

Planning the Project and Entering the Field

Using anthropological methods, unless we are entering an organization or community well known to us from previous research, the initial phase of the project should be considered a social exploration. We learn what we can in advance about this relatively unknown territory, but once we are there, the first requirement is to gain some initial familiarity with the local scene and establish a social base from which we can continue our exploration until we are able to study some parts of that territory systematically.

This style of research has both positive and negative aspects. Like the explorer of physical terrain, you run the risk of getting lost and never coming out with a coherent map of the territory. On the other hand, the flexibility of the methods offer the possibility of making discoveries far more valuable than you could have anticipated.

The planning process is begun but not completed before the researcher enters the field. I am not proposing that we enter with blank minds, leaving it to subsequent observations and experience to shape research plans. Striving for such a state of unconsciousness would be folly, but it is important to avoid the other extreme of becoming so fixated on a previously prepared and detailed research design as to miss opportunities to gather data about problems that may turn out to be more important.

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In *Barrios in Arms*, the researcher ended up with a more valuable study than that originally planned. However, José Moreno did not have to choose between following his original research design or studying an attempted revolution. The decision was made for him by events.

The more difficult problem arises when there is a choice: It is possible to carry out the project as initially planned, but the researcher encounters a new problem and perhaps also a new set of data that could lead to a more important study.

That was Wesley Craig's problem in his study in the Convención Valley on the eastern slopes of the Andes in Peru. Craig set out to make a study of intervillage systems. Here a peasant movement had recently accomplished an extraordinary social and economic transformation, overcoming the domination of the large landholders and establishing independent and autonomous villages. Craig was aware of this before he entered the field, but he had decided to focus on the current scene rather than upon a reconstruction of the dynamics of the peasant movement. On a visit to the provincial capital city of Cuzco, he accidentally discovered a storehouse of documents in an unguarded garage. In discarded cardboard boxes were files of grievances against the landlords of the haciendas by the workers who, at the beginning of the protest period, had been subjected to conditions close to serfdom. These were documents presented to the provincial labor authorities in Cuzco, and they appeared to be complete from the first grievance filed years earlier through an increasing flow of grievances culminating in the peasant movement. Each grievance identified the landlord and gave the name of the hacienda and the names of the complainants. With this set of documents, Craig was in a position to trace the process of peasant organization through time and in its geographical spread. The documentation of the names of the grievors, of those representing them, and of the landlords also provided rich data that could readily lead the researcher to key informants for their accounts of particular cases and of the process of peasant mobilization.

When I received Craig's letter informing me of his find, my first impulse was to urge him to forget about intervillage systems and concentrate on reconstructing the peasant movement. However, I was only a minor committeeman on his thesis. I therefore consulted with Frank Young, who was not only chairman of Craig's doctoral thesis committee but also the inventor of the theory and methodology underlying studies of intervillage systems. Young assured me that he would approve the change, and I passed on this recommendation to Craig. He replied that he had decided against abandoning the intervillage systems study and would do both projects at the same time. The intervillage study was well

done, but it hardly made an important contribution to the research literature.

Of course, it is never possible to prove what might have been, but the Craig thesis (1969) did not fully exploit the potential values of the peasant movement project.

Several years later, Eduardo Fioravanti (1974), a young Spanish sociologist, entered the Convención Valley to spend a full year devoted entirely to field work on the peasant movement. In general Fioravanti's analysis supported the interpretation earlier presented by Craig, while pointing out some errors on minor points, but Fioravanti had been able to document the case in much richer detail. Thus it was not Craig but Fioravanti who published the definitive study of this peasant movement. The moral of this case? When the field situation reveals opportunities to do a more valuable study by changing the research design, seize the opportunity—and don't compromise by making the dubious assumption that you can exploit the new opportunity fully and at the same time pursue your original research design.

ENTERING THE COMMUNITY

The entry process differs according to whether one studies a formal organization or a community. The organization has official gatekeepers who control access. The community has unofficial gatekeepers who can either facilitate entry and encourage access to information or see to it that the researcher never penetrates beyond superficial acquaintance and the formal portrait of themselves the people would like to give to the outside world.

Entry strategy depends in part upon whether you plan to study a whole community with all of its social classes, ethnic groups, associations, neighborhoods, and so on, or whether the study is more narrowly focused to gain a more intimate view of a particular segment of that community.

If you aim to study a whole community, the most open points of entry are among those who share your social class background. Since university-based researchers come from upper- or upper-middle-class backgrounds, or are moving into the upper middle class through higher education, this means that contacts will be established most readily with business and professional people. But not all contacts at a given level are of equal value. The researcher at an early stage tries to identify those in leadership positions in the hope that they will provide useful contacts and even informal sponsorship.

Having gained the acceptance of some key people, the researcher then attempts to participate in ways that establish an acceptable per-

sonal identity, making it possible to move beyond the limits of the initial sponsorship. The researcher then faces the question of deciding between broad and necessarily somewhat superficial coverage of the community and a narrower but more intensive study of one or more of its segments. (Chapter 12 contains a discussion of the depth versus breadth issue.) How much time the researcher can devote to the project and how many field workers are involved will determine to what extent it is possible to achieve both depth and breadth.

The Middletown Studies

In their pioneering study *Middletown* (1929), Robert S. and Helen Merrill Lynd moved to Muncie, Indiana, a city of about 38,000 population, opened an office, and lived there from January 1924 until June 1925. Their secretary worked with them for the entire period, and they had two assistants, one for a year, the other for five months. For the restudy (Lynd and Lynd, 1937), Robert Lynd returned to Muncie in June 1935 with five assistants for "less than a tenth of the man-days of research time" (p. 4) of the original study, which suggests that the later field period was about ten weeks. Of course, the first study had provided a baseline and a wealth of information, which made it possible to work more rapidly and efficiently in 1935.

Before entering the field in their initial study, the Lynds (1929: 4) had determined the broad outlines of the project:

getting a living
making a home
training the young
using leisure in various forms of play, art, and so on
engaging in religious practices
engaging in community activities

The six parts of the first Middletown book faithfully reflect the initial study outline. Since they aimed to cover all social classes and ethnic groups under each of these six headings, this necessarily limited their depth of penetration for any segment of the community. Even though their field time in the restudy was much shorter, they achieved much greater depth on one element with their chapter, "The X Family: A Pattern of Business Class Control" (pp. 74-101). It is interesting to note that it was local informants who guided them in this direction. "Since *Middletown* was published, some local people have criticized it for underplaying the role of the X family in the city's life" (p. 74). If they had

begun research in a more open, exploratory manner, they might have recognized the dominance of the X family in the first study.

Yankee City

W. Lloyd Warner did his first field study on a primitive tribe (Warner, 1958), but, as he reported later:

My fundamental purpose in studying primitive man was to know modern man better; . . . some day I proposed to investigate (just how I did not then know) the social life of modern man with the hope of ultimately placing the researches in a larger framework of comparison which would include the other societies of the world [Warner and Lunt, 1941: 3-4].

This suggested a more intensive analysis of kinship, social structure, and formal and informal organizations than the Lynds had undertaken. The community chosen, "Yankee City" (Newburyport, MA), with about 17,000 population, was less than half the size of Muncie, but still the attempt to achieve substantial depth and breadth was a formidable undertaking.

After describing the process of acquiring background information on the city, Warner gives this description on his interpersonal entry strategy:

It seemed highly advisable to secure the consent and cooperation of the more important men in the community lest we later find it impossible to obtain certain vital information. We finally selected one prominent and, it later developed, much-trusted individual who we knew was important in the town and who, we believed, might be interested in the work we proposed doing. We obtained introductions to him, told him in general what we wanted to do, and asked his cooperation. After asking us a number of questions and showing a decided interest in our work, he agreed to help us in any way he could. We then asked him to introduce us to some of his friends who were leaders in the city's activities. This he did, and from his friends we received other introductions which shortly spread our sources of information from the top to the bottom of the city [Warner and Lunt, 1941: 41-42].

Warner does not tell us how he obtained the initial introduction, but it is not hard to imagine how this was done. Newburyport is not far from Harvard, where the research program was based, and many of the leading citizens of the city were Harvard graduates. There must have

been a number of potential social bridges from Harvard into the Newburyport elite.

Warner does not tell us how coworkers went from one contact to another "from the top to the bottom of the city." However, this was a research program extending over several years, with 25 people participating in field work. At the time, this was the most exciting program within Harvard's anthropology department—so exciting that it seemed a threat to some senior professors. (Conrad Arensberg reports that Professor A.M. Tozzer warned him, "There are no jobs in social anthropology. Stick to archaeology, and you can get a job in a museum.")

So broad was the appeal of the Yankee City program that Warner was able to attract some of the best students in the department. He thus could find people able and willing to overcome the barriers of ethnic and social class differences to observe and interview far below the elite.

Deep South

Burleigh B. Gardner wrote me (January 27, 1984) this account of the launching of *Deep South* (Davis et al., 1941):

After the Yankee City study was well under way, Lloyd Warner, with the support of Elton Mayo, began seeking ways to expand his dream of similar studies in other communities. One goal was to conduct a study of an old southern community for comparison with the findings in Yankee City. In 1932 the Committee on Industrial Physiology at Harvard obtained funding for the project, and, with this assured, Warner took the following steps.

He decided on criteria for the study location and selected a number of communities that seemed appropriate, in terms of size and background. He then made a survey trip through the Old South to examine the communities. On this trip he met with a few leaders in each place, both to get information and to establish contacts if the community was to be used. His final choice was Natchez.

Because of the strong caste system, he thought it advisable to have both a Negro and a white who could be accepted. The opportunity was offered to Allison Davis and to Burleigh Gardner, who, with their wives, would make up the research team. Allison, having been raised in Virginia, and Burleigh Gardner, from Texas, were knowledgeable about appropriate behavior in the caste system.

