Chapter 3

Access

The problem of obtaining access to the data one needs looms large in ethnography. It is often at its most acute in initial negotiations to enter a setting and during the 'first days in the field'; but the problem persists, to one degree or another, throughout the data collection process.

In many ways, gaining access is a thoroughly practical issue. As we shall see, it involves drawing on the interpersonal resources and strategies that we all tend to develop in dealing with everyday life. But the process of achieving access is not merely a practical matter. Not only does its achievement depend upon theoretical understanding, often disguised as 'native wit', but the discovery of obstacles to access, and perhaps of effective means of overcoming them, itself provides insights into the social organization of the setting.

The work of Barbera-Stein (1979) illustrates this. Her fieldwork was undertaken in several different therapeutic or day-care centres for pre-school children. The original research design foundered because access was denied to several settings. She writes in retrospect of her experience: 'The access negotiations can be construed as involving multiple views of what is profane and open to investigation vs what is sacred or taboo and closed to investigation unless the appropriate respectful stance or distance is assumed' (Barbera-Stein 1979:15). She ties this observation to particular settings and particular activities in them:

> I had requested the permission to observe what the psychoanalytic staff considered sacred. In their interactions with emotionally disturbed children, they attempted to establish effective bonds modelled after the parent–child bond. This

> was the first step in their attempts to correct the child's faulty emotional development. This also was the principal work of the social workers at the day-care centre. Formal access to the day-care centre initially was made contingent upon my not observing on Tuesdays and Thursdays when the social workers engaged the children in puppet play sessions. Puppet play was used as a psychological projective technique in monitoring and fostering the emotional development of the children.
>
> (Barbera-Stein 1979:15)

Even after eight months of fieldwork, and after some renegotiation, access to such 'sacred' puppet-play sessions was highly restricted. Barbera-Stein was allowed to observe only three sessions and was forbidden to take notes.

In contrast, Barbera-Stein herself assumed that interactional data on families in the home would be highly sacred, and did not initially request access to such information. In fact it turned out that this was not regarded as problematic by the social workers, as they viewed working with families as their stock-in-trade, and it was an area in which they were themselves interested. Her experience illustrates, incidentally, that while one must remain sensitive to issues of access to different domains, it is unwise to allow one's plans to be guided entirely by one's own presuppositions concerning what is and is not accessible.

Negotiating access, data collection and analysis are not, then, distinct phases of the research process. They overlap significantly. Much can be learned from the problems involved in making contact with people as well as from how they respond to the researcher's approaches.

ENTRY TO SETTINGS

Access is not simply a matter of physical presence or absence. It is far more than the granting or withholding of permission for research to be conducted. Perhaps this can be illustrated by reference to research where too literal a notion of access would be particularly misleading. It might be thought that problems of access could be avoided if one were to study 'public' settings only, such as streets, shops, public transport vehicles, bars, and similar locales. In one sense this is true. Anyone can, in principle, enter such public domains; that is what makes them public. No process of negotiation is required for that. On the other hand, things are not necessarily so straightforward. In many settings, while physical presence is not in itself problematic, appropriate activity may be so.

Among other things, public domains may be marked by styles of social interaction involving what Goffman (1971) terms 'civil inattention'. Anonymity in public settings is not a contingent feature of them, but is worked at by displays of a studied lack of interest in one's fellows, minimal eye contact, careful management of physical proximity, and so on. There is, therefore, the possibility that the fieldworker's attention and interest may lead to infringements of such delicate interaction rituals. Similarly, much activity in public settings is fleeting and transient. The fieldworker who wishes to engage in relatively protracted observations may therefore encounter the problem of managing 'loitering', or having to account for himself or herself in some way.

Some examples of these problems are provided in Karp's (1980) account of his investigation of the 'public sexual scene' in and around Times Square in New York, particularly in pornographic bookshops and cinemas. Admittedly, this is a very particular sort of public setting, in that a good deal of what goes on may be 'disreputable' and the behaviour in public correspondingly guarded. Karp tried various strategies for achieving access and initiating interaction. He tried to negotiate openly with some bookshop managers, but failed. Similarly, after a while, regulars on the street interpreted his hanging around in terms of his being a hustler, or a cop. He also reports failure to establish relationships with prostitutes, although his fieldnotes display what seems a rather clumsy and naive approach to this.

