakrya's decision that I was trustworthy that opened up the *tanda* to me. He was also quick to rent me a hut and provide me with *tanda* people to hire who were kin, but then that's probably a fringe benefit of being a stranger-handler.

When I first began working in Lexington, I was met by stranger-handlers there, too. One became a close friend. Like many Lexington junkie–patients, he reappeared a couple of times during my two years there. In fact, he used to joke, "I don't know if it's better to do your time 8 hours a day like you do, or a few months here and then a few months on the street like I do." Anyway, Jack was one of the first who approached me when I went on to the "unit" (the patient dormitory area).

I later learned that Jack, like Sakrya, enjoyed a solid insider's reputation. I suppose a professional stranger-handler would have to enjoy such a reputation since the position requires the trust of group members as a prerequisite. When he first met me, he bluntly said, "How do I know you're not a fed?" I said, "Try me." He later told me he couldn't turn down a challenge, so he began carefully guiding me through patient territory. He ensured that I didn't see anything I shouldn't see. At the same time, he would introduce me into some settings to see how I acted and what kind of questions I asked. He gave me false information about violations of hospital rules to see if I would report them. Like Sakrya, his eventual approval opened up several new relationshps in the unit. And, like Sakrya, he benefited somewhat by having control over some resources—magazines, books, food, and phone calls.

[2] For the non-New Yorkers, it is said that the *Daily News* enjoys a wide readership among working-class folks, whereas those who would go to museum benefits probably read the *Times*.

The "deviants" were another case altogether. *Deviant* here simply means members who are on the boundary of the group in some low-status position. When the ethnographer approaches, he represents a potential resource that the deviant might control to gain status. The deviant has nothing to lose and everything to gain by approaching the ethnographer and establishing a relationship.

The *tanda* presented me with my most unusual case of early encounters with deviants. Early on, a younger man came up and talked to me at length. He had several ideas about things to do in the *tanda* and he wanted my help. When he left, I asked Sakrya who he was. "Oh, he's the *naik* ('headman')." The man was a loner, and didn't appear to receive any of the respect I would have guessed a headman should receive.

Later, I learned that this *naik* was a bad character. His father had been a good man, but he died young. Because the position was hereditary, there was no choice. He was an embarrassment to the *tanda* because he had grown up stealing from other Lambardi, and he continued to do so, especially when drunk, which was as often as possible. It was tragic. An informal council of elders ran the *tanda,* and when a *naik* was necessary, they called a respected *naik* from a neighboring *tanda* to come and preside over the council. At such *tanda*-wide gatherings, the local *naik* would sit apart from the group, not participating in the discussion. He was one of my first informants.

In the hospital at Lexington, some of the "deviants" were like the staff. Although they were heroin addicts, they were of middle- or upper-middle-class backgrounds, often with college educations. Many of them felt out of place in the imported street world that flourished in the unit, so they would hang around the staff a lot and talk with them.

One of them played an ironic role in staff–patient exchanges. He was an articulate, intelligent addict who was "doing well" in treatment, actively participated in institution activities, and was generally held up as a "model patient" by the treatment staff. However, as I learned after I lived in the institution for two weeks, he was also a homosexual, going around the unit at night after the staff went home and having sexual relations with several patients. The patients enjoyed the staff's presentation of a person they would ordinarily define as "sick" as representing the goal they should all aspire to.

I don't mean to say that it's *only* deviants and stranger-handlers who will approach you at first, but I'd be willing to bet that a large proportion of early ethnographic contacts are one or both of these types. The problem is even more acute because in the early part of fieldwork you probably don't know how to identify them. But, after all, what kind of person would run up to talk to a stranger with the bizarre intentions of an ethnographer? You should wonder.

There is more room for worry if you consider that these are the only people an ethnographer talks with. With too much distance, or too little

time, this could happen. Imagine the picture you get of community life by just talking with its outsiders and public relations experts. It might be accurate, but one should get some information from the "regulars" to verify that.

There are some notable counterexamples to deviants and stranger-handlers in the literature. One of the most striking is William F. Whyte's description of Doc, his main informant in *Street Corner Society.* Whyte was introduced to Doc by a community worker who thought he might be helpful in the study. By Whyte's account, Doc didn't seem to care about his background and intentions, beyond asking if the book he planned to write would help "change things." He turned out to be a leader in the street scene. He more or less told Whyte to be quiet and let him do the talking. Among other things, he quickly taught Whyte such basic ethnographic skills as not asking questions too early in the game, since people would just "clam up." He advised Whyte to listen and he would learn almost everything. He also told him to be himself. It doesn't look good, Doc said, to try to act like a street corner boy.[3] By my reading, Doc was neither a deviant nor a stranger-handler. We should all be so lucky.

At any rate, as time goes on you will begin to widen your social circle. In Chapter 4 I suggested that one not worry about the "official" explanation of one's work, though as I noted then, some ethnographers do offer specific advice, sometimes stressing the critical importance of the early definition of the ethnographer's role.[4] My advice is to worry more about deciding which of the people you meet you want to work with intensively in those critical early days. How do you know when a person like that comes along?

The official term for a good relationship with an informant is *rapport.* At this point, I feel like a newcomer to an art museum—"I don't know anything about modern art, but I know what I like." As I try to think about the key people I've worked with in my different ethnographic studies, they strike me as a varied lot. Most of them, I think, had characteristics that would have

[3] See Whyte (1955), especially the methodological appendix where he discusses meeting Doc. Casagrande (1960) has edited a collection of articles by anthropologists about their key informants. In his introduction, he notes some common themes in their lives—they usually had contact with Western culture, sometimes intense, with both positive and negative consequences. They usually had some personal tragedy in their life history, and some occupied positions of authority. There are also numerous accounts of early contact with informants in personal accounts (cited in Note 14 of Chapter 2) and sometimes in the text of ethnographic reports.

[4] See Note 20 in Chapter 4. Also, Wax (1971) emphasizes the importance and difficulty of the early stages of fieldwork. Evans-Pritchard (1940) gives some excellent examples of difficult fieldwork, such as a transcript of an interview with a hostile informant, and a description of informants showing up and announcing that everything they have told you was a lie. I do agree that the earlier part of fieldwork is difficult, but mostly because of the strains discussed in Chapter 4. Though there are exceptions, I agree with Pelto and Pelto (1973) who state that, by and large, the "stranger" (that word again) is usually well accepted after a time and finds himself capable of doing his work.

attracted me to someone in ordinary life—though there's always that boundary problem. I'm not sure how to define *rapport.* The personal equation—the importance of the ethnographer's personality—suggests that the definition will be a variable one. Maybe we should read the social psychological research on personal attraction.

Some in the literature do worry about why an informant would bother to enter into a long-term relationship with an ethnographer at all. There are arguments for the ethnographer as partner in a transactional exchange, ethnographer as exploiter, and ethnographer as exploited. The informant, then, depending on your point of view, gets "personal confirmation," is thoroughly ripped off, or acquires a resource to be exploited as a "Fort Knox, pawn, social expert, or technician." Then there is also the commonly reported ethnographic role as "druggist" and "scribe."[5]

I think there are several reasons why, once past the initial phase, an informant might talk with you. First of all, you're a person who's genuinely interested in and respectful of another's point of view. There usually aren't many people like that around. Second, most people enjoy telling their story to an interested listener. Third, rather than coming on like Hitler, you are interested in adapting to group life—sharing their living conditions, their food, and so on. And fourth, you are perfectly willing, probably insistently so, to reciprocate in reasonable ways with the people you work with.

You are a potential friend, though of a peculiar sort.[6] In fact, many ethnographers I know are uncomfortable with the depersonalized labels for the people they work with—somehow "natives," "informants," and so on seems too cold for the relationship they have. At the same time, the relationship is not really a friendship since the ethnographer is, after all, "doing research" and, once he leaves the group, he may have little or no further contact. The paradox of detached involvement appears again.

At any rate, as you begin fieldwork and encounter people with whom you have rapport, you must pick some to work with intensively. Here the choice becomes critical, for rapport with one person (or clique) may shut down alternatives.[7] For example, when Sakrya the stranger-handler gave me his

[5] Hatfield (1973) reviews the different models and uses the terms *Fort Knox,* and so forth. Lundberg (1968) describes the transactional model of fieldwork. Paul (1953) describes the "druggist" and "scribe" roles, and also cautions against the possible scapegoating of the ethnographer.