In his survey trip, Warner had become acquainted with the mayor of Natchez, had discussed the survey with him, and had gained the promise of cooperation.

It had been suggested that the study include a more rural community still dominated by the plantation system. Through someone he met in

Natchez, Warner was referred to the publisher of a local paper in Woodville, Mississippi, a small town about thirty miles from Natchez. When the Gardners were ready to start the study, they first moved to Woodville, where they were cordially received by the publisher. He and his family soon introduced them to the leading planters and businessmen. Of course, everyone was curious about "what the Gardners were doing," and they explained that they were studying the community and its historical background.

Through people in Woodville, the Gardners were introduced to people in Natchez, especially the editor of the Natchez newspaper and some of the prominent families. They also called on the mayor to tell him when they expected to move the study to Natchez.

After two or three months in Woodville, the Gardners moved to Natchez. Through the mayor, Burleigh met the chief of police, the sheriff, and other city and county officials.

A month or so later the Davises arrived in Natchez and took rooms with the leading Negro doctor. Gardner informed the mayor and the chief of police of this, with the implication that the Davises were helping with the study of the Negro community. It was necessary that the officials have an acceptable understanding of why the Davises had come to Natchez and what they were up to.

The Gardners were quickly accepted socially into the upper- and upper-middle-class white society, and had no difficulty interviewing those people. However, contacts with the lower-class whites ("po' whites" especially) were more difficult, so Mary Gardner volunteered with the local Emergency Relief Program, which dispensed aid to the needy. She requested a caseload of white families and was given the very poorest. She had to call on her "cases" regularly, and was able to make friends and interview them extensively. Once she was accepted as someone who liked to listen to their problems and their life stories, she had no difficulty in getting the desired information.

Gardner added later,

As you can see, the use of two research teams in the caste situation added some complications. I had to gain the acceptance of the mayor, sheriff, and police in order to protect the Davises in case of some unexpected suspicion of them. From the beginning, I spent a lot of time at the jail getting acquainted with the policemen. They may never have quite understood what I was doing, but I seemed innocuous, and I tried to see that any questions about the Davises would come to me.

Living with the leading Negro doctor must have provided the Davises with an entrée to the elite of Negro society. I have no informa-

tion on how they managed to establish effective research relations with lower-class Negroes.

For access and for their own protection, it was necessary for Gardner to inform certain key officials that the project included research in the Negro as well as the white community, but this was not generally known in Natchez. In fact, the two couples avoided contact with each other in Natchez, meeting for discussion and planning sessions at out-of-town sites.

Street Corner Society

In fall 1936, I set out to study an Italian-American slum district, the North End of Boston. After abortive attempts at entry through a housing survey and a bar on the edge of the district (Whyte, 1981: 289-290), I sought help from social workers in a settlement house. Here was I, the son of middle-class parents, seeking to make contact with lower-class people through association with middle-class social workers, none of whom, furthermore, was of Italian-American extraction. I now look upon this as like trying to get to know a foreign country by making entrance through the American Embassy and its immediate social circles. Fortunately, I recognized that this road into the community was bound to be a dead end, and I was also fortunate enough to meet a social worker who helped me to shift my point of entry. None of the other social workers expressed interest in my study, beyond giving me their definitive interpretations of the community, but somehow, in spite of the vagueness of my own explanations, the head of girls' work in the Norton Street House understood what I needed. She began describing Doc to me. He was, she said, a very intelligent and talented person who had at one time been fairly active in the house but had dropped out, so that he hardly ever came in any more. Perhaps he could understand what I wanted, and he must have the contacts that I needed. If I wished, she would make an appointment for me to see him in the house one evening. This at last seemed right. I jumped at the chance. As I came into the district that evening, it was with a feeling that here I had my big chance to get started. Somehow Doc must accept me and be willing to work with me.

In a sense, my study began on the evening of February 4, 1937, when the social worker called me in to meet Doc. She showed us into her office and then left so that we could talk. Doc waited quietly for me to begin, as he sank down into a chair. I found him a man of medium height and spare build. His hair was a light brown, quite a contrast to the more typical black Italian hair. It was thinning around the temples. His

cheeks were sunken. His eyes were a light blue and seemed to have a penetrating gaze.

I began by asking him if the social worker had told him about what I was trying to do.

"No, she just told me that you wanted to meet me and that I should like to meet you."

Then I went into a long explanation which, unfortunately, I omitted from my notes. As I remember it, I said that I had been interested in congested city districts but had felt very remote from them. I hoped to study the problems in such a district. I felt I could do very little as an outsider. Only if I could get to know the people and learn their problems first hand would I be able to gain the understanding I needed.

Doc heard me out without any change of expression, so that I had no way of predicting his reaction. When I was finished, he asked: "Do you want to see the high life or the low life?"

"I want to see all that I can. I want to get as complete a picture of the community as possible."

"Well, any nights you want to see anything, I'll take you around. I can take you to the joints—gambling joints—I can take you around the street corners. Just remember that you're my friend. That's all they need to know. I know these places, and, if I tell them that you're my friend, nobody will bother you. You just tell me what you want to see, and we'll arrange it."

The proposal was so perfect that I was at a loss for a moment as to how to respond to it. We talked a while longer, as I sought to get some pointers as to how I should behave in his company. He warned me that I might have to take the risk of getting arrested in a raid on a gambling joint but added that this was not serious. I only had to give a false name and then would get bailed out by the man that ran the place, paying only a five-dollar fine. I agreed to take this chance. I asked him whether I should gamble with the others in the gambling joints. He said it was unnecessary and, for a greenhorn like myself, very inadvisable.

At last I was able to express my appreciation. "You know, the first steps of getting to know a community are the hardest. I could see things going with you that I wouldn't see for years otherwise."

"That's right. You tell me what you want to see, and we'll arrange it. When you want some information, I'll ask for it, and you listen. When you want to find out their philosophy of life, I'll start an argument and get it for you. If there's something else you want to get, I'll stage an act for you. Not a scrap, you know, but just tell me what you want, and I'll get it for you."

"That's swell. I couldn't ask for anything better. Now I'm going to try to fit in all right, but, if at any time you see I'm getting off on the wrong foot, I want you to tell me about it."

"Now we're being too dramatic. You won't have any trouble. You come in as my friend. When you come in like that, at first everybody will treat you with respect. You can take a lot of liberties, and nobody will kick. After a while when they get to know you they will treat you like anybody else—you know, they say familiarity breeds contempt. But you'll never have any trouble. There's just one thing to watch out for. Don't spring [treat] people. Don't be too free with your money."

"You mean they'll think I'm a sucker?"

"Yes, and you don't want to buy your way in."

We talked a little about how and when we might get together. Then he asked me a question. "You want to write something about this?"

"Yes, eventually."

"Do you want to change things?"

"Well—yes. I don't see how anybody could come down here where it is so crowded, people haven't got any money or any work to do, and not want to have some things changed. But I think a fellow should do the thing he is best fitted for. I don't want to be a reformer, and I'm not cut out to be a politician. I just want to understand these things as best I can and write them up, and if that has any influence . . ."

"I think you can change things that way. Mostly that is the way things are changed, by writing about them."

That was our beginning. At the time I found it hard to believe that I could move in as easily as Doc had said with his sponsorship. But that indeed was the way it turned out.

While I was taking my first steps with Doc, I was also finding a place to live in Cornerville. My fellowship provided a very comfortable bedroom, living room, and bath at Harvard. I had been attempting to commute from these quarters to my Cornerville study. Technically that was possible, but socially I became more and more convinced that it was impossible. I realized that I would always be a stranger to the community if I did not live there. Then, also, I found myself having difficulty putting in the time that I knew was required to establish close relations on Cornerville. Life in Cornerville did not proceed on the basis of formal appointments. To meet people, to get to know them, to fit into their activities, required spending time with them—a lot of time day after day. Commuting to Cornerville, you might come in on a particular afternoon and evening only to discover that the people you intended to see did not happen to be around at the time. Or, even if you did see them, you

might find the time passing entirely uneventfully. You might just be standing around with people whose only occupation was talking or walking about to try to keep themselves from being bored.

On several afternoons and evenings at Harvard, I found myself considering a trip to Cornerville and then rationalizing my way out of it. How did I know I would find the people whom I meant to see? Even if I did so, how could I be sure that I would learn anything today? Instead of going off on a wild-goose chase to Cornerville, I could profitably spend my time reading books and articles to fill in my woeful ignorance of sociology and social anthropology. Then, too, I had to admit that I felt more comfortable among these familiar surroundings than wandering around Cornerville and spending time with people in whose presence I felt distinctly uncomfortable at first.

When I found myself rationalizing in this way, I realized that I would have to make the break. Only if I lived in Cornerville would I ever be able to understand it and be accepted by it. Finding a place, however, was not easy. In such an overcrowded district a spare room was practically nonexistent. I might have been able to take a room in the Norton Street Settlement House, but I realized that I must do better than this if possible.

I got my best lead from the editor of a weekly English-language newspaper published for the Italian-American colony. I had talked to him before about my study and had found him sympathetic. Now I came to ask him for help in finding a room. He directed me to the Martinis, a family which operated a small restaurant. I went there for lunch and later consulted the son of the family. He was sympathetic but said that they had no place for any additional person. Still, I liked the place and enjoyed the food. I came back several times just to eat. On one occasion I met the editor, and he invited me to his table. At first he asked me some searching questions about my study: what I was after, what my connection with Harvard was, what they had expected to get out of this, and so on. After I had answered him in a manner that I unfortunately failed to record in my notes, he told me that he was satisfied and, in fact, had already spoken in my behalf to people who were suspicious that I might be coming in to "criticize our people."