Karp resolved his problems to some extent by realizing that they directly paralleled the interactional concerns of the participants themselves, and he was able to draw on his access troubles for analytical purposes in that light. He quotes a research note to this effect:

> I can on the basis of my own experience substantiate, at least in part, the reality of impression-management problems for persons involved in the Times Square sexual scene. I have been frequenting pornographic bookstores and movie theatres for some nine months. Despite my relatively long experience I have not been able to overcome my uneasiness during activity in these contexts. I feel, for example, nervous at the prospect of entering a theatre. This nervousness expresses itself in increased heartbeat. I consciously wait until few people are in the vicinity before entering; I take my money out well in advance of entering; I feel reticent to engage the female ticket seller in even the briefest eye contact.
>
> (Karp 1980:94)

In the face of such interactional constraints, Karp decided to resort to observation alone, with minimal participation beyond casual conversation. He concludes by pointing out that such public settings may be as constraining for a researcher as any organizational setting.

To a considerable extent Karp's is an account of relative failure to establish and maintain working 'presence' and relationships, although he learns from his problems. One should not conclude from his experience, however, that 'loitering' can never lead to workable research conditions. West writes about the value of such apparently casual approaches: he 'met both . . . referred delinquents and others by frequenting their hangouts, such as stores, pool halls, restaurants, and alleys, and by trying to strike up casual acquaintanceship'; though he comments that 'some boldness and a tough-skinned attitude to occasional personal rejection were helpful, in addition to skills in repartee, sports, empathy, and sensitivity'. He reports that 'after a few visits or perhaps a couple of weeks, I became recognized as something of a regular, and usually had managed to strike up conversations with a few youngsters' (West 1980:34).

As in the case of West's research, some individuals and groups who one might want to study may be available in public settings. However, they are not always welcoming to researchers, or indeed to outsiders of any kind. Sometimes very extensive 'hanging about', along with lucky breaks, is necessary before access is achieved, as Wolf's experience illustrates:

> As a new graduate student in anthropology at the University of Alberta, Edmonton, I wanted to study the 'Harleytribe'. It was my intent to obtain an insider's perspective of the emotions and the mechanics that underlie outlaw bikers' creation of a subcultural alternative. . . . I customised my

Norton, donned some biker clothing, and set off to do some fieldwork. My first attempts at contacting an outlaw club were near-disasters. In Calgary I met several members of the Kings Crew MC in a motorcycle shop and expressed an interest in 'hanging around'. But I lacked patience and pushed the sitation by asking too many questions. I found out quickly that outsiders, even bikers, do not rush into a club, and that anyone who doesn't show the proper restraint will be shut out.

Following this, Wolf bought himself a new bike, and approached a new group, the Rebels, in a 'final make-it-or-forget-it attempt'. He writes that he sat in a bar watching them and working out how to approach them:

> I discovered that I was a lot more apprehensive than I thought as I sat at the opposite end of the Kingsway Motor Inn and watched the Rebels down their drinks. The loud thunder of heavy-metal rock music would make initiating a delicate introduction difficult, if not impossible, and there were no individual faces or features to be made out in the smoky haze, only a series of Rebel skull patches draped over leather jackets in a corner of the bar that outsiders seemed to avoid warily. . . . I decided to go outside and devise an approach strategy, including how I would react if one of the Rebels turned to me and simply said 'Who invited you?'. I had thought through five different approaches when Wee Albert of the Rebels MC came out of the bar to do a security check on the 'Rebel iron' in the parking lot. He saw me leaning on my bike and came over to check me out. For some time Wee Albert and I stood in the parking lot and talked about motorcycles, riding in the wind, and the Harley tradition. He showed me some of the more impressive Rebel choppers and detailed the jobs of customizing that members of the club had done to their machines. He then checked out my 'hog', gave a grunt of approval, and invited me to come in and join the Rebels at their tables. Drinking at the club bar on a regular basis gave me the opportunity to get to know the Rebels and gave them an opportunity to size me up and check me out on neutral ground. I had made the first of a long sequence of border crossings that all bikers go through if they hope to get close to a club.
>
> (Wolf 1991:212–15)

Making contact in public settings with people one wishes to study can be a difficult and protracted process, then; though Wolfe's experience is undoubtedly extreme.