[6] Pelto and Pelto (1973) describe the peculiar nature of this strange "friendship."

[7] Morris (1973), Miller (1955), and Vidich (1955) all discuss this problem. Berreman's (1962) monograph is a thorough case study of how two different field assistants—one Brahmin and one Muslim—led to very different contacts in the village and very different kinds of data. Wax (1971) gives a similar account of how the choice of a family to live with on an Indian reservation was disastrous both in terms of the control over information that the family exercised and in the range of social contacts they allowed.

seal of approval in the *tanda,* his faction opened up to me, but, the other faction in the *tanda* became more distant. Though I had cordial relationships with them throughout the study, and friendships with a few, most of what I learned came from Sakrya's group. Because of such problems, it's good to pick people to work with from different sections of the community. It's not easy to do that, because in the early days you're not sure what those sections are. It's because of this representativeness problem that issues of sampling and distribution will be discussed in Chapter 7.

Finally, you need to select informants who can inform. They must first of all have the time to do so. Then, as it was so succinctly stated in one article, the informant's job is to collect, retain, and transmit information, and to have the motivation to do so.[8] Although I wouldn't advise ruling out others, especially later when observational work is discussed, it does suggest that inarticulate people who have poor recall, and who don't get around the community much, would be poor choices to work with. It's good to remember, too, that you're looking for a teacher with whom you have rapport.

Asking Informal Questions

Now that you've found people to talk with, what do you talk about? There are at least two possible starting conditions. The first assumes that you know enough about local talk to actually conduct an interview. When I began working with junkies, there were language differences, made more subtle because we supposedly shared the same language, but at least I could begin interviewing. In the *tanda,* on the other hand, though I had studied Kannada before leaving, I found myself incapable of participating in the flow of conversation, so I needed interpreter help and more language study. For the moment, assume you can, in fact, talk well enough to begin interviewing.

Because my immediate goal is to understand the informant's ability to give an account of a situation, my interest is in learning what the significant details of an event are and how they are connected. In short, I am after what one anthropologist calls "experience near" concepts. I want to learn how informants interpret the world through which they move. There are other ethnographers who emphasize the direct observation of behavior, recording that behavior using concepts that they define—"experience distant" concepts—and they, of course, would write a methods book from that point of view.[9]

[8] The article is by Back (1956).

[9] Experience near and experience distant concepts are discussed in Geertz (1976).

There is a meeting ground for these two points of view that we'll discuss later in the chapter. But my bias is to take the informal interview as a central form of data in the early part of ethnography and use observation in a supplemental role. So let's begin with a discussion of the *informal ethnographic interview.* It's called "informal" for a variety of reasons. First, you don't have a written list of questions. Rather, you have a repertoire of question-asking strategies from which you draw as the moment seems appropriate.

It's also informal because you are not taking on the formal role of interrogator. The ethnographer is very much in the one-down position discussed earlier. He doesn't know enough to ask the appropriate specific questions. In this early dance, the informant takes the lead. The ethnographer's role is to look interested and suggest a couple of turns toward the other side of the ballroom so that he can check the view from there.

Finally, it's informal because it happens in many different situations besides a one-on-one isolated talk. You might ask informal questions while working with an informant on a harvest; you might ask during a group conversation over coffee; or you might ask while watching a ceremony. If used with tact, the strategies in this section can add to your ability to give accounts while doing minimal harm to the natural flow of events into which your questions intrude.

The general idea distinguishing formal from informal interviews is, again, the idea of control. In the informal, everything is negotiable. The informants can criticize a question, correct it, point out that it is sensitive, or answer in any way they want to. Compare this to a Likert-type item, where the social scientist defines a question and forces the informant to respond on a seven-point scale: "How close are you and your best friend?—1. Not close at all, 2. Somewhat close," and so on. There's quite a difference.

At the same time, formal versus informal is not a neat dichotomy. There are degrees of control that can be exercised. Whyte, for example, offers a typology of interview directiveness.[10] At the least-directed end of the scale are expressions of interest. You simply encourage the informant to keep talking by word or gesture. Next comes a simple reflection back of the informant's last statement. He says something, and you simply repeat it as an invitation to elaborate. Then there is a "probe" on the last remark. You ask some specific question about the last statement, inviting elaboration in a specific direction. And so it continues, through probes on earlier material up to an interviewer-requested change in the topic of the interview.

Beyond Whyte's typology of informality, even more structure can be imposed on the presentation of questions to informants. For example, in Chapter 8 structured ethnographic questions such as "frames" and "hypothetical situations" will be introduced. But that comes later, when you know enough to begin to more systematically document your ethnographic work. For now, we'll stay at the informal end of the scale.

[10] See Whyte (1960).

As you sit down with an informant, what do you talk about? Because the goal is to learn about informant accounts, it makes sense to start with informant descriptions of situations. It might be something you both saw occur in the flow of life. Or, you might begin by getting an initial characterization of a range of situations from an informant point of view. To do this, you might use interviews on the informant's daily routine, or you might begin asking for a life history. In either case, you begin to learn what sort of situations exist and how they are interpreted.

To give you a sense of how to formulate questions, we can talk either at the level of general guidelines, or at the level of specific question formation in response to what an informant just said. To discuss the first level, we again flirt with the boundary between theory and method. Until recently, most people who were interested in theories of talk worked with single sentences as the maximum unit. Now we are again in an epoch when there is interest in long sequences of talk and the relationship of those sequences to their social context. Any of this work can profitably be consulted for guidance in that particular sequence of talk called "the informal ethnographic interview." At this point, another book could be written on the methodological implications of recent work in sociolinguistics, discourse analysis, semiotics, and computer simulations of natural language. I'm not going to do that here, but let me take a well-known example and show you how it might work.

The example comes from Dell Hymes's work on the "ethnography of speaking." Hymes views talk as a piece of behavior in a situation that includes many other features as well. There is no way to tell what aspects of the situation will be significant, a priori, but Hymes offers a list of things to get you started.

The mnemonic for the list is represented by SPEAKING—setting, participants, ends, acts, key, instrumentalities, norms, and genre. Each aspect represents some potentially important part of a speech event, and by a bit of metaphorical extension, any social event at all. Before you start using it as an inspirational source of informal questions, note that you can apply the framework to analyze the informal interview of which you are a part as well.

Hymes's work must be consulted to understand the full range of possibilities.[11] It does not automatically generate specific questions, but it suggests areas within which you can creatively improvise some questions. Let's say you're standing with an informant watching two people work in a field. Just

[11] See Hymes (1972).

take the first two aspects of speaking. *Setting*—what kind of field is this? Who does it belong to? What are the special characteristics of this field, if any? *Participants*—who are those two men? Why are they here? Is there any reason why those two men in particular should be in this particular field?

Of course this, and any other, framework also generates questions that you can use in interviews when you're distant from the behavior you're talking about. Imagine you're talking to a street junkie and she says, "Yeah, and then I started going to the clinic across town." *Setting*—what neighborhood is the clinic in? How is the clinic designed inside? How is it different from other clinics the informant had seen? *Participants*—what kinds of junkies go there, or is it just frequented by junkies? Who is the staff, and are there different kinds? Again, *speaking* generates general questions that begin to range over the territory of how your informant accounts for events.

This is only one example of a framework for large chunks of talk representing a theoretical area with strong methodological implications for ethnographers. There are many others, and not all of them are from the social sciences. Both my parents worked in the newspaper business, and I remember their "five W's and an H"—who, what, when, where, why, and how. This guideline was intended to help reporters remember to get all the necessary information when they collected a story. If that suits you better during the early days of ethnographic work, then use that instead.

In addition to general guidelines such as these, there are some specific question-forming strategies that an ethnographer might find useful. The situation will arise when the informant just said something, but you need to learn something else, and one of these strategies can request an extension of the informant's talk from where he is to some unexplored territory. It's up to you to pick the opportunity and the strategy, or to develop new ones. Sometimes I think ethnography is to social science as jazz is to music.

Here are some examples of possible strategies. The items are neither mutually exclusive nor exhaustive, but they should give you the general idea.

Bait

Some social scientists worry a great deal about whether or not a question "leads" the informant. I've never understood what a "nonleading" question would sound like. It is not whether or not a question leads, but whether or not the ethnographer controls it enough to know where it is going. Besides, the informant may or may not follow. He at least has that option when the ethnographer is one-down or symmetrical in the relationship.