We discussed my rooming problem again. I mentioned the possibility of living at the Norton Street House. He nodded but added: "It would be much better if you could be in a family. You would pick up the language much quicker, and you would get to know the people. But you want a nice family, an educated family. You don't want to get in with any low types. You want a real good family."

At this he turned to the son of the family with whom I had spoken and asked: "Can't you make some place for Mr. Whyte in the house here?"

Al Martini paused a moment and then said: "Maybe we can fix it up. I'll talk to Mama again."

So he did talk to Mama again, and they did find a place. In fact, he turned over to me his own room and moved in to share a double bed with the son of the cook. I protested mildly at this imposition, but everything had been decided—except for the money. They did not know what to charge me, and I did not know what to offer. Finally, after some fencing, I offered fifteen dollars a month, and they settled for twelve.

The room was simple but adequate to my purposes. It was not heated, but, when I began to type my notes there, I got myself a small oil-burner. There was no bathtub in the house, but I had to go out to Harvard now and then anyway, so I used the facilities of the great university (the room of my friend, Henry Guerlac) for an occasional tub or shower.

Physically, the place was livable, and it provided me with more than just a physical base. I had been with the Martinis for only a week when I discovered that I was much more than a roomer to them. I had been taking many of my meals in the restaurant and sometimes stopping in to chat with the family before I went to bed at night. Then one afternoon I was out at Harvard and found myself coming down with a bad cold. Since I still had my Harvard room, it seemed the sensible thing to do to stay overnight there. I did not think to tell the Martinis of my plan.

The next day when I was back in the restaurant for lunch, Al Martini greeted me warmly and then said that they had all been worried when I did not come home the night before. Mama had stayed up until two o'clock waiting for me. As I was just a young stranger in the city, she could visualize all sorts of things happening to me. Al told me that Mama had come to look upon me as one of the family. I was free to come and go as I pleased, but she wouldn't worry so much if she knew of my plans.

I was very touched by this plea and resolved thereafter to be as good a son as I could to the Martinis.

At first I communicated with Mama and Papa primarily in smiles and gestures. Papa knew no English at all, and Mama's knowledge was limited to one sentence which she would use when some of the young boys on the street were making noise below her window when she was trying to get her afternoon nap. She would then poke her head out of the window and shout: "Goddam-sonumabitcha! Geroutahere!"

Some weeks earlier, in anticipation of moving into the district, I had begun working on the Italian language with the aid of a Linguaphone. One morning now Papa Martini came by when I was talking to the phonograph record. He listened for a few moments in the hall trying to make sense out of this peculiar conversation. Then he burst in upon me with fascinated exclamations. We sat down together while I demonstrated the machine and the method to him. After that he delighted in working with me, and I called him my language professor. In a short time we reached a stage where I could carry on simple conversations, and, thanks to the Linguaphone and Papa Martini, the Italian that came out apparently sounded authentic. He liked to try to pass me off to his friends as *paesano mio*—a man from his own home town in Italy. When I was careful to keep my remarks within the limits of my vocabulary, I could sometimes pass as an immigrant from the village of Viareggio in the province of Tuscany.

Since my research developed so that I was concentrating almost exclusively upon the younger, English-speaking generation, my knowledge of Italian proved unnecessary for research purposes. Nevertheless, I feel certain that it was important in establishing my social position in Cornerville—even with that younger generation. There were school-teachers and social workers who had worked in Cornerville for as much as twenty years and yet had made no effort to learn Italian. My effort to learn the language probably did more to establish the sincerity of my interest in the people than anything I could have told them of myself and my work. How could a researcher be planning to "criticize our people" if he went to the lengths of learning the language? With language comes understanding, and surely it is easier to criticize people if you do not understand them.

My days with the Martinis would pass in this manner. I would get up in the morning around nine o'clock and go out to breakfast. Al Martini told me I could have breakfast in the restaurant, but, for all my desire to fit in, I never could take their breakfast of coffee with milk and bread.

After breakfast, I returned to my room and spent the rest of the morning, or most of it, typing up my notes regarding the previous day's events. I had lunch in the restaurant and then set out for the street corner. Usually I was back for dinner in the restaurant and then out again for the evening.

Usually I came home again between eleven and twelve o'clock, at a time when the restaurant was empty except perhaps for a few family friends. Then I might join Papa in the kitchen to talk as I helped him dry the dishes, or pull up a chair into a family conversation around one of

the tables next to the kitchen. There I had a glass of wine to sip, and I could sit back and mostly listen but occasionally try out my growing Italian on them.

The pattern was different on Sunday, when the restaurant was closed at two o'clock, and Al's two brothers and his sister and the wives, husband, and children would come in for a big Sunday dinner. They insisted that I eat with them at this time and as a member of the family, not paying for my meal. It was always more than I could eat, but it was delicious, and I washed it down with two tumblers of Zinfandel wine. Whatever strain there had been in my work in the preceding week would pass away now as I ate and drank and then went to my room for an afternoon nap of an hour or two that brought me back completely refreshed and ready to set forth again for the corners of Cornerville.

Though I made several useful contacts in the restaurant or through the family, it was not for this that the Martinis were important to me. There is a strain to doing such field work. The strain is greatest when you are a stranger and are constantly wondering whether people are going to accept you. Much as you enjoy your work, as long as you are observing and interviewing, you have a role to play, and you are not completely relaxed. It was a wonderful feeling at the end of a day's work to be able to come home to relax and enjoy myself with the family. Probably it would have been impossible for me to carry on such a concentrated study of Cornerville if I had not had such a home from which to go out and to which I might return. (I lived with the Martinis for eighteen months until I married Kathleen King and we moved into our own flat.)

I can still remember my first outing with Doc. We met one evening at the Norton Street House and set out from there to a gambling place a couple of blocks away. I followed Doc anxiously down the long, dark hallway at the back of a tenement building. I was not worried about the possibility of a police raid. I was thinking about how I would fit in and be accepted. The door opened into a small kitchen almost bare of furnishings and with the paint peeling off the walls. As soon as we went in the door, I took off my hat and began looking around for a place to hang it. There was no place. I looked around, and here I learned my first lesson in participant observation in Cornerville: Don't take off your hat in the house—at least not when you are among men. It may be permissible, but certainly not required, to take your hat off when women are around.

Doc introduced me as "my friend Bill" to Chichi, who ran the place, and to Chichi's friends and customers. I stayed there with Doc part of the

time in the kitchen, where several men would sit around and talk, and part of the time in the other room watching the crap game.

There was talk about gambling, horse races, sex, and other matters. Mostly I just listened and tried to act friendly and interested. We had wine and coffee with anisette in it, with the fellows chipping in to pay for the refreshments. (Doc would not let me pay my share on this first occasion.) As Doc had predicted, no one asked me about myself, but he told me later that, when I went to the toilet, there was an excited burst of conversation in Italian and that he had to assure them that I was not a G-man. He said he told them flatly that I was a friend of his, and they agreed to let it go at that.

We went several times together at Chichi's gambling joint, and then the time came when I dared to go in alone. When I was greeted in a natural and friendly manner, I felt that I was now beginning to find a place for myself in Cornerville.

When Doc did not go off to the gambling joint, he spent his time hanging around Norton Street, and I began hanging with him. At first, Norton Street meant only a place to wait until I could go somewhere else. Gradually, as I got to know the men better, I found myself becoming one of the Norton Street gang.

Then the Italian Community Club was formed in the Norton Street Settlement, and Doc was invited to be a member. Doc maneuvered to get me into the club, and I was glad to join, as I could see that it represented something distinctly different from the corner gangs I was meeting.

As I began to meet the men of Cornerville, I also met a few of the girls. One girl I took to a church dance. The next morning the fellows on the street corner were asking me: "How's your steady girl?" This brought me up short. I learned that going to the girl's house was something that you just did not do unless you hoped to marry her. Fortunately, the girl and her family knew that I did not know the local customs, so they did not assume that I was thus committed. However, this was a useful warning. After this time, even though I found some Cornerville girls exceedingly attractive, I never went out with them except on a group basis, and I did not make any more home visits either.

As I went along, I found that life in Cornerville was not nearly so interesting and pleasant for the girls as it was for the men. A young man had complete freedom to wander and hang around. The girls could not hang on street corners. They had to divide their time between their own homes, the homes of girl friends and relatives, and a job, if they had

one. Many of them had a dream that went like this: some young man, from outside of Cornerville, with a little money, a good job, and a good education would come and woo them and take them out of the district. I could hardly afford to fill this role.

Joining a political organization seemed the best way to study local politics, but I hesitated to make a commitment to one faction. That problem solved itself with a special election for a vacant seat in Congress. State Senator Joseph Ravello was the only Italian-American running, so the other North End politicians all had endorsed him—rather reluctantly. I signed up as a campaign worker. The candidate had no idea how to use me, but I suggested that I take notes on meetings of the campaign workers and write them up later so that no good ideas would be lost. I am sure the candidate never made any use of those notes, but carbon paper enabled me to document the campaign for my own purposes. I never did penetrate the high-level strategy sessions in which Ravello met with political leaders in other wards of the congressional district, but I did get a picture of politics at the grass-roots level.

Many months had passed before I had an opportunity to approach a study of the rackets organization. This was frustrating because I could look out my window to the store across the street where the man who was said to be the racket boss for all New England sometimes dropped in to see old friends.

In fact, I once got up my courage, crossed the street, and told the proprietor I would like to talk to Joe Lombardi. Naturally, he wanted to know why. I told him, "I am collecting for the United Fund, and I would like to see him about a contribution." The reply: "He already gave at the office." I was tempted to ask, "And where is the office?" but I could not think of a plausible reason for the question.