Sometimes, initial contacts may completely transform research plans. Liebow (1967) reports that on his first day he fell into conversation with some of the onlookers present at a scuffle between a policeman and a woman. This led into several hours of talk with a young man. This he wrote up, and in retrospect he comments:

> I had not accomplished what I set out to do, but this was only the first day. And, anyway, when I wrote up this experience that evening, I felt that it presented a fairly good picture of this young man and that most of the material was to the point. Tomorrow, I decided, I would get back to my original plan – nothing had been lost. But tomorrow never came.
>
> (Liebow 1967:238)

The 'original plan' that Liebow was cherishing initially was to do several small studies, 'each covering a strategic part of the world of the low-income male': a neighbourhood study, a labour union, and a bootleg joint, perhaps supplemented by some life-histories and genealogies. In the event, however, in the first neighbourhood he tried,

> I went in so deep that I was completely submerged and any 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 any fieldwork – 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 1967:236–7)

On the second day of his fieldwork, Liebow returned to the scene of his first encounter. Again he fell into conversation, with three 'winos' in their forties, and a younger man 'who looked as if he had just stepped out of a slick magazine advertisement' (1967, 238–9). This younger man was Tally Jackson, who acted as Liebow's sponsor and confidant, and on whose social circle the research came to be focused.

Now Liebow's study is an impressive and important contribution to urban ethnography, but there are danger signals in his account of the fieldwork. It may or may not have been a good idea to abandon his original intentions of conducting several small, related projects. On the other hand, it may not have been such a good idea to have, as it appears, surrendered himself so thoroughly to the chance meeting with Tally and its consequences. As Liebow himself remarks, 'the principal lines of my fieldwork, were laid out, *almost without my being aware of it*' (1967:237; our emphasis). Here, rather than the research problem being transformed in response to opportunities arising in the course of the research, and the research design being modified accordingly, Liebow seems to have abandoned systematic research design altogether.

Nevertheless, Liebow's research illustrates the significance of informal 'sponsorship'. Tally vouchsafed for him, introduced him to a circle of friends and acquaintances, and so provided access to data. The most famous of such 'sponsors' in the field is undoubtedly 'Doc' who helped in Whyte's study of 'corner boys' (Whyte 1981). Whyte's methodological appendix is a classic description of the serendipitous development of a research design, and the influence of Doc was a major determinant in its evolution. Doc agreed to offer Whyte the protection of friendship, and coached him in appropriate conduct and demeanour.

Liebow's and Whyte's contacts with their sponsors were quite fortuitous. However, sponsorship of a similar kind may be gained through the mobilization of existing social networks, based on acquaintanceship, kinship, occupational membership, and so on. This is not always straightforward, however. Cassell reports the difficulties she had in negotiating access in a study of surgeons, and her reliance on personal and occupational networks:

> When I decided to study surgeons, I negotiated for the better part of a year with a representative of the Department of Surgery, at a hospital where my ex-husband was an attending physician, before the Chief of Surgery definitively refused to allow me access to his department.
>
> At the same time, after spending six months obtaining an interview with a representative of the American College of Surgeons, I flew to Chicago to ask for advice and possible sponsorship from this prestigious group. After a charming Southern surgeon, in his sixties, indulged in an hour of small-talk, I broke in and asked if he thought my study was worth doing. Silence. 'Your husband is a doctor?' he finally inquired. When I assented, he said: 'Have you ever thought of . . . I mean, with your background, you'd be such an asset . . . has it ever occurred to you to become active in the Ladies Auxiliary of your husband's hospital?' This was the only advice I received.
>
> Eventually, at almost the last minute, when a reviewer for the agency that eventually funded my study asked for proof that I had access to surgeons, a friend of my ex-husband said that I could do research in the hospital where he was Chief of Surgery (and wrote a letter to that effect).
>
> (Cassell 1988:94)

Hoffman (1980) also provides insight into the way in which personal networks can be used, while drawing attention once more to the relationship between problems of access and the quality of the data subsequently collected. Her research was concerned with a locally influential elite – members of boards of hospital directors in Quebec. In the first place she notes a general problem of access to such an elite:

> Introducing myself as a sociology graduate student, I had very limited success in getting by the gatekeepers of the executive world. Telephone follow-ups to letters sent requesting an interview repeatedly found Mr X 'tied up' or 'in conference'. When I did manage to get my foot in the door, interviews rarely exceeded a half hour, were continually interrupted by telephone calls (for 'important' conferences, secretaries are usually asked to take calls) and elicited only 'front work' (Goffman 1959), the public version of what hospital boards were all about.
>
> (Hoffman 1980:46)

During one interview, however, Hoffman's informant discovered that he knew members of her family. This gave rise to a very different sort of interview, and more illuminating data:

> The rest of the interview was dramatically different than all my previous data. I was presented with a very different picture of the nature of board work. I learned, for example, how board members used to be recruited, how the executive committee kept control over the rest of the board, how business was conducted and of what it consisted, and many other aspects of the informal social organization of board work.
>
> (Hoffman 1980:46–7)

Abandoning her original research design – based on interviewing a representative sample from different institutions – Hoffman therefore started to select informants on the basis of social ties. She began with direct personal contacts, and then asked those acquaintances to refer her to other informants, and so on. This strategy, she concludes, produced 'more informative and insightful data'.

Hoffman graphically juxtaposes typical responses to illustrate the point:

Response to an Unknown Sociologist	*Response to a Known Individual*
Board Member A	Board Member B

Q. *How do you feel in general about how the board has been organized?*

I think the basic idea of participation is good. We need better communication with the various groups. And I think they probably have a lot to offer.	This whole business is unworkable. It's all very nice and well to have these people on the board, they might be able to tell us something here and there, or describe a situation, but you're not going to run a hospital on that!

Q. *How is the new membership working out? Do they participate? Any problems?*

... oh yes, Mr. X (orderly) participates. He asked something today, now what was it? Sometimes they lack skill and experience, but they catch on. There is no problem with them. We get along very well.	Mr. X (orderly) hasn't opened his mouth except for a sandwich ... But what can he contribute? ... You could rely on the old type of board member ... you knew you could count on him to support you. You didn't have to check up all the time. But these new people, how do you know how they will react? Will they stick behind you? And there is the problem of confidentiality. Everything you say you know will be all over the hospital ten minutes after the meeting. You can't say the same things anymore. You have to be careful in case someone interprets you as being condescending or hoity-toity.

(Hoffman 1980:48–9)

Hoffman tends to portray the issue of access here in terms of 'penetrating informants' fronts', and clearly contrasts the two varieties of data in terms of aiming for 'better' and more truthful accounts. This can be problematic: 'frankness' may be as much a social accomplishment as 'discretion', and we shall return to the problem of the authenticity of accounts later. But Hoffman's discussion dramatically focuses attention on the relationships between 'access', the fieldworker's perceived identity, and the data that can be gathered.

GATEKEEPERS

Cassell's and Hoffman's accounts take us towards those 'formal', 'private' settings where boundaries are clearly marked, are not easily penetrated, and may be policed by 'gatekeepers'. In formal organizations, for example, initial access negotiations may be focused on official permission that can legitimately be granted or withheld by key personnel. Although not necessarily the case, such gatekeepers are often the ethnographer's initial point of contact with such research settings.

It should be said, though, that identifying the relevant gatekeepers is not always straightforward. Indeed, the distinction between sponsors and gatekeepers is by no means clear-cut. Even in formal bureaucratic organizations it is not always obvious whose permission needs to be obtained, or whose good offices it might be advisable to secure. Gouldner reports precisely this kind of problem in his research on the Oscar Center gypsum plant. He recounts that the research team

> made a 'double-entry' into the plant, coming in almost simultaneously by way of the Company and the Union. But it soon became obvious that we had made a mistake, and that the problem had not been to make a double-entry, but a triple-entry; for we had left out, and failed to make independent contact with a distinct group – the management of that particular plant. In a casual way, we had assumed that main office management also spoke for the local plant management and this, as a moment's reflection might have told us, was not the case. In consequence our relations with local management were never as good as they were with the workers or the main office management.
>
> (Gouldner 1954:255–6)