Sometimes it might be sensible to lead, and lead strongly.[12] Questions that strongly suggest an answer are called *baits*. They are particularly important,

[12] See Nadel (1939). He argues that for some informants, and for some topics, what he calls a "bullying" technique can be used.

because they are one way to *falsify* emergent conclusions in informal interviews. The ethnographer takes a statement made by the informant, especially when it fits his idea of what is going on in the community, and then does everything possible to get the informant to modify, contradict, or weaken the statement.

Here's an example from my work in New York. I'm talking with a methadone patient in a coffee shop. (Throughout the examples in this section, "I" will indicate "informant" and "E" will indicate "ethnographer.")

I: It's fucking ridiculous, man. Everybody on the street's doing methadone. Look at that dude over there, man, methadone and goofballs. I mean, you can't get a decent bag of stuff anywhere, man.
E: C'mon, man, there's got to be some stuff somewhere.
I: Yeah, well, maybe some of them rocks once in a while, but that shit's hard to find unless you know the right people.[13]

I should note that you've got to be careful about contradicting your informant this way. Where I worked, street rules allowed for such verbal assaults. With other groups, you might need to be more subtle.

This example, or a variant of it, happened several times in New York. I was interested in the way that methadone had become a popular street narcotic. It seemed to be replacing heroin. The informant in the example was going my way, so I tried to get some counterexamples. In this case, I suddenly became aware of "rocks." Rocks, it turned out, were a strong form of narcotic smuggled in by illegal immigrants from China (or so the story went). When I left New York, rocks were still increasing in popularity, as was heroin from Mexico and other sources in Asia. Though the "French connection" had been disrupted, and though the street scene had turned to methadone, it appeared that other sources were beginning to fill the gap. My learning about that all began with the use of simple baits to falsify the then common statement that "everyone's into methadone."

A couple of ethnographers actually did a study of baiting, or "leading questions."[14] They found that seven interviewers judged as good and experienced used leading questions 22–40% of the time. They also studied 20 student-conducted interviews and found that "proportionately more" leading questions elicited additional volunteered information, and that the amount of misinformation was the same for leading and nonleading ques-

[13] Given the sensitivity towards portrayals of "the other" in new ethnographic writing—not to mention sensitivity to diverse audiences—I might have toned down the profanity in the constructed examples in this chapter if I were writing them today. Then again, I might not have, since early seventies New York street junkie talk was in fact laced with such profanity as a matter of course. I mention this as yet another example of how much more problematic the matter of ethnographic writing is than it used to be in 1975.

[14] See Dohrenwend and Richardson (1964).

tions. There's not enough in the article to get a full sense of how they made those judgments, though.

At any rate, they went on to classify leading questions into three types: (1) When the question correctly anticipates the informant's answer, it gives an impression of friendliness and interest and encourages volunteered elaboration. (2) If the question is clearly incorrect, it gives the impression of a misinformed ethnographer and elicits corrected information. (3) If the question is partially correct or incorrect, it gives the impression of inattention and lack of comprehension, and the informant may fail to correct the ethnographer. This last type, they argue, is the "bad" type of leading question, since you don't know what's coming back.

Baiting, in short, is not a problem to pretend you're avoiding. It is a strategy to use to encourage informants to continue, or more importantly, to encourage them to disagree with what you think you've learned.

Distribution Checks

In Chapter 7 the problem of testing the distribution of patterns will be treated. Before we get to that, though, an ethnographer can get a check on distribution from the informant's point of view during informal interviews. Every language has so-called quantifiers, and such terms are among the first you should learn. Quantifiers, in English, are words like *none, a few, some, many, all.*

If the informant makes a statement that interests you, try to get him to estimate how many people in the group the statement applies to.

I: The other day, I was talking to a kid, the only thing he's ever done is methadone. He doesn't know anything about heroin.

E: Does that happen a lot now? [Or, "That doesn't happen much, does it?" or "How often would you say that happens?"—depending on whether you want to bait, and in which direction.]

I: Yeah, well, not a lot, but you sure never used to see shit like that 10 years ago.

Note that this question allowed for a distribution check with or without baiting. I wanted to show how strategies can be combined, though this is the only time I'll construct an example to make that point.

Folk Estimates

When asking for a distribution check, or when asking other questions requiring estimates of proportion or value on the part of informants, an ethnographer should try to learn folk methods for giving those estimates. Among the Lambardi, for example, I wanted to ask how well their crops had produced that year. I kept trying to get estimates using awkward questions like, "If your crop was perfect, let's call it 100. So how much of 100 did you get this year?" I got answers, but there was a long pause while informants fit their estimates into my unfamiliar framework.

Later, I learned the folk method of doing the same thing. Rupees are the main currency in India, and each rupee is divided into 100 *naye paise.* Not long ago, though, the rupee was divided into 16 *annas.* In fact, there were specific expressions for the values of 4, 8, and 12 *annas,* and people used the *anna* along with *naye paise* when they discussed purchases. When they made proportion estimates, they used the old *anna* system. I finally learned that when I overheard a Lambardi talking to another about his "12-*anna*" crop. From that point on, I had no problem with my question.

In matters of value, the junkies at Lexington hospital also had a specific way of evaluating material goods. Material goods were seen, logically enough, in terms of their monetary value. They would sometimes characterize goods in terms of how many "bags" they would bring. It was a popular game to look around my office and talk about how many bags my typewriter and tape recorder were worth. As time went on, I learned not to laugh quite as nervously as I had at first.

Contrast

This strategy is borrowed from linguistics. In phonology, for example, one learns something about how to characterize a unit of sound by comparing it to other units of sound to see if it differs. Compare *p* and *b* and the contrast is that one is voiced and the other is voiceless. The trick here is that the two items must be similar enough to compare. If I ask you to contrast *p* and *dog,* you would look at me strangely and say one was a word and one was a sound. You couldn't compare the two.

So you get two possible lessons with one question. Are the two items comparable? If so, then how do they differ? Here's an example, again from my research on methadone.

I: Yeah, so I was on methadone, but my partner was doin' speed.

E: What's the difference between methadone and speed?

I: Oh man, they're completely different things. Methadone is a narcotic, you dig, and speed is an up.

E: So what's the difference between methadone and heroin?

I: Well, the methadone comes on slower. You know, like there's no rush. And methadone's harder to kick if you get strung out. But the high is pretty much the same.

The first comparison taught me that I was asking about apples and oranges; the second got down to specific differences.

Self and Other

This type of question is really a specific form of a contrast question. It is singled out for special attention because it throws a switch that personalizes or depersonalizes a question. The strategy can go either way. For example, you might want to depersonalize some question that gets into a sensitive area. Instead of asking a Lambardi,

E: Did your wife ever leave your house and return to her father's?

You would do better, and be more tactful, to ask,

E: Does it ever happen that someone's wife would leave the *tanda* and go to her father's house?

A wife's departure is an important statement on her part, a call for help in arbitrating a situation that she finds intolerable. It happens sometimes, and people will talk about it, but it is an embarrassment for the parties concerned.

On the other hand, you might want to personalize other questions to learn if an informant's personal experience includes some of the activities being described.

E: If the "ideal bride" [*'cross-cousin'*] exists, can a boy refuse to marry her?
I: Yes, but he will be fined and everyone will be angry with him.
E: Do you know anyone yourself who actually refused?
I: No.

These are only two examples of where this strategy might be appropriate. The self–other choice is one you should always be sensitive to. It has attracted other social scientists as well. There are psychological scales, for example, that ask the subject to say whether some attributes are true of people in general as well as of himself. Among other things, a large difference might be used as a measure of alienation. Psychiatrists watch the self–other distinction as well. Often when a patient talks in sensitive areas, the pronouns change from *I* to the general *you* or *he/she*. There are other examples as well.

Linguistic Links: Part I—Ethnographic Semantics

There is a subfield in anthropology variously called *ethnographic semantics, ethnoscience,* and *cognitive anthropology.* It appeared in the late 1950s

and is now floating off in different directions as it merges with different issues in anthropology and linguistics. One of its concerns was, and still is among some, the structure of the lexicon—how are different lexically labeled concepts interrelated. Contrast is one of the relations talked about, but that was sketched earlier.

There are a variety of other relationships explored as well. Among them are the following: X is a kind of Y ("A cat is a kind of animal."); X is used for Y ("A knife is used to cut."); X is part of Y ("A piston is part of an engine."); and, as a final example, X is a stage in the process of Y ("To cook, you first have to pick out a pot."). Again, one should consult the literature to get the full picture.[15] The relations discussed suggest some strategies for question formation in response to items mentioned by the informant.