My opening finally came when the eldest son in the family with which I had been living was grumbling to me about a pair of banquet tickets he had had to buy from a local policeman. His wife did not want to go to the banquet, and he asked if I would like to accompany him. I asked what the occasion was. He told me that the banquet was in honor of the son of the local police lieutenant. The young man had just passed his bar examinations and was starting out on his legal career. I thought a moment. It was perfectly obvious what sorts of people would be present at the banquet: mainly policeman, politicians, and racketeers. I decided that this might be an opportunity for me.

My friend knew Tony Cataldo, a middle-level operator in the numbers racket, and we sat at his table and went bowling later with him and

a business associate. Luckily for me, Tony lived with his family within a block of the flat where Kathleen and I lived; the small store he owned was just as close to us. Tony was not there much of the time, but his older brother conducted legitimate business while the numbers operation was carried on in a back room. After this promising beginning, however, I was unable to develop the relationship so as to lead me deeper into the racket organization.

Would it have been possible for me to carry out an intensive study of the racket organization? Taking advantage of the fact that a major "crime family" has some of its second- and third-generation members established in legitimate businesses and professions and in politics, years later Francis Ianni (1972) built on a chance meeting with a young lawyer in what he calls the Lupollo family to build a friendship that led him into family social gatherings. He became so much a part of the Lupollo social circle that, without formal interviewing, he was able to gather a wealth of data, not only on the family organization, but also on the racket operations.

But note that Ianni did his study over a period of years, picking up data as opportunities arose. Throughout his field work period he was engaged full time in other activities. Such a study does not lend itself to concentrated and systematically planned field work.

Other students made their initial entries into inner-city slums by pathways somewhat different from mine. Elliot Liebow, Elijah Anderson, and Ruth Horowitz all began by finding a place to hang around, but followed up on initial chance contacts by finding a key informant and gatekeeper and establishing a firm social base through this relationship.

Tally's Corner

Liebow (1967: 236) had originally planned to do a series of urban studies,

a neighborhood study, then moving on say, to a construction laborer's union, then a bootleg joint, and perhaps rounding these out with a series of genealogies and life histories.

I was going to give myself about a month or so of poking around town, getting the feel of things, before committing myself to any firm plan of action.

In taking up the director's [Hylan Lewis's] suggestion that this would be "a good place to get your feet wet," I went in so deep that I was completely submerged and my plan to do three or four separate

studies, each with its own neat, clean boundaries, dropped forever out of sight. My initial excursions into the street—to poke around, get the feel of things, and to lay out the lines of field work—seldom carried me more than a block or two from the corner where I started. From the very first weeks or even days, I found myself in the middle of things; the principal lines of my field work were laid out, almost without my being aware of it. For the next year or so, and intermittently thereafter, my base of operations was the corner Carry-out across the street from my starting point.

Liebow began by simply hanging around a street corner, picking up casual conversations until he was able to extend them into more intimate and friendly discussions. In the early days of his study, these contacts led him to Tally Jackson, around whom much of the life of the neighborhood revolved. Tally then filled the role for Liebow that Doc had played for me.

Elliot Liebow did his field work before the height of black militancy, when Afro-American intellectuals were attacking white social scientists for studying black communities instead of concentrating on the white neighborhoods and organizations oppressing minority people.

Would it have been impossible for Liebow to study a black slum neighborhood in the more militant later period? I doubt it. In the first place, black middle-class intellectuals don't control access to black slum neighborhoods. Furthermore, during the height of the black power militancy, I heard a black sociologist pause in his argument to say that he was specifically exempting Elliot Liebow.

A Place on the Corner

For his study in and around Jelly's Bar in a black neighborhood, Elijah Anderson (1978) had neither the color difference to overcome nor a major gap in social status between himself and those he studied. Nevertheless, there was a difference both in social class and culture as well as a major difference in levels of education. In a session at the 1981 meeting of the American Sociological Association, he gave this interpretation of himself:

I grew up in a home situation in which my father worked in a factory and my mother worked for a while as a domestic. Later, they bought and operated a grocery store that served the local black community. What we call "middle-class" values were emphasized in my home situation—decency, hard work, neatness, personal hygiene, punctuality, delayed gratification. Having been raised in a black American

milieu and having attended integrated schools, I became bilingual: I spoke what linguists are beginning to call "Black English Vernacular" and standard English, and I, like so many black Americans, speak either, often depending on the social situation. With this ability and other related cultural skills, I was able to fit in with the social environment of Jelly's in ways that other researchers might not have been able to. My past cultural experience undoubtedly helped me as I began to negotiate my way into that setting.

What became *A Place on the Corner* (Anderson, 1978) began as a term paper project in graduate work at the University of Chicago.

Even the finding of Jelly's was affected by certain presuppositions that I held about such places. When Jerry Suttles told members of the seminar to go out and find sites to study, I remember driving along and up and down the ghetto streets of the Southside as well as the Westside, looking for a suitable place to study. I checked out a number of taverns, some of which were very tough looking and some of which seemed mild. Although I have a great love for social science, I do care about my physical safety, and that was a consideration. Somehow Jelly's as a place felt good to me. When I walked in, took a seat, and began drinking and talking with people, something clicked. The ambience, the hospitality, which the people maybe didn't know they were offering—it just felt good, and I sincerely wanted to get to know the people. And I think this fact was very important for the success of my study.

Before he met his principal guide and informant, Anderson had already put in enough time at Jelly's to become a familiar figure, while still remaining on the fringes of any social group.

After about four weeks into the setting, I met Herman. He became my main informant, sort of like Doc was for you, I guess. Herman seemed to take a liking to me right away. I was very straight and up front with him, or at least as straight as I could be at the time. You see, I wasn't certain in the beginning how this whole thing would work out. I didn't know it would work out to be anything more than a seminar paper, and I certainly didn't know the work would become a dissertation, let alone a book. As we talked, Herman and I became friendly. I told him that I was a student at the University of Chicago, and he thought that was very nice and decent, and he told me as much. As he got to know me and found me trustworthy, he warmed up to me considerably. When I went to Jelly's, I dressed the way the others dressed—in jeans,

army jackets, sneakers and boots. Herman believed me and I believed myself at that point, and I thought I was simply writing a paper for a class and I felt it wasn't worth it to formalize things by saying, "I want to do a study of your group, can I do it?" And I didn't say that then, but after a couple of months when I was clearly becoming increasingly involved and sensing that this project was much more than a seminar paper, I broached the subject with Herman. And he became very supportive and said, "Yea, sure, do it." Then the next day, Herman told the men, "Al' right, ya'll, do somethin'. Eli's studyin' ya'll. Do somethin'." It seemed clear to him that our friendship was much more important than any paper coming out of this work.

It was Herman who vouched for Anderson and brought him into the inner circle at Jelly's.

Honor and the American Dream

Ruth Horowitz's (1983) study of a Chicano neighborhood is noteworthy in two respects. In the first place, Horowitz carried out her study in two time periods, 1971 to 1974 and 1977, thus enabling her to follow her people over a six-year period. In the second place, Horowitz was able to present apparently equally intimate accounts of the lives of Chicano teenagers and young adults of both sexes. I had assumed that it would have been impossible for me to study young Italian-American women as a participant observer, and indeed that was probably the case. While they often passed the street corner, it was not customary for "good girls"—or for any females, for that matter—to spend time on the corners. Pursuing them into their homes would hardly have led me into the kind of relaxed, informal situation conducive to good participant observation.

Horowitz was fluent in Spanish, a practical asset for communication with the older generation and no doubt an advantage with the younger generation as it suggested a sincere interest in people in their ethnic background. Beyond that, as she described herself,

I am Jewish, educated, small, fairly dark, a woman, dressed slightly sloppily but not too sloppily, and only a few years older than most of those I observed.

I had little choice but to acknowledge publicly the reasons for my presence on 32nd Street; not only do I differ in background from the 32nd Street residents but I had to violate many local expectations to gather the data I needed. For example, women do not spend time

alone with male gangs as I did. Because I was an outsider I had to ask a lot of "stupid" questions—"Who are the guys in the black and red sweaters?" or "Why do you fight?" As anything but an acknowledged outsider I would have had a difficult time asking them. Moreover, while my appearance allowed me to blend into a youthful crowd, I sounded and looked sufficiently different so that most people who did not know me realized that I was not from the neighborhood [Horowitz, 1983: 6].

Here she describes the initial encounter with the first group she studied:

I chose to sit on a bench in a park where many youths gathered from noon until midnight. On the third afternoon of sitting on the bench, as I dropped a softball that had rolled toward me, a young man came over and said, "You can't catch" (which I acknowledged) and "You're not from the hood [neighborhood], are you?" This was a statement, not a question. He was Gilberto, the Lions' president. When I told him I wanted to write a book on Chicano youth, he said I should meet the other young men and took me over to shake hands with eight members of the Lions.

The park became my hangout every day after that, but it was several months, several bottles of Boone's Farm Strawberry Wine, and a number of rumors about my being a narcotics agent before gang members would give me intimate information about their girlfriends, families, and feelings about themselves and the future [pp. 7-8].

Though she writes that "my relationship with the male gang members never was easy" (p. 9), Horowitz was able to build on her acceptance with the Lions to establish relations with other neighborhood gangs. Her openings to groups of young women came in the fourth week of field work and through their friendships with members of the Lions. Her contacts with upwardly mobile youth began about a month later when two young women approached her to strike up a conversation. After they had satisfied their curiosity, they and their friends informally became her protectors in the neighborhood.

That night and many nights during the first year, they and a group of their friends walked me the five blocks to the bus stop. By the third time I saw them at the park they invited me to their homes to meet their families and to eat. In one family I was immediately adopted and included in all the family celebrations and holiday dinners [p. 10].