Knowing who has the power to open up or block off access, or who consider themselves and are considered by others to have the authority to grant or refuse access, is, of course, an important aspect of sociological knowledge about the setting. However, this is not the catch-22 situation it might appear. For one thing, as we argued in Chapter 1, research never starts from scratch; it always relies on common-sense knowledge to one degree or another. We may already know sufficient about the setting to be able to judge what the most effective strategy is likely to be for gaining entry. If we do not, we may be able to 'case' the setting beforehand, for example by contacting people with knowledge of it or of other settings of a similar type. This will often solve the problem, though as Whitten (1970) found out in his research on black communities in Nova Scotia, there is no guarantee that the information provided is sound. He was told by local people that he should phone the councillor for the

largest settlement, that to try to meet him without phoning would be rude. He did so, 'with disastrous results':

> I introduced myself as an anthropologist from the United States, interested in problems encountered by people in rural communities in different parts of the Americas. Following procedures common in the United States and supported by educated Nova Scotians, I said that I was particularly interested in Negro communities kept somewhat outside of the larger social and economic system. I was told, politely, but firmly, that the people of the rural Dartmouth region had had enough of outsiders who insulted and hurt them under the guise of research, that the people of the region were as human as I, and that I might turn my attention to other communities in the province. I was asked why I chose 'Negroes' and when I explained that Negroes, more than others, had been excluded from full participation, I was again told that the people of rural Nova Scotia were all alike, and that the colored people were tired of being regarded as somehow different, because there was no difference.
>
> (Whitten 1970:371)

Whitten discovered that he had made two basic mistakes:

> First, when Nova Scotians tell one to first call the official responsible for a community, they are paying due respect to the official, but they do not expect the investigator to take this advice. They expect that the investigator will establish an enduring contact with someone who can introduce him to the official. Crucial to this procedure is that the investigator be first known to the person who will make the introduction, for the middleman may be held responsible for the investigator's mistakes. The recommendation to call relieves anyone from the responsibility for the call, and hence it is not expected that a person will follow this advice. Second, it is not expected that one will use the term 'Negro' in referring to Nova Scotians ethnically identified as colored. The use of ethnic terminology (including the term 'colored') is reserved for those who are already a part of the system. . . .
>
> The most effective way to approach an official, we found, is to recognize no ethnic distinctions whatsoever, thereby forcing the official to make the preliminary distinction (e.g.

> between colored community and white community). By so doing the investigator is in a position to immediately inquire as to the significance of ethnicity. Had we acted a bit more slowly, and ignored ethnic differences, we might have succeeded in gaining early entrée, but we erred by assuming that we knew the best way to do things in Anglo-America. By talking too much, and not reflecting carefully on the possible connotations attached to our 'instructions', our work bogged down for a time.
>
> (Whitten 1970:371–2)

Whether or not they grant entry to the setting, gatekeepers will generally, and understandably, be concerned as to the picture of the organization or community that the ethnographer will paint, and they will have practical interests in seeing themselves and their colleagues presented in a favourable light. At least, they will wish to safeguard what they perceive as their legitimate interests. Gatekeepers may therefore attempt to exercise some degree of surveillance and control, either by blocking off certain lines of inquiry, or by shepherding the fieldworker in one direction or another.

As an illustration of one way in which gatekeepers may try to influence things, Bogdan and Taylor report:

> We know one novice who contacted a detention home in order to set up a time to begin his observation. The supervisor with whom he spoke told him that he wouldn't be interested in visiting the home that day or the next because the boys would just be making Hallowe'en decorations. He then suggested which times of the day would be best for the observer to 'see something going on'. The observer allowed himself to be forced to choose from a limited number of alternatives when he should have made it clear that he was interested in a variety of activities and times.
>
> (Bogdan and Taylor 1975:44–5)

Although Bogdan and Taylor report this as happening to a novice, it often remains a problem for even the most experienced fieldworker. (In this instance, the ethnographer needs to explain that he or she is willing or even eager to sample the mundane, the routine, or perhaps the boring aspects of everyday life.)

One of the difficulties regularly faced in this context arises from the fact that it is often precisely the most sensitive things that are of most *prima facie* interest. Periods of change and transition, for example, may be perceived as troublesome by the participants themselves, and they may wish, therefore, to steer observers away from them: the conflict of interest arises from the fact that such disruptions can be particularly fruitful research opportunities for the fieldworker.