Here is an example illustrating a couple of the relations.

I: So then he grabbed his works and split, man.
E: What are works?
I: Works, you know, gimmicks, outfit [*is the same as*]. You use them to shoot dope [*used for*].
E: Are there different kinds of works [*kind of*]?
I: Sure, there's the regular rig and then there's a gun [*'regulation hypodermic syringe'*].
E: What are works made of [*part of*]?
I: An eyedropper, the bulb from a baby pacifier, a spike [*'needle'*] and some thread or paper for a tight fit.

Sometimes the informant made the links himself, and sometimes they were requested by the ethnographer. But in both cases, a knowledge of the relations is useful in the informal interview.

Linguistic Links: Part II—Logic

Many in linguistic theory are currently interested in explaining the logical relationships among sentences in a given language.[16] Although this is not the time or place to review the issues and explain the logical notions, let me give an example. If A "entails" B, then if A is true B must be true. To put it another way, A is a sufficient condition for B.

Entailment can be a useful tool in guiding the formation of questions. Supposedly, the ethnographer is putting the pieces together as he learns. New

[15] Some of the classic articles in the field are collected in two volumes—Tyler (1969) and Spradley (1972). I wrote a short monograph to introduce the perspective in introductory anthropology courses—Agar (1974).

[16] See Kempson (1977) for an introduction.

bits of knowledge are integrated into a growing structure that he is trying to build. Entailment is one way of checking if you've put the new piece in the right place.

I: At Mangya's daughter's wedding there was only one drummer.
E: He must be very poor. [*Ethnographer has learned that financial status is a key to explaining differences in wedding ceremonies, so he tests the statement that little money entails fewer drummers.*]
I: No, he just doesn't have many friends.

The ethnographer has just learned that lack of money may not entail fewer drummers (it is not a "necessary" condition)—he'll have to check that later. But he has also learned that social isolation has something to do with ceremonies as well, and this opens up a whole new area of questioning that must be pursued.

Linguistic Links: Part III – Case Grammar

In 1968 Charles Fillmore came up with a new way to represent the underlying structure of a sentence.[17] The verb is central, he argued, and hanging from it are a variety of slots, some of which *must* be filled, and some of which may be *optionally* filled. These slots stand in certain relations to the verb. One slot, for example, may require an "agent," a doer of the action encoded by the verb. Another slot may be optionally filled with an "instrument," a specification of the means through which the action was carried out.

Like everything else in this section, one should consult the literature. *Case grammar* has undergone many changes since its first appearance (in its new form) in 1968. But still, even in the simple form presented here, it suggests a strategy for question formation. By attending to the verb the informant just used, the ethnographer can run a quick check for unfilled case slots. If there are any, he can make the unfilled slot the focus of the next question.

E: So then what happened?
I: The dude was busted.
E: Who busted him [*agent—no pun intended*]?
I: This snitch named Joey set him up.

Or, to take another street junkie type example:

I: So I went ahead and opened the door.
E: How'd you do that [*instrument*]?
I: With a regular set of picks, you know.

[17] See Fillmore (1968).

Not only is case grammar useful in formulating specific questions, but it also can serve as a broader framework, like SPEAKING. The main event under discussion in the interview is represented by a verb or verb phrase, so case relations can guide you to get a complete account. In fact, many of the cases bear some resemblance, at this level, to the old five Ws and an H that newspaper reporters use to guide their interviews.

Frames

Though "frame" is an idea with different uses in different disciplines (not to mention in the streets), I use it here in the sense developed in cognitive anthropology. A *frame* is simply a statement with a hole in it that can be filled in a variety of ways. It is a more general strategy than those considered earlier. In fact, most any of the relations discussed in this section could be used to develop a frame. "Toys are used for _____." is a frame. Different informants might fill in the slot in several ways, and in ways that differ from person to person. For example, if an informant said "gifts," he would be responding in the relation mentioned in the discussion of cognitive anthropology. If, on the other hand, he said "having fun," he would be using the relation in the sense of the case relation "instrument."

An ethnographer should always be on the lookout for frames. They are perhaps less useful in immediate question formation, and more important for the systematic testing that will be developed later. Nevertheless, here's an example of one possible use: A Lambardi says, "You need a wife to wash your back." (I was often teased about that, being single at the time. It was said that I had the dirtiest back in India.) The frame that you quickly spot is, "You need a wife to _____." There are other possible frames—"You need a _____ to wash your back."— but they don't look as interesting. You make a note for possible use of the frame later, and then use it to form the question, "What else do I need a wife for?" If nothing else, you'll learn some folk wisecracks, an important attribute of your status as an ethnographer.

So there you have a sample of strategies for forming specific questions based on what the informant is saying. There is a wealth of additional suggestions in literary criticism, computer science, and linguistics, not to mention the social sciences. You may also come up with some that are group-specific as well. But to remind you of where we started, one begins at the informal end of Whyte's scale, expressing interest and reflecting back the informant's statements once she begins giving an account of some events. Then one uses more general frameworks, like SPEAKING, or the five Ws and an H, or Fillmore's cases to begin to get more details in the account. Finally, one gets pushier, using specific question-forming strategies to obtain details, learn connections with other kinds of accounts, and attempt to prove yourself wrong in what you think you've learned.

Actually, the process may not be this linear at all. As you sit listening, something bewildering may be said that launches you into detailed specific questions. No problem, as long as you keep a record of where you've been, so that you can come back later. In the long-term relationships established during fieldwork, there is plenty of time to ask questions that might have been missed earlier. There is no need to panic and try to get everything in a few sessions. That would be impossible anyway. If you're always asking questions, you'll drive an informant crazy and exhaust yourself.

The problem, and the challenge, of the informal interview is that questions are not prearranged in some linear way. Their use depends on where the informant leads you in conversation. An ethnographer should not self-consciously monitor informant statements, and then pause for a few seconds while he ticks off a list of alternatives, comes up with one that fits, and uses it to ask a question, for that would deprive the talk of its spontaneity and informality. The way out is for us to do more work on the structure of the informal interview, and then train students before they do fieldwork. It would be something like learning a limited language. At first the student would be self-conscious and awkward, but with practice the different strategies would become habit.

Finally, theese strategies are useful not only in forming questions, but in analyzing transcripts of informal interviews that have been tape-recorded and transcribed. These and other strategies can help you understand what you've done, even if you didn't know what you were doing at a conscious level at the time. Of course, not all of your talks will be taped and transcribed. Some of it will occur during an event that you are at least observing and perhaps participating in. (More on that shortly.)

But how does one interview if one doesn't speak the language? One of the most interesting results of my trip through the literature was the scarcity of discussions I found on the topic of translation. Many authors emphasize the importance of language competence in ethnography, and they also note the new problems that are introduced when one must rely on an interpreter.[18] Yet the literature is eerily quiet on the subject. I get the image of nervous ethnographers who are far from fluent trying not to bring up the subject.

That problem would characterize my own research. In South India, my Kannada was fair after a few months. I could pass the time of day and ask questions about things. But when it came to rapid speech or specialized topics (which requests for detail usually get you into), I was in trouble. And then the Lambardi native language is of North Indian origin. But since I had

[18] See, for example, Pelto and Pelto (1973), Paul (1953), Williams (1967), and many others, all the way back to Rivers (1913). Powdermaker (1966) talks about the problems in the one field setting where she did not know the language, as compared to the other three where she did. Naroll (1970) uses as one of his key measures of the "quality" of ethnographic reports the ability of the ethnographer to use the local language.

learned some Kannada before I left the United States, I worked in that. We were all operating in a second language. When things shifted into Lambardi, I was totally lost.

I relied heavily on two interpreters. The first was an ex-untouchable, Christian high-school graduate who sometimes worked with me, but he knew no Lambardi. The second was my cook, a Lambardi who figured out what I was generally doing. He would take a butchered Kannada sentence, add some intent, and rephrase it for me in either Kannada or Lambardi. In neither case was I as in control of the situation as I should have been, but I expect that's not unusual.

In Austria I was in a different kind of situation, using a language that I once spoke fairly fluently. By the end of my 6 months in a small Austrian town as a high-school exchange student, I was fluent in the role of obnoxious teenager. When I went to college, I took a year of additional German, so that by the time I stopped off to visit my Austrian family in 1964, I was babbling away, making wisecracks, and able to get definitions easily for words that came up that I didn't know.