Regarding the problems of building effective research relations with both the young men and the young women, she writes:

I did have to be extremely careful not to develop a sexual identity. My lack of care with appearance, which both males and females continually remarked upon, helped, but I was very careful not to spend too much time alone with any one male and not to dance with them at the many parties and dances I attended [p. 10].

ENTERING THE WORKPLACE

Where jobs are available, becoming a worker participant observer presents no problems of access in our own society. Gatekeepers in the employment office can consider you just another worker. However, access as a participant observer can present formidable problems in a culture and society foreign to the researcher.

Participant Observer in Japan

In a 1981 panel discussion at the American Sociological Association meeting, Robert Cole described how he started his research in Japanese industry:

I thought I wrote a wonderful dissertation prospectus on the applicability of Max Weber to Japanese industrialization, but in fact my heart was set on trying to get into a Japanese factory. I was discouraged heavily by most folks I came into contact with, and for a long time I thought I was going to write my dissertation on what we can learn about Japanese society without getting in to work there.

The access was extremely difficult in Japan compared to what I think one would experience in the U.S. for a Westerner for a number of reasons. First, there is very little tradition of doing participant observation in Japan by scholars. They were much more into the philosophical tradition, in terms of social science, and so that was fairly novel. There are cases of journalists doing what we would call participant observation, but strictly for journalistic purposes. Since that tradition is not well established in Japan, you simply have trouble explaining what you are about. Second, Japan is a pretty closed society. They don't have the mix of ethnic groups that we are accustomed to, that would make it understandable why a Westerner would want to work in a Japanese factory. They have a tradition of exploiting Koreans and some other marginal groups. Most Westerners were therefore excluded from employment in major firms. Most Westerners, to the

extent that they get involved in a Japanese factory, would be "technical experts" who came for a short period to impart information.

I didn't come to impart information, so I had a lot of trouble. I thought I had fully explained that as much as possible I wanted to be treated as a "normal" worker. I demanded pay, to give credibility to my venture, but still when I showed up that first day they didn't really understand that I expected to work. I made an ordinary request of a Western field worker that no special treatment be given, for all sorts of the usual reasons since with special treatment one runs the risk of discrediting yourself in the eyes of the workers. I had a lot of trouble with that because, in the context of Japan, no special treatment, to management and to the union, meant that I was being extraordinarily cocky. It was as if I was telling them I knew all I had to know and I could take care of myself. For them it was patently obvious that I didn't know a damn thing about Japan, and no one could ever understand us, the Japanese, so that was a real problem. In fact, at one company where I was turned down, that was given as the reason. In this particular case, I answered a newspaper ad and that blew everyone away. They were having interviews, and I remember showing up, and a woman who was taking applications just got all shook and turned to me and said in the most blatant racist terms, "We don't hire foreigners." They hadn't learned the sophistication of Westerners in terms of how racism gets used to keep people out of jobs. But my application was so unprecedented that she hadn't really learned how to deal with it.

I finally made some contacts through a union in one company and through a former employee of a major company, and they both arranged for me to get into subcontract firms. That was not accidental because the larger firms were concerned about me causing disruption, and they felt safer if I went into a smaller subcontract firm where anything that happened wouldn't embarrass the "parent" firm.

There was a lot of testing of me. The workers were involved in a rather ferocious struggle. The plant had a militant union, and the struggle was between the communists and the socialists. In the beginning, the workers were feeding me information to see if it got back to management. Not that it made any sense for me to be a management spy. Japanese management didn't need an illiterate foreigner as a management spy. It also took time to build the trust necessary to do my work.

Initially, there were a couple of workers that befriended me. One of them was a worker who had rather menial jobs in the plant, but he took it upon himself to tell me like it was. If there was a fight going on in the other section, he would come in to me and say, "Hey, you better

get over there and see what's going on." He really educated me in many ways. One of the factional leaders in the union also took me under his wing. These individuals became points of entry.

When I went into the Japanese factory, I went in "as an American student who was writing a paper." It was made very clear to me by Japanese scholars and other people that if I had come in as a professor, particularly in the context of Japan with all the status implications, I could not have developed the necessary rapport with workers that I needed to understand factory life. At least for some kinds of participative observation studies, there may be some age grading involved in the sense that one's age impacts on the kind of research you can do. When I began my work in the factory I was 28 years old. At the diecast company I was working at, this was the average age of workers as well, and it matched the ages of those in my work group. Consequently, a lot of the ice-breaking could be around similar kinds of life experiences. I had a good deal more difficulty developing rapport in my second job in an auto parts factory where those in work group were in their late teens and early 20s and just off the farm.

Studying Phillips Petroleum Co.

When I began my first industrial study in 1942, such research was unknown in Oklahoma and little known elsewhere. To break into this new field, as a novice I had to gain entry through people who had not the vaguest idea of what such a study might be like.

The head of the University of Oklahoma Sociology Department, W. B. Bizzell, was most helpful. He had previously been president of the university and had been bumped back to head of the department in a clash with the regents. As president he had become very well acquainted with Frank Phillips, founder and chairman of Phillips Petroleum, one of the great industrial entrepreneurs. The business was then only about 25 years old but already moving up to become one of the major oil companies.

The basis of Bizzell's friendship with Phillips was Phillips's interest in archaeology. Previously it had been believed that Oklahoma's early Indian tribes had been very primitive. Then some archeologists discovered the Spiro mound, which revealed an enormous quantity of elaborate pots and other artifacts that excited archaeologists and made Oklahomans swell with pride. So Bizzell would arrange for a fly-in dig for Frank Phillips. He would come in his private plane, the university archaeologist would lead him to the appropriate spot, Phillips would hand his coat to his pilot, the archaeologist would hand him a shovel,

Phillips would dig, and up would come a pot. Then, while Phillips posed with Bizzell and the pot, the university photographer would take pictures. Phillips would then climb into his plane with the pot and fly back to his headquarters in Bartlesville.

In November 1942 I arrived at Bartlesville with a letter from Bizzell. Phillips greeted me: "I can give you two minutes. Then I have to meet with my Board of Directors." I tried to give my two-minute introduction, presenting the study I wanted to do, interviewing and observing human relations among workers and management.

He asked me two questions: "What experience have you had in the oil industry?" None. "Are you a lawyer?" No. He raised his eyes to the ceiling. There was a pause while I tried to claim that I understood working people and would fit in somehow. He called in Warren Felton, his manager of employee relations. Phillips left me with Felton without apparent instructions. Felton took me to his office. We talked, and he certainly was puzzled too, as to what to do with me, but he introduced me to his assistant. I learned that his assistant was about to take off for Oklahoma City, a three-hour train ride, so I arranged to go back with him and had a good chance to get acquainted. He promised to explain the study to Division Manager Al Wenzel, and it would be up to him to decide.

When I encountered Wenzel and Jeff Franklin, the personnel manager, they had been briefed by Felton's assistant. They said that the study was a dandy idea but not right at that time. The CIO was then organizing, and they were facing a representation election. If I would just wait until that was over, whichever way it went, then it would be fine for me to come in. I asked when it would take place. "Maybe in a couple of weeks," they said, "but we're not sure." I knew nothing about industrial relations, but I knew enough not to accept that guess. This was November, the election took place in mid-April, and, as it turned out, I left Oklahoma in May.

I had to scramble for another approach. I explained that I knew practically nothing about the oil industry, so it would be very valuable if I could sit in the office and go over their personnel records. They couldn't think of a way to say no to that, so, for what must have been three to four weeks, two days a week I engaged in some of the duller work of my career. I sat in the office with big sheets of paper, going through personnel records and marking things down, accumulating an enormous pile of data which I subsequently destroyed, but it gave me an excuse to be there.

Fortunately the headquarters of the division was on the edge of Oklahoma City. There were no restaurants nearby, so for lunch they would bring in sandwiches. We would sit and talk, and I would try to be as charming and nonthreatening as possible.

After this went on for some time, they figured they would never get me out of the office if they didn't think of something. They told me that a staff man in the personnel department was going into the field to do a job description, and I could go along and help him.

And so we went out to the Capok plant that was making aviation gasoline from natural gas. I met the plant superintendent and the foreman, and then I followed the staff man around as he interviewed workers. I made notes and I began to learn something about the nature of the work. But the next week the staff man was pulled off onto something more urgent. The superintendent was sympathetic and said if I wanted to continue with the job description that it was okay with him.

However, I was engaged in doing the first job description I had ever heard of, so it seemed rather implausible to continue. I had no recourse but to level with the men as to what I was really up to. I told them that I was a professor at the University of Oklahoma. They were naturally suspicious. Was I a company man? Well, I was on the payroll, but for only the \$25 a month that the company was giving me for field expenses. I even showed my paycheck to the men, assuming that they wouldn't think a college professor would sell his soul that cheaply.

This turned out to be an ideal setting for a study because the work consisted of watching the dials and charts, running brief tests about once every hour, and making occasional adjustments, except in emergencies. The men were bored and were glad to talk to somebody new. I was happy to listen to them, and gradually they began talking more and more freely. Thus I was able to view from the inside the struggle between the CIO and management to win the hearts and minds of the workers—a project on which I wrote an unpublishable book (see Chapter 11).

Studying Restaurants

In 1944, when I began the restaurant projects, social research in industry was still such an unfamiliar phenomenon as to pose formidable problems of access. But since Vernon Stouffer was a member of the National Restaurant Association committee, which was sponsoring the

project, he could hardly bar the way when I decided to make the Chicago Loop Stouffers our first case study.

Margaret Chandler wormed her way into a cafeteria where she had been having some of her meals and where she had already become personally acquainted with the owner. She then combined interviewing and observation with helping out as cashier and doing other odd jobs. Edith Lentz began her field work as a waitress.