The issue of 'sensitive' periods is something that Ball (1980) explicitly remarks on in the context of a discussion of initial encounters in school classrooms. He notes that researchers have tended to devote attention to classrooms where patterns of interaction are already well established. Hence there is a tendency to portray classroom life in terms of fixed, static models. The pictures of classroom interaction with which we are familiar, Ball argues, may be artefacts of the preferred research strategy. He goes on to note:

> The problem is that most researchers, with limited time and money available to them, are forced to organise their classroom observations into short periods of time. This usually involves moving into already established classroom situations where teachers and pupils have considerably greater experience of their interactional encounters than does the observer. Even where the researcher is available to monitor the initial encounters between a teacher and pupils, the teacher is, not unreasonably, reluctant to be observed at this stage.
>
> But the reasons for the teacher's reluctance are exactly the reasons why the researcher should be there. These earlier encounters are of crucial significance not only for understanding what comes later but in actually providing for what comes later.
>
> (Ball 1980:143–4)

Here, then, Ball neatly draws attention to a particular problem of access, and shows how this is not simply a practical matter of organizing the fieldwork (though it is that too), but also bears on issues of descriptive accuracy and analytical adequacy.

TO DECEIVE OR NOT TO DECEIVE?

Sometimes, of course, it may be judged that the relevant gatekeepers will almost certainly block entry altogether. Here, resort may be made to secret research. (We discuss the ethical issues surrounding covert research in Chapter 10.) Holdaway (1982) provides an example from his work on the police. As a serving officer who was seconded to university to read sociology and returned to the force wishing to do research on it, he was faced with six options:

> A Seek the permission of the chief officer to research, giving full details of method and intention.
> B Seek permission as above, so phrasing the research description that it disguised my real intentions.
> C Seek permission of lower ranks, later requesting more formal acceptance from senior officers.
> D Do no research.
> E Resign from the police service.
> F Carry out covert research.
>
> I chose the final option without much difficulty. From the available evidence, it seemed the only realistic option; alternatives were unrealistic or contained an element of the unethical which bore similarity to covert observation. I believe that my senior officers would have either refused permission to research or obstructed me. Option B is as dishonest a strategy as covert research, if the latter is thought dishonest. For example, if I were a Marxist and wanted to research the police and declared my Marxism, I know that I would be denied research access; yet to 'front' myself in a different research guise is surely dishonest. Option C could not have been managed. D denies the relevance of my studies, and Option E would have been its logical progression – yet I felt an obligation to return to the police who had financed my study.
>
> (Holdaway 1982:63)

Holdaway was in the unusual position of knowing the setting he wanted to research, and the gatekeepers who could give him permission to do the research, very well indeed. Often, however, judgments that access to a setting is impossible are less well founded. There are some settings to which one might expect entry to be blocked but that have been shown to be accessible, at least to some degree. For example, Fielding (1982) approached an extreme right-wing political organization, the National Front, for permission to carry out research on their organization, and received it – though he felt it necessary to supplement official access with some covert observation.

Indeed, there is often a considerable amount of uncertainty and variation in the scope for negotiating access. Shaffir was told that the Tasher Hassidic community he was interested in studying would not agree to be researched. He was advised to get a job in the community and do covert research, which he did:

> Since I suspected that members of the community would not sanction my sociological investigation, I did not inform the Tasher that I was collecting data about them. (Neither did I tell them about my connection with the Lubavitcher, a community they disapproved of because of the involvement of its members with non-Orthodox Jews.) I did, however, tell those who were interested that I was a sociology student at McGill University. Invariably, I was asked to explain the meaning of 'sociology', a term that was entirely foreign to the Tasher. . . . But I was able to define it sufficiently to use my interest in sociology to add legitimacy to the kinds of questions I regularly asked about the organization of the community. . . . Some people were surprised at my curiosity about topics unconnected with my clerical duties. However, others seemed convinced by my explanations and volunteered information about themselves which they believed might interest an outsider. But several members looked at me so oddly that I felt they considered me an intruder and were (quite rightly!) suspicious of my presence.
>
> (Shaffir 1985:126)

Shaffir found his covert role a severe constraint on his research, and experienced great difficulty in combining a full-time clerical job with his university studies. He decided to reduce his hours of work, explaining this to his Tasher employers on the grounds that

> my commitments at the university required me to conduct research and to write a thesis. That thesis, I explained, would

probably be about pool halls. 'Pool hall, what is that?', asked the rabbi in Yiddish. The other man, who had graduated from university before becoming a Tasher Hassid, gave his version of a pool hall, 'It's a place where you play with balls on a table', and turning to me, he asked: 'How can I describe a pool hall to him? He's never been'. Then he elaborated: 'It's a dirty place that attracts the criminal element. It's suitable for Gentiles, not for Jews.'