But by the time I worked in the Austrian village, it had been about 7 years since I had used German. My language ability was returning quickly, though it never approached the previous level in the short time I worked there. I was able to work without an interpreter, though, but I was doing a much narrower task in Austria, working as part of a team to look at the question of bilingual semantics. The focus of the study was on the language itself.

That, by the way, is one of the reasons why I was attracted to ethnographic semantics in the first place. I think most ethnographers would agree that learning the language and learning the culture are far from independent tasks. In ethnographic semantics, the focus is on developing an explicit set of relationships between language and culture so that the study of language becomes a systematic exploration of the conceptual system that underlies it. The basic premise of the field—the learning of how people think about the world by attending to how they talk about it—pervades the methodological orientation of this book.

The problem doesn't disappear when you work in your own society. In fact, it is sometimes more complicated because the partial sharing of the surface forms of the language can create the illusion that you understand when in fact you don't. It's too easy to interpret informant statements into your own framework, missing the fact that the informant framework that produced the statement is quite different.[19]

Let me give an example from my work with junkies. Take the word *square*. When I first heard it, I thought it was a derogatory label for a socially acceptable, unadventurous orientation to life. That's the way I had learned

[19] James (1972) also makes this point.

it. In the next stage, I learned that as junkies used the term, it was not necessarily a derogatory label—its core meaning was "nonheroin user." There could be "hip" squares. But then as time went on, I learned more about the meaning of *square,* which derived from the contexts in which junkies encountered them. Squares were hypocrites, since they often frequented the streets to obtain goods and services that they ordinarily would say were illegal or immoral. Squares were larcenous, since their desires to get something for nothing made them a seller's market for stolen goods and a likely target for confidence games. Square was a complicated term to learn, and my initial belief that I understood it only got in the way.

Just to add more complications, consider a critique of ethnographic work mentioned earlier. In one article, an African anthropologist shows some key misunderstandings in a couple of classic works in the field. He argues that the misunderstandings are grounded in an inability to understand the vernacular. The few studies that I did find are impressive documentations of the amount of time and energy necessary just to get reasonable approximations in translations.[20]

I don't know how to resolve this problem. On the one hand, some prior language study is essential to get at least a basic idea of syntax and a rudimentary lexicon, even if only to have a basis for modification.[21] The problem also makes one year in the field look like a ridiculously short time to do ethnography. Finally, though I'm biased in this direction, it suggests that training in ethno- and sociolinguistics is a must for ethnographers. And it gives you a better understanding of why this book emphasizes the details of informant talk as a way to get at their interpretation of the world around them—to learn how informants give accounts is to learn the language. They become inseparable tasks.

Analysis of Informal Interviews

Once you have some cassettes full of talk, they must be analyzed. Under the general term *content analysis,* there are a variety of techniques for doing this. To take just one example, B. N. Colby codes folktales and then does a computer analysis of the thematic structure, looking for *eidons,* to use his term. Louis Gottschalk, a psychiatrist, codes clinical interviews for affect, then does an analysis with interest in the relation between affect and content of talk.[22]

[20] The article by the African anthropologist is Owusu (1978). The two studies of interview translation are Phillips (1960) and Werner and Campbell (1970).

[21] There are some introductory treatments of linguistic fieldwork, such as Lounsbury (1953) and Gudschinsky (1967).

[22] See Colby (1973) and Gottschalk and Gleser (1969).

These are only two sophisticated methods for dealing with informal interviews. There are others that ethnographers should familiarize themselves with as well. For now, I want to point out some obvious things to do with informal interviews, whatever your interest. The procedure described here only involves the analytical tools known as pencil and scissors.

In the initial stages of fieldwork, it's a good idea to transcribe tapes completely. Transcription is a chore—with a clear tape and a good typist, it might take around 6 hours for each hour of tape. Later, selective transcriptions may be adequate, but at the beginning, one should lay out the entire stretch of talk to see what one has. It goes without saying that the first thing to do is to read the transcripts in their entirety several times. Immerse yourself in the details, trying to get a sense of the interview as a whole.

Having done this, you, the analyst, will now seek to categorize the different segments of talk. The categories will allow you to pull different pieces of the interview—and pieces from different interviews—together into one place. The trick is to try and develop categories from the way the informants talked, rather than imposing a set from outside. There's nothing necessarily wrong with imposing categories—you may do that later. But that's not what the early phase of ethnography is all about.

The simplest way to begin is to take a pencil and go through the transcripts, marking off stretches of talk that cohere because they focus on the same topic. This is not an automatic procedure by any means. For example, you may be able to mark off statements at different levels of generality—topic, subtopic, and so on. Or there may not be a clear transition from one topic to another. But for the most part, ethnographers and informants—like everyone—orient their talk around some focus. They keep it there until one or the other changes that focus, and the change is usually obvious in the transcript.

As you go through the transcripts, you'll begin to notice that some topics recur. These recurrent topics are prime candidates for categories to code the information in the transcripts. You will need to give the topic some kind of name for your own purposes, and you can then go through the transcripts again and mark which sections fit the new category.

The goal is to come up with ways of marking off topics that cover as much of the transcribed talk as possible. Again, the method is hardly foolproof, but it allows for categories to grow from the way the informants talked during the interview. In almost all of my fieldwork, I've done this as part of the research, and it has never failed to teach me some important things about the pattern that underlies the talk.

Once the material is categorized, you apply the next analytical device—scissors—and cut up a copy of the transcripts according to the new topic-oriented code. Each group of talk can then be read to check for consistency within each informant and variation across informants. You can also see what was talked about and, more importantly, what was *not* talked about. With the luxury of time, you can also sketch out some other types of informal questions that you would like to use in each area in the next round of interviews.

At this point perhaps a specific example would help. When I began working in New York, I was struck with the frequent use of methadone as a street narcotic. My first strategy was to conduct several informal interviews—some with individuals and some with groups—in three different treatment centers where I had spent some time. I began by talking about how things had changed in the streets since my work at Lexington. Then I either expressed interest or used some of the questioning strategies discussed earlier.

After typing the transcripts, I did the kind of analysis described above. At one point, my apartment floor was covered with piles of butchered transcript. But as the topics emerged, they were remarkably similar to the analyses I had done of tapes focusing on heroin use at Lexington. In other words, ways of talking about methadone in New York were similar to ways of talking about heroin in Lexington in 1969. There were, of course, differences, but they were interesting as well.

In this particular case, the methodology was the message. The analysis suggested the continuity of the heroin subculture in the process of adaptation to a shift in the chemical ecology of the streets. The results of that analysis have been published elsewhere.[23] Though I didn't set out to code the transcripts the same way, the results can still be questioned on the basis of out-of-awareness influences on me from my past work. But then that's where the systematic check of my conclusions comes into play, a topic that will be treated in more detail beginning in Chapter 8. In fact, one of the specific examples used in that chapter will be a small survey done to check some of those conclusions.

But that's jumping the gun. For now, I just wanted to illustrate a simple procedure to begin dealing with the data of informal tape-recorded interviews. From this simple beginning, you begin building a map of the territory that will help you give accounts, and subsequently begin to discuss what "those people" are like.[24]

[23] For a discussion of continuity and change in the shift from heroin to methadone, see Agar (1977b).

[24] Most of Chapter 1 deals with the problem of analysis. Ethnographic software, for instance, models this cut and paste approach, and much of the new ethnographic literature critiques it. Sections in Chapter 1, mainly dealing with abductive logic and transcripts, also extend the discussion of analysis further than I could have when the first edition was written. We still have a long way to go, but we've also covered a lot more territory in this area over the last sixteen years.

Types of Informal Interviews

About now, the idea of an informal interview may be changing into an amoeba. The concept includes a variety of kinds of talk that the ethnographer engages in. On the other hand, the ethnographer may do nothing more than suggest a broad area and sit back while the informant talks for half an hour. On the other hand, the ethnographer may get downright pushy, interfering frequently in an attempt to steer the informant's talk in a particular direction. There is a rough correlation here with how much you have learned. The more you know, the more specific questions you can ask.