Beyond these beginnings, I found the access problems were getting no easier. When I tried to sell the study to restaurant owners or managers, I was confronted with two equal and opposite rejections. One restaurateur would say, "Things are going so smoothly now that I don't want to take any chances letting an outsider in." The next restaurateur would say, "We have so many problems and things are so tense right now that letting an outsider in might cause a blow-up." In vain I tried to explain that the research methods we used did not "put ideas into people's heads" and that the catharsis of talking to a friendly outsider could have a calming effect.

Frustrated in this direct approach, I adopted a different strategy. After an initial explanation of the nature of the study, I asked the restaurateur to tell me how things were going in his or her restaurant and what problems should be given special attention in our study. As the discussion proceeded, with the restaurateur doing most of the talking, I would find openings to relate similar problems or experiences from other restaurants we were studying. Without pressing the point, I would add that we were then considering further restaurants for case studies and had a number of possibilities in mind.

On my first try with this strategy, the restaurateur asked if we would be willing to study his establishment. After a moment of apparent indecision, I agreed.

Although I have not used this strategy in experimentally controlled situations, it makes theoretical sense to me. If you approach the gatekeeper with the idea that you are determined to study his or her organization, your eagerness tends to build up his or her defenses. If you go in with the assumption that this is only one of a number of organizations that might be appropriate to your study, you can carry on a much more relaxed discussion, leading to a mutual exploration of the advantages and disadvantages for both parties of making this firm one of your cases. Nor does this strategy need to be considered simply a

diplomatic maneuver. There can be cases in which the gatekeeper eventually expresses willingness to grant access, but the course of the conversation suggests to you that it would be better to carry on the study elsewhere. Over the years, as gatekeepers became more familiar with social research in industry, I found it much easier to negotiate my entry.

ACCESS ROUTES

The routes to access are somewhat different according to whether we are studying a community or a work organization (factory, government agency, hospital, and so on). For the work organization, access routes also differ according to whether you enter as an employee participant observer or as a recognized researcher. In the former case, in our own society, if a job is open, entry may require nothing more than going through the procedures open to any applicant. As Robert Cole found in Japan, this route may not be open in another country with a markedly different culture. For a recognized researcher, entry is impossible without permission from an *official* gatekeeper. The problem then is how to explain our purposes in a way that satisfies the gatekeeper and yet does not distort or unduly limit the nature of the study.

In a community-based study, social acceptance must be negotiated with gatekeepers. If we plan to study the total community, then it makes sense (as in Muncie, Newburyport, and Natchez) to approach first the key officials of local government and/or leading citizens. Students or professors should find that their upper-middle-class status and their university involvement provide a presumption of legitimacy to the research role in the eyes of such gatekeepers.

If we aim to gain an intimate view of lower class people beyond the boundaries of social agencies, access routes are much less clearly marked. Gatekeeping functions are less centralized, and public officials, social workers, or leading citizens are likely to be uninformed or misinformed regarding the identity of gatekeepers to informal groups or formal associations. The researcher has to plunge into unmapped territory to discover gatekeepers without outside help (as in the case of Liebow, Anderson, and Horowitz) or, as in my case, with just a single (but indispensable) bit of guidance from a social worker.

As I reflect upon the experience of the four of us who engaged in extended and intimate participant observation in lower class neighborhoods, I find important elements in common. The same conclusions

may apply in broader community studies that include intensive studies within such neighborhoods.

(1) At the outset, we did not know what we were looking for. We did not enter the field with blank minds, yet our original formulations proved to have little relation to the studies that eventually evolved. We set out on the frontiers of our personal knowledge and began exploring beyond those frontiers.

(2) Such an exploration demands an investment of many weeks' time in getting familiar with the social terrain and gaining acceptance by local people. Participant observation is not for the researcher who aims to get firm answers quickly.

(3) Though far from our customary social circles, we do not operate alone. The successful participant observer finds local guides to join in the exploration and to vouch for the credibility and sincerity of the researcher.

(4) Full-time participant observation over an extended period of time tends to be an age-graded phenomenon. Such studies are most likely to be done by young people, in our student years. When we are established professionals, with teaching or other professional responsibilities, we are unlikely to have the time and the motivation to make such a full commitment. Nevertheless, the techniques we learn in full-time participant observation can be adapted to later studies where such immersion in the field is not possible.

Observational Methods

What should we observe in the field? Before answering that question, let us first deal with two common misconceptions. Since human beings are the only species with a language, the field worker is likely to assume that the verbal content of interpersonal interactions is all that matters. Speech is obviously important, and we will deal with verbal content later, but it is important to recognize that a great deal of what is important to observe is unspoken.

The beginner is inclined to assume that social observation takes a high level of skill and sensitivity. Indeed, there are some subtle behaviors that provide significant clues to what people are thinking and feeling. For example, a more active than usual movement of the Adam's apple may portray emotions that the informant is trying to suppress. Similarly, if the informant has been looking straight in the eyes of the researcher for most of the time but then looks away when discussing a particular topic, he or she may be dealing with a delicate subject. Such bodily clues are significant, but we should not assume that the major task of the observer is to discover emotional states that the subject is trying to conceal. Some of the most basic aspects of behavior are readily observable and recordable by anyone of normal intelligence.

FOCUS ON STRUCTURE AND LEADERSHIP

We assume that human behavior is not random but structured. Much of it is *socially* structured, and we need to discover the framework

Author's Note: Parts of this chapter are adapted from Whyte (1951b) and Whyte (1981). Material from W. F. Whyte, *Street Corner Society* (© 1981 University of Chicago Press), used by permission.

for such structuring. This is obvious enough when we are studying a formal organization, with titles, offices, and so on, but even there, behaviors may not closely conform to what we would expect from titles and office arrangements. We must go beyond the organization chart in order to discover the social uniformities of behavior.

Social anthropologist Eliot D. Chapple puts it most simply when he states that we need to answer the question, "Who does what with whom, when, and where?" Note that the question does not include *why*. Answers to the question *why* are based on inferences from research data. We cannot observe *why* anyone does anything. We can observe who the actors are, the time during which the interactions are taking place, and the location of those interactions. Such observations, over a period of time, provide essential evidence regarding social groupings, and the frequency and duration of interaction among those observed.

To go beyond groupings and interactional frequencies and durations so as to get at informal leadership and followership relations, we need to make the critical distinction suggested by Eliot D. Chapple and Conrad Arensberg (1940) between pair events and set events. A *pair event* is an interaction between two individuals. A *set event* involves interactions among three or more individuals.

If we observe only pair events, we often find it impossible to make valid judgments about who is influencing or dominating whom. At the extreme, if we observe several incidents in which A flatly tells B what to do and we subsequently observe B carrying out the action, A is clearly dominating B. But how are we to interpret an observation in which B suggests a course of action and A agrees, and they then jointly follow this course of action? Here B is initiating an activity for A, but A appears to have some freedom of action and could reject the suggestion.

In set events the structural relations become clear—and without our having to assume that the stimulus for the activity is an order, a direction, a suggestion, or an entreaty. We observe here the interaction, including conversation, through which there is an objective change in the pattern of group activity. These examples will serve as illustrations:

Seven men are standing in the club room, in groups of two, two, and three. Individual X comes in and the three little groups immediately re-form into one larger group, with the seven men remaining silent while X talks and each man seeking to get the attention of X before he himself speaks.

X says, "Let's take a walk." We then observe the group setting out for a walk. Or A says to X, "Let's go to the Orpheum." X says, "Naw, that

picture is no good." No change in group activity. Then B says to X, "Let's go to the State." X says, "O.K." The group is then off to the State.

Some of the fellows are sitting around a table in a cafeteria having their evening coffee-and. A leaves the group to sit down for a few minutes with people at a nearby table. X remains at the original table, and the conversation continues much as it did when A was present. On another occasion, the same people are present in the same spatial arrangement in the cafeteria, but this time it is X who gets up and goes over to another table. The conversation at X's former table noticeably slows down and perhaps breaks up into twos and threes. The men talk about what X could be doing over at the other table, and their attention is frequently directed to that table. If X stays away for some time, we may observe his friends picking up their chairs and moving over to the other table with him.

Observations along these lines establish that X characteristically initiates action for this group, that he is the leader of the group.

CHARTING SPATIAL RELATIONS

When the observer is studying a small group, the record naturally notes the names of the individuals present and interacting during the period of observation. In studying a larger organization we cannot follow the interactions of all members in the same physical space and during the same time period, and therefore we must devise methods to help us sort out the subgroupings. I encountered this problem in a study of what I called the Cornerville S&A Club.

The club had fifty members. Fortunately, only about thirty of them were frequent attenders, so that I could concentrate on that smaller number, but even that presented a formidable problem.

I felt I would have to develop more formal and systematic procedures than I had used when I had been hanging on a street corner with a much smaller group of men. I began with positional mapmaking. Assuming that the men who associated together most closely socially would also be those who lined up together on the same side when decisions were to be made, I set about making a record of the groupings I observed each evening in the club. To some extent, I could do this from the front window of our apartment. I simply adjusted the venetian blind so that I was hidden from view and I could look down and into the store-front club. Unfortunately, however, our flat was two flights up, and the angle of vision was such that I could not see past the middle of the clubroom. To get the full picture, I had to go across the street and be with the men.

When evening activities were going full blast, I looked around the room to see which people were talking together, playing cards together, or otherwise interacting. I counted the number of men in the room, so as to know how many I would have to account for. Since I was familiar with the main physical objects of the clubroom, it was not difficult to get a mental picture of the men in relation to tables, chairs, couches, radio, and so on. When individuals moved about or when there was some interaction between these groupings, I sought to retain that in mind. In the course of an evening, there might be a general reshuffling of positions. I was not able to remember every movement, but I tried to observe with which members the movements began. And when another spatial arrangement developed, I went through the same mental process as I had with the first.