They both quickly agreed that I ought to be discouraged from pursuing that research and suddenly the rabbi said, 'Look, you know us. Why don't you write about us and we could help you . . . I'm telling you, you'll win a prize. I'll help you and so will the others and you'll win an award . . . When do you want to start? Let's set a time.' The other man seemed to be of the same opinion. Stunned, I managed to say calmly that I would consider the suggestion and meet them the next day to pursue it further.

Of course, I intended to tell them that I would do as they advised. By the following afternoon, however, both men had changed their mind. . . . That was the end of my first attempt at fieldwork among the Tasher.

I was to be more successful a few years later in the same Tasher community. There were new administrators in charge of the community's day-to-day affairs who were quite receptive to my request to visit and chat about matters of community life that interested me. I candidly explained my research interests to them. . . . The chief administrator appeared to adopt a 'We have nothing to hide' attitude.

(Shaffir 1985:128–9)

Rather surprisingly, perhaps, Chambliss recounts a more straightforward process of gaining access to the world of organized crime, but once again one relying on an initial covert approach:

I went to the skid row, Japanese, Filipino, and Black sections of Seattle dressed in truck driver's clothes. . . . Sitting in the bar of a café one day I noticed several people going through a back door. I asked the waitress, Millie – a slight, fortyish ex-prostitute and sometime-drug-user with whom I had become friends – where these people were going:

MILLIE: To play cards.
ME: Back there?
MILLIE: Yes, that's where the poker games are.
ME: Can I play?
MILLIE: Sure. Just go in. But watch your wallet.

So I went, hesitantly, through the back door and into a large room which had seven octagonal, green felt covered tables. People were playing five card stud at five of the tables. I was immediately offered a seat by a hand gesture from the cardroom manager. I played – all the time watching my wallet as I had been advised.

I went back every day for the next week. . . . In conversation with the cardroom manager and other players I came to realize (discover?) what any taxicab driver already knew: that pornography, gambling, prostitution, and drugs were available on practically every street corner. So I began going to other cafés, card-rooms, and bars. I played in many games and developed a lot of information just from casual conversation.

Within a week I was convinced that the rackets were highly organized. The problem became one of discovering how, and by whom. I was sitting talking to Millie on the 30th of the month when a man I recognized as a policeman came through the door and went into the manager's office. I asked Millie what he was doing:

MILLIE: He's the bag man.
ME: The what?
MILLIE: The bag man. He collects the payoff for the people downstairs.
ME: Oh.

I spent the next two months talking informally to people I met at different games, in pornography shops, or on the streets. I soon began to feel that I was at a dead end. . . . I had discovered the broad outlines of organized crime in Seattle, but how it worked at the higher level was still a mystery. I decided it was time to 'blow my cover'.

I asked the manager of the cardroom I played in most to go to lunch with me. I took him to the faculty club at the University of Washington. This time when he saw me I was shaven and wore a shirt and tie. I told him of my 'purely scientific' interests and experience and, as best I could, why I had deceived him earlier. He agreed to help. Soon I began receiving phone calls: 'I understand you are interested in Seattle. Did you ever think to check Charles Carroll's brother in law?' And there was one honest-to-God clandestine meeting in a deserted warehouse down at the wharf. . . .

Over the next ten years I pursued this inquiry, widening my contacts and participating in an ever larger variety of rackets. As my interest in these subjects and my reliability as someone who could be trusted spread, I received more offers to 'talk' than I had time to pursue.