Earlier in this chapter, daily-routine and life-history interviews were mentioned. The daily routine is an old standby of ethnographic work. The basic idea is to get the informant to outline the events that he moves through on both typical and special days. Then, based on his outline, informal questions are used to encourage him to provide richer accounts of daily life. One version of this that I found in the literature is the diary-interview method. In this version, the ethnographer first works with informants to develop a guide for diary keeping. Then the informants are turned loose, recording the flow of events in some sample of their life. When they return, informal interviews are conducted and they expand on the diary entries.

In another type of interview, called *network serials,* the ethnographer was interested in learning from informants whom they encounter in daily life, in what settings, and in what kind of activities. With these three particular kinds of topics in mind, he conducted informal interviews with a sample of informants for a sample number of days. He then used the results to better understand the kinds of social networks that the informants were involved in in an urban setting.[25]

In contrast to the daily-routine interview, the life-history approach encourages the informant to talk about his or her own life in a chronologically ordered way. The life history has been more elaborately treated in the literature than the daily-routine interview.[26] Everyone agrees that life histories are important, but no one agrees on how they should be analyzed, beyond a simple presentation (edited in unknown ways) of the informant's narrative. For our present purposes, it is sufficient to suggest that the approach is a

[25] The diary-interview method is described in Zimmerman and Wieder (1977). Network serials are discussed in Sanjek (1978). For another discussion of diaries, see Axinn and Axinn (1969).

[26] See, for example, Allport (1942), Gottschalk *et al.* (1945), Dollard (1949), Mandelbaum (1973), Langness (1965), and Watson (1976). The Langness reference is particularly good for a review of the literature and a historical sketch of the uses of life histories, together with a methodological critique.

useful way to begin obtaining informant accounts of events, because that's the data we're after at this stage.

Another kind of informal interview is based on the use of videotape recording equipment. The use of visual documentation in ethnography is something that I have neglected here. I have no experience in the area, and the field has mushroomed into a subdiscipline with its own professional society. But one application that fits into the methods being discussed here is the use of videotape to elicit informal interview material. A recorded event is presented to informants, singly or in groups, as the basis for interviews focusing on their interpretations. There are several advantages to this way of relating observation and interview—the observational material is the same in each interview, and the wealth of detail is preserved in the recording.[27]

There are of course other kinds of informal interviews. You might sit down with an informant to talk about some recent event that you observed. Or you might have learned of a particular topic that you want to explore with somebody. Or you might make a compilation of categories of activities, like the codebook of the Human Relations Area File (HRAF) or the guide to fieldwork known as *Notes and Queries,* and work up informal interviews based on those.[28] But, as promised earlier, the purpose here is to talk about doing ethnography in general rather than providing an exhaustive catalog of methods that have been used elsewhere.

Interviews and Observations

Now it's time to come to an issue that anthropologists love to talk about—the relationship between what people say and what they do, or the relative importance of talking to informants as opposed to watching them. Imagine a world where only two positions are possible. If you take the first position, you would argue that the only way to learn about people would be talk with them in private in an isolated room. If you take the other, you would not listen to anything people say about what they do, but just observe their movements. Ridiculous choices.

Yet sometimes anthropologists have arguments where these seem to be the corners they work themselves into. Every ethnographer, in saner moments, realizes that you take what you can, especially in the early phase that is of concern to us now. Most would also agree that sometimes people do

[27] For sample introductions to visual anthropology, see Heider (1976), Hockings (1975), Scheflen (1973), and Collier (1967). The use of videotape as an eliciting device is discussed in an essay by Bellman in the Hockings (1975) volume.

[28] Williams (1967) discusses this strategy. The field guides are in the References as Murdock *et al.* (1950) and Royal Anthropological Institute of Great Britain and Ireland (1951).

what they say, and sometimes they don't, and that, at any rate, one needs both kinds of data. But my bias is to take talk as the primary area for systematization, and use behavior as input into talk and as a test of the result of talk. Before I get into that, we need to clarify some confusion about the dichotomy of talk versus behavior.

Aren't people behaving when they talk, and don't they talk when they behave? Of course, yet in some discussions this problem is ignored as if everyone knew what they were "talking" about. The real problem, I think, is whether the talk is about something remote in space and time, or whether talk is part of what's currently going on.

Perhaps we should fall back on the distinction discussed earlier between language as object language and language as metalanguage. But then what if I write this: "It's a bigger job than I thought writing this book." Is that a comment on the book, part of the book, or both? And is the comment I just made a comment on the comment, or part of the book, or both?

I know intuitively that there are important differences in the sources from which an ethnographer learns. I also know that if you watch people doing things, you learn something you can't get by just talking with them, although you can't learn much unless you do talk with them before, during, and after the event. I don't mean to ignore the problem, if only I could clearly define it.

I think it might be more profitable, in the underdeveloped state of ethnographic methodology, to take the transcribed informal interview as the methodological core, and then ask how other kinds of things ethnographers do relate to it. These other things, then, either provide material on which an informal interview is based, or provide natural experiments for the testing of things analyzed from those same interviews.

Let me give one example. You go to a couple of funerals in South India, and you notice that all the bodies point in the same direction. Later, you ask a couple of Lambardis who are sitting and talking with you why that is. They say, "Because Hanuman is in the south." You begin to ask more questions, and you learn that in the epic, *The Ramayana,* Hanuman did indeed wind up in Ceylon (now Sri Lanka). You put this together with your earlier observations in the Hanuman temple, and you begin to realize that the Lambardi have a special relationship with Hanuman that you must learn more about. Without this simple observation, you would probably never have thought to ask the question, and it would probably not have been volunteered by an informant discussing funerals.

Notice that this observation fed informal interviews in two ways. First of all, it suggested something to ask during the ceremony itself. If it had been appropriate, you might have turned to someone next to you and asked why the bodies pointed south. Before you ask questions during the event, though, you must be sure that the asking does not disrupt the very event you are observing. Sometimes informants would motion me to be quiet, or give a curt answer and turn away.

But there is always time later. In this case, I brought up the subject again in a conversation that was going on anyway. I might also have asked an informant for an interview and taken him into my house and turned on the tape recorder. This would have allowed for a more uninterrupted focus on the funeral and Hanuman, and might have freed the informant from immediate social pressure so that he could say some things that would have made him uncomfortable among his peers.

Let me now use some junkie experiences to illustrate the other role of observations—to test the results of interviews. When I worked at Lexington, I was particularly interested in the study of events. I wanted to learn the junkie theory of events by attending to how they talked about them. One event that I heard discussed was "hanging out." By listening and later asking questions, I learned that hanging out was a filler, a sort of nonevent while waiting for other things to happen. The middle-class equivalent, I thought, would be like two strangers waiting for a plane—"Where you from? What do you do? Rotten weather, isn't it?" And so on. I ignored it in favor of other events like copping and using heroin.

When I worked in the streets of New York, I found myself actually hanging out with junkies on street corners and in coffee shops. They were usually waiting for something else to occur, but hanging out served many useful purposes. While they hung out, they exchanged information critical for "getting over" on the streets. Hanging out served as a context for talk of news. Who had the good stuff today? Where was so and so? Jack just got busted. Rick is back on the streets. "The man" is hanging out over by a particular dealer's place right now. The street scene was a social environment that could change rapidly because of outside pressures beyond one's control. Hanging out was used to keep posted on the current state of that environment.

In this case, then, the observation in the streets corrected the account I received in the hospital. To pursue an understanding of the event, I now needed to talk with more informants, observe some more, and so on through the dialectic until I felt I could give a more thorough account of the event. I did not do this because I was focusing on other topics. There is always that problem—the more you zero in on one area, the more you must neglect others.

So, observation and interview mutually interact with each other, either simultaneously or sequentially, in the course of doing ethnography. Perhaps this frequent mutual interaction is one reason why they're so difficult to separate. Following my own biases, I would advocate making talk the central source of data in informal work and pulling in other data to interact with it in a more dependent role. Other ethnographers would strongly disagree with that bias. The test for all the biases, though, will be how well key statements are supported in later, more systematic checks of the informal part of the research. That's a topic we'll take up in Chapter 7.

To sum all this up, then, observation is critical in enriching our ability to give accounts of events. Informants, giving accounts in interviews, may leave things out.[29] They may do so for any number of reasons. Informants, like ethnographers, have their "personal equation" that influences how they see things and what they report. Within his relationship with an ethnographer, an informant may judge that certain parts of an event account should be left out while others should be stressed. Some details of the account may be left out as "unimportant" or "obvious" when, in fact, they represent important things for the ethnographer to learn. The informant may just flat forget some details, or perhaps be misinformed, or, on the basis of limited experiences, give an idiosyncratic account.