I managed to make a few notes on trips to the men's room, but most of the mapping was done from memory after I had gone home. At first, I went home once or twice for mapmaking during the evening, but, with practice, I got so that I could retain at least two positional arrangements in memory and could do all of my notes at the end of the evening.

I found this an extremely rewarding method, which well compensated me for the boring routines of endless mapping. As I piled up these maps, it became evident just what the major social groupings were and what people fluctuated between the two factions of the club. As issues arose within the club, I could predict who would stand where.

This observation of groupings did not, in itself, point out the influential people in the club. For that purpose, I tried to pay particular attention to events in which an individual originated activity for one or more others—where a proposal, suggestion, or request was followed by a positive response. Over a period of six months, in my notes I tabulated every observed incident where A had originated activity for B. The result of this for pair events (events involving only two people) was entirely negative. While I might have the impression that, in the relationship between A and B, B was definitely the subordinate individual, the tabulation might show that B originated for A approximately as much as A for B. However, when I tabulated the set events (those involving three or more people), the hierarchical structure of the organization clearly emerged.

In a study of a local union, George Strauss (1952) used similar methods. He attended every meeting of the union throughout a year. While there were hundreds of members in the local, attendance was generally limited to 35 to 50 more or less regulars, so it was not difficult for Strauss to learn the names of each person he was observing. The

officers of the local conducted the meeting from a platform in the front of the hall. The members were seated on both sides of a central aisle. It did not take Strauss long to determine that the seating positions were structured rather than random. He observed the same individuals sitting together from meeting to meeting. He also observed that the seating pattern provided an unofficial separation of the members into two factions, those sitting on one side of the hall supporting the incumbent leaders and those sitting on the other side raising questions and arguments that clearly indicated their opposition. Beyond the formal positions as represented by those on the platform, Strauss observed an important difference in behavior among the rank and file members. Most of them were seated throughout the meeting, but there were a few individuals on both sides of the hall who would get up from time to time, move around, and seek out another member to whisper some message to him. Strauss learned that those who moved around were more influential members than those that sat still. In effect, the movers were initiating action for some of those sitting down, who were later observed to speak up in the meeting. Furthermore, as Strauss observed a shift in the number of members seated on the two sides of the aisle, he was able to predict correctly that the incumbent officers would lose in the next election.

As a footnote to methodological arguments, we should note that Strauss's paper was published in a journal then called *Sociometry*. The editor accepted the article with obvious reservations because he insisted that Strauss change the title of his article, making it "Direct Observation as a Source of Quasi-Sociometric Information." In effect, he was saying that what Strauss really should have done was circulate a questionnaire among the members attending the meeting to ask them what other members they would like to sit next to, and so on. Unfortunately, he had not used the sociometric method, so the editor was willing to settle for the next best thing: observation of actual behavior.

These spatial relations and interaction patterns may remain stable for considerable periods of time, but they do change, and it is important to observe those changes. For example, when the members were planning the annual outing of the Cornerville S&A Club, a member who had previously shown no evidences of leadership spoke up with a glowing description of an amusement park in the suburbs of the city. The members responded favorably to his description, and the club president appointed him to the committee to plan the outing. At the meeting the following week, this suddenly prominent member was not even present. One of the members had discovered that the amusement

park in question had burned down two years ago. Within eight days I had observed major gains and losses of influence in this case. Changes in groupings and in informal group structure can also be important in providing explanations for the behavior and the personal problems of individuals. In Chapter 2, I traced the onset and subsequent resolution of Long John's mental health problems in terms of a series of changes in patterns of interpersonal interaction. This demonstrates that these methods are useful, not only for charting group structures but also for understanding the emotional adjustment of individuals. Note also that these methods can provide systematic quantitative data. Although field observation provides much information that does not lend itself to quantification, it is a serious (but very common) error to assume that observation is simply a *qualitative* method.

CLASSIFYING AND QUANTIFYING VERBAL CONTENT

The study of interpersonal interactions, as illustrated above, can be carried out with minimal attention to the verbal content of the interactions. That is, if we observe a group of men in a street corner conversation and then walking together to the Orpheum Theater, it is important to note who proposed this walk and who endorsed the suggestion. To determine the structure of the group, we need to understand what is said only insofar as it enables us to observe who is initiating changes in activities for whom.

Important as these structural observations are, we will generally wish to go beyond counting to observing, recording, and interpreting the verbal content of conversations. In everyday life we are constantly interpreting such verbal content, but must this be entirely an intuitive operation, subject to no checks on its validity? There is also the question of reliability—the extent to which two observers of the same spoken words would interpret them in the same way. Without some standards of judgment regarding the classification of verbal content, it is impossible to advance beyond personal intuition.

R. Fried Bales (1950) developed a methodology he calls "interaction process analysis." The scheme involves classifying each utterance, gesture, or facial expression in terms of its assumed intent from the standpoint of the speaker. Bales uses six categories to represent expressions of agreement or disagreement, solidarity or tension, and six others centering around the task problems of asking or giving suggestion, opinion, or orientation. The Bales method also involves noting who speaks to whom, and in this way is related to methods concentrating strictly on the quantitative patterns, without verbal content.

Bales and his associates have used this methodology in observing and analyzing small group meetings in the Harvard social laboratory. They have been able to achieve a high enough degree of reliability among different observers and raters that they can claim a scientific foundation for the methodology. However, interaction process analysis is so complex that it takes considerable training for observers to reach a point at which reliability scores are high enough to warrant confidence. Furthermore, the methodology does not lend itself readily to use outside of the small groups laboratory.

In an unpublished study I made some years ago of a discussion group of 21 members in the National Training Laboratory for Group Development in Bethel, Maine, I devised a much simpler method that enabled me to focus particularly on questions of leadership and influence. I recorded who proposed a given action to the group and who supported it. I also noted who gave the proposal conditional support but modified it in some way. I noted who opposed a given proposal and then observed the outcome. In this situation the outcome was not limited to an either/or choice: acceptance or rejection of the proposal. I observed many proposals that seemed to die on the table, with no one speaking up in opposition and no one venturing to offer support.

As I recorded these observations in daily meetings over a two-week period, a clear pattern of leadership and followership and of factional cleavages emerged. Furthermore, my conclusions were supported by sociometric questionnaires in which members of the group put on paper their judgment of who were the most influential members. The combination of observation and the sociometric questionnaire also provided an interesting contrast in evaluations of popularity and influence. For example, I found that the most highly chosen person for leisure time activities did not figure at all among the sociometric ratings for influence. This fit my behavioral observations. I never observed this popular member making any proposal for action, nor did he play a prominent role in either supporting or opposing proposals made by others. While it may seem obvious that there is an important difference between popularity and leadership, in everyday life people all too often fail to recognize the difference.

Since I did not have other observers applying the same methodology on the group I observed, so that I have no evidence regarding the reliability of my simple methodology, I describe it here simply to indicate the possibility of field workers developing their own method of observation and classification, adapted to their own purposes.

The possibilities of using a simplified version of the Bales methodology in the field are illustrated by a study of changes in leadership behavior in a supermarket chain. Top management had committed itself to a program for decentralizing authority and responsibility. This required changes in the relationships between store managers and district managers, who oversaw the operations of a number of supermarkets. In order to determine whether the desired changes had actually taken place, it was necessary for Paul Lawrence and James Clark (1958) to make systematic and quantitative behavioral observations. They observed three district managers in their interactions with store managers.

Lawrence and Clark developed a twofold typology for the content of the verbal interactions. They classified topics as people, merchandise, records systems, physical plant, and small talk. They also categorized each statement as question, information, opinion, direction, or suggestion. They also measured the time that each member of the pair talked.

This scheme of analysis yielded a number of important distinctions. Researchers noted that when "people" were the topic discussed, this led to more talking time by the store manager, since most of the people discussed were working under him. The researchers discovered that, in conversations with their store managers, each district manager (DM) had his favorite topic. DM1 spent 48 percent of his talking time on people, DM2 spent 41 percent of his time talking on merchandise, DM3 spent 47 percent of his time on records systems. They also noted major differences in the amount of time devoted to small talk. DM3 devoted only .5 percent of his time to small talk, compared to 7 percent and 6 percent, respectively, for DM1 and DM2.

Two years later the researchers returned to check for any changes in the interactions between district managers and store managers. This time their observations were necessarily confined to two division managers since DM1 had been promoted. This in itself is of interest. Since the pattern of interaction of DM1 fit far better with the delegation objective of higher management, it is not surprising that he was the one promoted.

The researchers did find major changes in the interactions of DM2 and DM3 with their several subordinates. DM2 markedly reduced his expressions of opinions and suggestions or directions, while his store managers (SMs) showed approximately the same percentages. DM3 reduced his directions or suggestions by more than half, while his SMs

remained constant. The researchers found no significant changes in DM3's proportions of opinions expressed or information offered but observed sharp increases in these categories by the SMs. Furthermore, while the DMs had used approximately three-quarters of the talking time in the first period, in the second period they were down to 55 percent and 62 percent.

A plant manager once described his experience under a former boss who thought he was delegating responsibility and authority:

Of course he talked about delegation. I suppose he went home and told his wife, "We're doing things differently now in the plant. We're delegating."

One day he called me into the office and he said, "Damn it, Ed, we've gotta delegate around here. Now you take this letter from the telephone company and handle it for me. They want to put six more lines in here. Hell, we can't afford it. You tell them that."

I told him I would handle it, but I felt like asking him whether I should bring my letter back for him to sign [Whyte, 1961: 673].