The informant may be giving an account that is covered in part by the rules of the group that apply to account giving. In one group the rule might be to downplay one's personal wealth, so in an account from an informant, you don't hear that a lot of what's going on has to do with his position as a wealthy member of the community. On the other hand, in another group the rule might be to stress personal control over others, so you get an account that makes it sound like the informant engineered and directed the entire flow of events. Or the informant, after repeated contacts with you, may become trained into your way of looking at situations, and give you accounts that fit your own developing ability to do the same. Or he may just decide to put you on.

Notice how much more complicated this all is compared to the discussions of "reliability" and "validity" in the hypothesis-testing literature. In many of the ethnographic discussions that deal with differences in informant accounts and observations, the differences are not seen as a problem to be eliminated. Whether they are called problems of "reliability," "error," or "lying," the tone is that differences between reports and behavior are a normal part of human interaction. Just because one human is an ethnographer and the other is an informant, there is no reason to expect it not to occur. The problem is to understand it, not to pretend that it doesn't happen, and then to include in your methodology some procedures for dealing with it.

[29] For related evaluations of informant reliability and precision, see Young and Young (1961) and Poggie (1972). For discussions of lying, see Passin (1942) and Salamone (1977). General discussions of informant error in ethnography are in Dean and Whyte (1958), Whyte (1960), and Vidich *et al.* (1964). Cancian (1963) shows how informant error can be used to demonstrate the correctness of an ethnographic statement. Killworth and Bernard (1976) compare informant statements with communication data to look at accuracy in reporting network data.

As an ethnographer, your involvement in personal relationships with informants and your participation in community life will provide you with rich information to evaluate informant interviews. As time goes on, you will begin to learn about idiosyncratic and systematic differences between informant accounts and actual events, and it will largely be your observations that enable you to do so.

Because I'm advocating interviews as the core of ethnographic fieldwork, and because there are other ethnographers who would argue with this, it might be well to consider the other side of the question, though with a view towards supporting my bias. As already noted, most ethnographers use a blend of interview and observation, and many would agree with the kind of strategy advocated in this book. But others would emphasize the data of direct observation as an end in itself.[30]

John and Beatrice Whiting have done an interesting piece discussing situations where systematic observation might be necessary.[31] In some cases, they note, informants may be unable to give relevant information in areas of interest to the ethnographer. Further, ethnographers may want some kind of standardized data from a variety of different settings. They then go on to illustrate the development and use of a scheme for the direct observation of children's behavior.

Even here, though, they note the importance of a sense of the community as a prerequisite for the design of a successful observational study. And they caution against trying to record everything. They also note the need for other "measures" of the sorts of things one obtains from observational data, and conclude that systematic observation is "laborious and time-consuming." They advise doing it only if informant reports are not reliable.

Other ethnographers have also developed observational schemes to systematically record ongoing behavior.[32] But again, a charting of motion through space without a sense of the meanings of those motions to group members doesn't seem to me to be what ethnography is all about. A major way to learn those meanings, especially in the early stages of ethnography, is to ask people what they are about. It is for this reason that I emphasize

[30] Harris is one of the better known critics of what he calls "mentalist" positions. In his book (1964) he attempts to develop a scheme for the description of the structure of behavior without recourse to the meanings the participants assign to it. In an article (1974), he argues that an ability to give accounts like an informant does not mean that you can describe all the things that happen in an event.

[31] See Whiting and Whiting (1970).

[32] For example, see Guest (1960), Hall (1963), and Wolff (1960). Webb *et al.* (1966) in a related vein, advocate the use of "unobtrusive measures" to study human behavior without actually interviewing informants. Even here, as they note, the development and interpretation of those measures depend on a prior understanding of group perceptions of their world.

informal interviews as the core of ethnography, with observation in a supplemental role.[33]

Field Notes

Field notes are considered the traditional core of data from ethnographic research. Usually, a discussion of participant observation, field notes, and informal interviews constitutes field methods training in anthropology. Field notes are the record of an ethnographer's observations, conversations, interpretations, and suggestions for future information to be gathered. They are so central traditionally that often anthropologists refer to analysis and publication as "writing up your notes."

In my opinion, field notes are the most overrated thing since the Edsel. Part of the reason such significance is attached to them, I think, is because they are all we have. Yet in my own fieldwork, I rapidly became disillusioned with the idea. Let me use some experiences to tell you why. By the time I first went to India, I had heard the professional folklore about writing in your notebook. For example, one story has it that an anthropologist who did not want to write in front of his informants would learn something and then run off into the bushes to scribble furiously. No one said anything, but eventually the folk herbalist visited him with a cure for diarrhea.

That gets us to the first problem. Because you do not yet know what is significant, you don't know what to record. As we'll see shortly, since there is a problem with memory, it is best to write things down as quickly as possible. But it takes much longer to record everything you can remember about what just happened than it did for it to happen. In one field methods book, it is suggested that the ratio of recording time to participant observation time is six to one! So while you're scribbling furiously, other things are going on that represent the continuity between when you left and when you return.[34]

Of course you can write everything down at the end of the day, but then you must contend with long-term memory problems. A colleague of mine once held a graduate seminar where he decided to do some ethnography of remembering. One student had four friends who were starving graduate students. They saved up their money for a month, and then went to dinner at a very fancy restaurant.

[33] There's nothing new here. Many earlier statements emphasize the interaction between interview and observation, as in, for example, Nadel (1939) and Paul (1953).

[34] Among others, Wax (1971) and Williams (1967) both suggest that early notes are not worth much. The suggested six to one recording ratio is in Bogdan and Taylor (1975).

The student waited until after they had gone out, and then she interviewed each separately about the meal. They all described the *maitre d'*, the waiters, the piped-in music, the potted palms, and so on. But when the ethnographer went to the restaurant, the *maitre d'* was a woman, the waiters were waitresses, and there was no music, nor were there potted palms. But it was a *very fancy restaurant.*

The problem is that long-term memory recall often produces distorted results. And the distortion is not random, but rather alters in a direction away from the details of the specific event and towards the more general stereotypical conceptualization of that event. What the four diners "erroneously" remembered were some details from their shared idea of a very fancy restaurant.[35] This kind of primary method of recording is far from desirable.

Field notes, then, are a problem. In their worst form, they are an attempt to vacuum up everything possible, either interrupting your observation to do so or distorting the results when retrieving them from long-term memory. Not that you shouldn't keep notes, but they should be more focused in topic, and they should eventually be made obsolete.

The more specific focus indicates that you are no longer trying to capture everything. At any given time during your early informal fieldwork, there will be a couple of topics you are focusing on. Center your informal interviews, conversations, and observations on those topics. When you are not learning anything new, you are ready to move on, unless something remarkable occurs to cause you to reevaluate what you think you learned. Or, you may choose this reevaluated topic as one of the few you will later research more systematically. Of course, no one would advise ignoring everything that is not related to the current topics of interest. That would be destructive in the unique openness of informal fieldwork. When something interesting appears, *note* it. But don't lose the focus of the topics currently under consideration.

Field notes will consist, according to this scheme, of two kinds of things: First, some ideas from observation to follow up with interviews, or some observations/questions to follow up that came from interviews; second, some things you've noticed that you want to be sure to get eventually. Field notes, then, are *working* notes. They are a step on a ladder used for an ascent towards an understanding of some group. Once you've used a rung to get a little higher, you don't need it anymore. Observational and interview material centered on a particular issue can be gathered into working papers as a first step in the analysis. At least you won't have boxes full of stuff that you will never look at in 100 years, except for nostalgia.

[35] D'Andrade (1973) has published an article documenting and discussing this problem. His work is also discussed in Johnson (1978).

On the other hand, a personal diary is a record of another sort. Diaries focus more on the reactions of the ethnographer to the field setting and the informants, the general sense of how the research is going, feelings of detachment and involvement, and so on. It is just this sort of material that goes into "personal accounts," bringing the ethnographer's role more explicitly into the research process. Personal diaries would profit from more careful development as an ethnographic method in their own right.

Ethnographic Goals Revisited

Before moving on, it would be good to step back for a moment from all the talking and watching and get a sense of how it relates to the ethnographic goal discussed in Chapter 5. Though used in a variety of ways, perhaps the best cover term for all this activity is *participant observation.*[36] The term suggests that you are directly involved in community life, observing and talking with people as you learn from them their view of reality.

As I've repeatedly warned, though, the preceding discussion has not inventoried all methods that one might use in this initial participant observation phase. For example, in some traditional communities kinship is an important system for organizing social relationships. Because of kinship's traditional importance, there is an abundance of literature on working with genealogical material. There is also another, more recent perspective on social relations that involves the study of networks.[37] But in spite of these and other holes in the discussion, you hopefully now have a sense of the kinds of activities that ethnography involves, together with a sense of what these activities are intended to accomplish.

But still there has been no sketch of the process through which all this activity accumulates into an ability to give accounts of events as informants do. I don't feel too bad about this because this higher level analysis of ethnographic materials is difficult to explicitly discuss—there haven't been too many attempts in the literature.[38] In fact, in the first draft of this book, I

[36] The notion of participant observer has been elaborately discussed in the literature. See especially the reader edited by McCall and Simmons (1969). Much of the literature discusses the nature of the research role, with variations depending on the amount of involvement. See Gold (1958), Furk (1955), Jones (1961), Junker (1960), Kluckhohn (1940), and Becker and Geer (1957).

[37] For discussions of the genealogical method, see Barnes (1947), Conklin (1964), and Schusky (1965). For an introductory overview of network approaches, see Whitten and Wolfe (1973).

[38] For general statements of ethnographic analysis see Weakland (1951), Wolff (1945), and Bruyn (1966). Some of the more specific discussions of grounded theory and analytic induction will be taken up in Chapter 7.

jumped from the discussions of interviews and observations into the next chapters, where systematic checks of what you think you've learned are suggested. The implicit message was, "Well, you do all this, and then you sort of get some ideas about how things work, so you test them out."

Perhaps much informal work is right-hemisphere activity that's difficult to translate—the intuition and pattern perception that is another part of the ethnographic mystique. Or maybe it's a series of procedures that have just not been made explicit yet. There is a history of change from intuition to explicit analysis in the sciences in general, so perhaps right now we're in the midst of that process in ethnography.[39] At any rate, although some of these issues will be discussed under "culture theory" in Chapter 10, it won't hurt to think a bit about them now.

The first issue has to do with all the bouncing around between accounts and events. An informant may be inconsistent in the account she gives; then two informants may disagree in their account of the same event; finally, there may be discrepancies between an informant's account and your observations.[40] The first thought to keep you from going over the edge is that all of these are normal. Informants, as mentioned earlier, bring their own personal equation and notion of your relationship into interviews, and the hidden agenda in all of that can produce problems in their descriptions. Besides, their inconsistencies may reflect an honest effort to report variation in the events being discussed. Then two informants can be expected to disagree on some counts, but then again there should also be some agreement—a shared framework within which two informants, and eventually the ethnographer, can talk about events. Finally, who in the world could report all the details of an actual event? If your observations don't teach you something new, you're probably not paying attention.

My own goal, as I wander through the maze, is to build some kind of theory of events. The test of the theory, in informal ethnography, is that it should allow you to take a particular slice of behavior and interpret it by putting it into context. In other words, something occurs, and you are able to talk with informants about what it is and how it is related to an ongoing event and other events to which that event may be related. The theory you put together may be nothing that you obtained directly in any interviews, but it should serve as a resource for you to talk with people in the group about "what's going on."

The next step, in my view, is to take the ability to give accounts, define that as data, and look for patterns across events. Say I can talk to junkies about copping heroin and also about avoiding a bust. Are there common

[39] Margaret Mead (1969) makes this point.

[40] Paul (1953) outlines these three basic problems in checking out ethnographic data.

patterns in the way I have learned to give accounts of those two different events? I call this process of searching for common principles that cross events and define the context in which accounts occur a *concern with themes*.

Then you can take events and themes and turn *them* into data. Why do folks interpret reality like this? What circumstances gave rise to and continue to maintain such a view? Is it a human universal, a product of group history, a response to some aspects of the physical, biological, or social environment? The search moves to a higher level as a context is sought for events and themes.

Now I'm getting way ahead of myself. Besides, my version of these three levels of analysis—*events, themes,* and what, for lack of a better word, I'll call *adaptation*—is clearly just one way of looking at ethnography. Besides, we are now struggling with the notion of "culture," and that will be discussed in Chapter 10. But let me at least give a brief example from my Lexington study. This isn't the best example, because I was working a place remote from the actual events I was interested in, but it will hopefully give you the general idea. When I first arrived in Lexington, I spent time in the patient's situations—the living unit, the dining halls, therapy groups, social activities, and so on. My first observation was that although we spoke the "same" language, junkies talked about events in ways that I didn't understand. Their accounts of things were different from mine.[41]

I narrowed the focus to some street events, and then did informal interviews about them. Because I was distant from these actual events, we invented a way to "simulate" them to get some observational data. (All of this is discussed in detail in Chapter 8, so I won't repeat it here.) By going back and forth between interviews and events, I built a theory of the nature and interrelationships of at least some of the key events in junkie street life.

Although I didn't systematically study themes, I did speculate on some. For example, I noticed that a concern with the background of social others was something that might be talked about in a variety of event-specific discussions. Whether it was buying or using heroin, or being busted or ripped off, the interpretation often included some consideration of the question, "Who are these people involved in this event?"

Next I speculated on the adaptive nature of that theme. In the junkie street environment, there are no institutions dedicated to your protection. When you encounter people, you must evaluate the likelihood that they are *informants* (that word in a new context), people who will cheat you, or people who will pull a weapon and take your money or heroin. In the social

[41] Looking back from 1996, the discussion of giving accounts, themes, and adaptations, though still adequate for introductory purposes, is perhaps the weakest part of the old *Stranger*. Chapter 1 picks up where the first edition left off.

environment of street junkies, the theme is useful if you are to survive in the streets.

Although I stopped there in the Lexington study, the analysis could have continued. For example, I might have turned the environment into data, and tried to figure out where drug policy came from. In fact, there is some recent work in the history of narcotics legislation in the United States that addresses just that issue. Or, because junkies are not born into the group, I might have wondered more about their socialization as a personal adaptation to circumstances.

At any rate, you will see much more of the Lexington study in Chapter 8. My purpose here was to offer a sketch of my version of the ethnographic process. In this chapter the focus has been on that critical first phase of learning to give and understand accounts. I'm not sure what to tell you about looking for themes. That's a problem I'm interested in right now, and another example in Chapter 8 will deal with some of that work. But early participant observation is critical—it is the bedrock on which the rest of your research will rest. It is because so much of social science research skips this phase that there are so many ethnographic critiques of hypothesis-testing research.

So, you're off and rolling. You are suitably cautious about the people that approach you to talk early on. In your conversations and informal talks over a tape recorder, you draw on some strategies that help you form questions to lead into new areas. The role of observations and field notes has been clarified somewhat, though you may very well take issue with the formulation given here and put together your own.

After some months of this, your language skills should be in fairly good shape. Your ability at giving accounts should be quite good at this point as well. When you hear or see something about an actual or reported event, you should be able to comment in a manner that makes sense to group members. Now it's time to take stock. The materials gathered during the informal work need to be gone over again, and you need to make some choices. In the limited amount of time left, you will probably want to fill in some important gaps informally. But you can also pick some of the key areas of what you have learned and document them more systematically. And that leads us into the next chapter.

7

Narrowing the Focus

Narrowing the focus of an ethnography can be a painful operation. By the time choices must be made, you are probably nearing that arrogant stance, partly grounded in truth, that no one understands "these folks" like you do. This is the "my people" syndrome viewed a bit more sympathetically than it was earlier in the book. There are undoubtedly dozens of things that you would like to pursue in more depth. But if you are committed to the goals of social science, you *must* narrow the focus.

One goal is the presentation of evidence to show a skeptical outsider why you have come to certain conclusions. You have learned a great deal during the informal work—some of it in a way that you can display effectively. But much of what has been learned comes from insight and intuition. You may be willing to bet a large amount of money that you are correct, but why should someone who has not shared the experiences believe you? If you are going to "do science," you must offer the outsider some way to evaluate your statements.

Sometimes the task may seem dreary because you already know how the more systematic procedure will come out. On the other hand, you may be unpleasantly surprised. If you design the systematic test correctly—so that the statements you are checking can be proven false—you may learn that your understanding was not as complete as you originally thought. Before worrying about the form taken by systematic tests, though, some thought needs to be given to the people with whom the check will be done.