Ridiculous as this case sounds, it illustrates a common management problem. I have never met an executive who didn't believe that he delegated. Most subordinates report that their bosses do not delegate enough. Why these opposing interpretations? The differences arise because the term "delegation" has no commonly accepted *behavioral* definition.

If management people are serious about delegation, they need to adopt behavioral indices reflecting degrees of delegation. The behavioral categories and observational methods of Lawrence and Clark demonstrate the possibility of producing quantitative measures reflecting behavior relevant to achieving any policy of increasing participative (or autocratic) leadership.

WORK FLOW, WORK STATIONS, AND STATUS

Here we are dealing with observations that are so simple to make that we may overlook them altogether. If so, we are likely to miss data basic to understanding group behavior at work. To a considerable extent the social relations at work, and the ability or inability of work groups to stick together and exert pressure on management, will be influenced not only by the nature of the jobs but also by the way work

passes from one work station to another and by the physical location of the work stations (Sayles, 1958).

Jobs vary enormously in the amount of physical movement and social interaction allowed. The noise level is also important, since at some work stations people have to move close together and shout if they are to be understood. On the conventional automotive assembly line, workers have minimum freedom of movement and, while on the line, can only communicate—and that with difficulty—with those on immediately adjoining work stations. At the other extreme, in the control room of the Phillips Petroleum plant I studied, the workers had great freedom of movement since, except in emergencies, the required job activities consumed only a few minutes every hour. The engine operator responsible for monitoring and adjusting the process that furnished the motive power for the operations similarly devoted a minimum of his time to required work activities (again, except in emergencies), but his social situation was far different from that of the control room operators. The engine room was about a hundred yards from the control room, and the engine operator worked alone among his thundering engines.

It makes a difference in interactions whether those stationed close together are working on the same interrelated work operations, or whether each is doing an independent operation. Fully independent operations require no communication among the operators, whereas interrelated operations demand some communication, unless the work is completely machine-paced. On independent jobs it makes a difference whether the job involves direct production or machine tending. Donald Roy's job on the clicking machine required constant attention to the operations so that interaction was only possible during "banana time" and at other work breaks. On the other hand, some machine-tending jobs require work actions only to start and stop operations and, in between, to monitor operations only to be able to intervene when something goes wrong. When the machine is running smoothly, the operator may have considerable freedom of movement.

When people are working in groups, it makes a difference whether the group is hierarchically organized or whether all members have the same job classification and work responsibilities. In a labor gang, division of labor and leadership may arise, but these patterns develop informally. At the other extreme, the work teams in the Steuben Glass division of Corning Glass Works are stratified in formal titles and work responsibilities, with the production process being necessarily under the control of the gaffer, who holds the top position.

In offices, sociologists have long been familiar with the ways in which work location, size and style of desk, easy access to a telephone, and so on reflect the status of employees. The layout and furnishings of executive offices tell us something about the relative importance of the executives.

We tend to take such matters for granted, but we recognize their importance when we discover that organizations can differ from each other in the United States in the degree of emphasis on status distinctions. The importance of this point has been driven home by the recognition that the Japanese policy generally is to minimize the differentiation of status symbols. In the typical large Japanese plant, blue- and white-collar workers, and even many management people, wear the same basic uniform; the executive offices show minimal status distinctions; there is no separate management dining room; and so on. Some see this deemphasis of status distinctions as important in fostering the cohesiveness of the Japanese firm. In any case, status symbols are always important, and the observer can note the presence or absence of physical symbols that reflect status distinctions.

COMBINING INTERVIEWING WITH OBSERVATION

In directing the path-breaking Yankee City study in the 1930s, W. Lloyd Warner emphasized the importance of combining observation and interviewing. Whenever an event can be anticipated, it is important to interview the principal actors both beforehand and afterward. When an important meeting is scheduled, the researcher should talk in advance with those planning the meeting to get them to explain why it is being held, what they hope to accomplish, and what problems they may encounter. Where the researcher has identified prospective participants who are likely to oppose any proposal to be made by the organizers, they also should be interviewed. Following observation of the meeting, it is important to interview the same people again to get their interpretation of what happened and why.

Such interviewing can be exceedingly important as it is not always obvious from observation what is going on in the meeting. We are all familiar with the notion of the *hidden agenda*—objectives never explicitly stated, but that may nevertheless be more important to some of those participating than what people say the meeting is about.

Nor can the observer always readily judge how people feel about a particular issue by the way they speak and by the overt emotional

accompaniment of the words. This is particularly likely to be the case in collective bargaining. For example, we may observe union negotiators pushing a particular demand very vigorously, with a great show of emotion, and then, after an extended and apparently fruitless interchange with management, setting aside that issue and going on to the next item, which they introduce without any emotional freight as if it were simply a minor matter. The expressions of emotion accompanying the two items would indicate that the first issue is of great importance to the union, whereas the second one is relatively minor. In fact, if the researcher has the opportunity to interview union negotiators before the meeting, he may learn that they raised the first issue with the full knowledge that it was going to be impossible to get management to concede on this point. Therefore, they pushed the issue with apparent vigor simply for the purpose of softening up the management people. Recognizing that bargaining involves both give and take, and having refused to give on the first item, managers might feel some subtle pressure to accommodate the union on the second item.

PLACING OBSERVATIONS IN CONTEXT

In stressing the importance of linking interviewing with observation, I have noted that observation alone does not reveal to us what people are trying to accomplish or why they act as they do. Furthermore, interviewing may not lead us to the underlying dynamics in some cases unless we are armed with advance knowledge of the rewards people are seeking or of the penalties they are trying to avoid.

Consider the way the media reported upon the national election in El Salvador in 1982 during the nation's civil war. Reporters provided the following information:

- (1) Leaders of the guerrilla forces declined to offer themselves as candidates. They urged citizens to boycott the election and threatened to disrupt the voting.
- (2) Nevertheless, the election was carried out with a minimum of disruption, and the voter turnout was over 80 percent—far higher than the average turnout in a national election in the United States.

Spokesmen for the Reagan administration hailed this turnout as reflecting the desire of the people for democracy and their rejection of

the Marxist doctrines espoused by guerrilla leaders. Critics argued that the reported number of voters was exaggerated. Be that as it may, the argument misses the critical point: the following information that (so far as I have been able to discover) no U.S. reporter provided the U.S. public around the time of the 1982 election:

- (1) As in many Latin American countries, voting in El Salvador was compulsory.
- (2) To enforce this law, each adult was required to carry a *cedula*, a document to be stamped by election officials when the citizen voted.
- (3) As a means of identification, citizens must have their *cedulas* with them at all times. Government and military officials had the right and the power to inspect these *cedulas* at any time.

What would have happened if a citizen failed to produce a stamped *cedula* upon official demand? Government officials had let it be known that they considered failure to vote an act of treason. The potential consequences of such a judgment would be evident to all citizens, who were aware that the right-wing death squads had murdered thousands of people on the basis of no more substantial evidence.

Armed with this information, we see that the turnout may only mean that people were more afraid of government-related violence if they did not vote than they were afraid of guerrilla violence if they voted. Since there could be no voting in the still relatively small area controlled by the guerrillas, it would be only sensible to be more concerned, where voting did take place, with government reprisals against nonvoters.

Why did reporters miss information so crucial to the understanding of the election? Probably because they unconsciously projected what they were observing against their own cultural background and experience. Here were people turning out to vote in large numbers, in spite of a lack of previous democratic experience, and without any *observable* coercion to make them vote. (By the time of the 1984 election, the conditions I have described had been reported by some journalists.)

The case indicates that when we observe in another culture social processes that appear similar to those with which we are familiar, we should not jump to the conclusion that we know what is going on. Even in our own society, we should interpret what we observe with caution. Beyond contextual interviewing, we need to ask ourselves what would happen if those whom we are observing did not do what we see them

doing. That "what if" question is unanswerable, but it may lead us to the discovery of potential positive and negative sanctions that are not obvious simply from observation.

CONCLUSION

I have argued that observation of behavior is important for research and that the operations involved can be specified, taught, and learned. Naturally, some individuals will be more skilled than others, but we should not think of observation as an activity requiring a rare type of skill. Much of what the observer does can be reduced to routines that are readily learned and practiced.

Field observation is often referred to in the sociological literature under the heading of "qualitative methods." While I do not argue that all important observations can be quantified, researchers have devised reasonably reliable methods to quantify much of the behavior we wish to record.

Finally, before proceeding to a discussion of interviewing, I stress the importance of linking interviewing and observation. Observation guides us to some of the important questions we want to ask the respondent, and interviewing helps us to interpret the significance of what we are observing. Whether through interviewing or other means of data gathering, we need to place the observed scene in context, searching for the potential positive or negative sanctions, which are not immediately observable but may be important in shaping behavior.

6

Interviewing Strategy and Tactics

Interviews may be of various types, ranging from the orally administered interview schedule of predetermined questions to the more freely structured interview common to studies in social anthropology.

In the present chapter I shall give only incidental attention to questionnaires and interview schedules, since they are systematically discussed in a number of books. I shall concentrate upon the method in which the interviewer does *not* follow a standard order and wording of questions.

NATURE OF THE INTERVIEW

The interview we use is often called "nondirective." This is a misnomer. The nondirective interview was a therapeutic development based on the theory that patients would make progress best if left free to express themselves on their problems as they wished, stimulated by an interested and sympathetic listener.

The good research interview is structured in terms of the research problem. The interview structure is not fixed by predetermined questions, as in the questionnaire, but is designed to provide the informant with freedom to introduce materials that were not anticipated by the interviewer.

Whatever its merits for therapy, a genuinely nondirective interviewing approach simply is not appropriate for research. Far from putting informants at their ease, it actually produces anxieties. Once, while

Author's Note: Some of the material in Chapters 6 and 7 is from Whyte (1960).