(Chambliss 1975:36–8)

The work of Holdaway, Fielding, Shaffir, and Chambliss raises the question of deception in negotiations over access. Where the research is secret to all those under study, and to gatekeepers too, the problem of access may be 'solved' at a stroke, providing the deception is not discovered. Even when 'cover' is successfully maintained, though, the researcher engaging in covert research has to live with the moral qualms, anxieties, and practical difficulties to which the use of this strategy may lead. However, research carried out without the knowledge of anyone in, or associated with, the setting is quite rare. Much more common is that some people are kept in the dark while others are taken into the researcher's confidence, at least partly.

What is at issue here, though, is not just whether permission to carry out the research is requested, and from whom, but also what those concerned are told about it. Some commentators recommend that an explicit research bargain, spelling out in full the purposes of the research and the procedures to be employed, be made with all those involved, right from the start. Often, though, this is neither possible nor desirable. Given the way in which research problems may change over the course of fieldwork, the demands likely to be made on people in the setting and the policy implications and political consequences of the research are often a matter for little more than speculation at the outset. There is also the danger that the information provided will influence the behaviour of the people under study in such a way as to invalidate the findings. While often it may be judged that the chances of this are small, given the other pressures operating on these people, there are instances where

it may be critical. Had Festinger *et al.* (1956) informed the apocalyptic religious group they were studying not only that the research was taking place but also about the hypothesis under investigation, that would almost certainly have undermined the validity of their research.

The other argument for not always providing a full account of one's purposes to gatekeepers and others at the beginning of the research is that unless one can build up a trusting relationship with them relatively rapidly, they may refuse access in a way that they would not do later on in the fieldwork. Wolf's study of bikers, in which he spent three years hanging out with them before he raised the question of doing research, is an extreme but instructive example (Wolf 1991). Once people come to know the researcher as a person who can be trusted to be discreet in handling information within the setting, and who will honour his or her promises of anonymity in publications, access may be granted that earlier would have been refused point blank. On this argument it is sometimes advisable not to request at the outset the full access to data one will eventually require but to leave negotiation of what seem to be the more delicate forms of access till field relationships have been established – though we should perhaps reiterate that assumptions about what is and is not delicate may not always prove reliable.

Nevertheless, while telling the 'whole truth' in negotiating entry for research, as in most other social situations, may not always be a wise or even a feasible strategy, deception should be avoided wherever possible, not just for ethical reasons but also because it can rebound badly later on in the fieldwork. Indeed, sometimes it may be necessary to warn gatekeepers or sponsors of possible consequences of the research to avoid problems subsequently, as Geer notes from her research on American colleges:

> In colleges of high prestige, the researcher may be hampered in his negotiations because the administrators cannot imagine that anything harmful to the college could be discovered. In this case, it is up to the researcher to explain the kinds of things that often turn up – homosexuality, for example, or poor teaching. The administrator can sometimes be drawn into a scientific partnership. By treating him as a broadminded and sophisticated academic, one gradually works him around

> to a realization that although the study may be threatening, he and his college are big enough to take it. It may seem unnecessary to prepare administrators for the worst in this fashion, but it prepares the ground for the shock they may get when they see the manuscript at the end of a study. Administrators may attempt to prevent publication or feel that the college has been exploited and similar research should not be authorized. However, the administrator who has committed himself to a generous research bargain is more likely to be proud of the results.
>
> (Geer 1970:83)

Negotiating access is a balancing act. Gains and losses now and later, as well as ethical and strategic considerations, must be traded off against one another in whatever manner is judged to be most appropriate, given the purposes of the research and the circumstances in which it is to be carried out.

OBSTRUCTIVE AND FACILITATIVE RELATIONSHIPS

Seeking the permission of gatekeepers or the support of sponsors is often an unavoidable first step in gaining access to the data. And the relationships established with such people can have important consequences for the subsequent course of the research. Berreman, discussing his research on a Pahari village in the Himalayas, reports:

> We were introduced [to the villagers] by a note from a non-Pahari wholesaler of the nearest market town who had long bought the surplus agricultural produce of villagers and had, as it turned out, through sharp practices of an obscure nature, acquired land in the village. He asked that the villagers treat the strangers as 'our people' and extend all hospitality to them. As might have been expected, our benefactor was not beloved in the village and it was more in spite of his intercession than on account of it that we ultimately managed to do a year's research in the village.
>
> (Berreman 1962:6)

Equally, though, one can be fortunate in one's associations with gatekeepers:

> The impression I received of people's attitudes to me was that

> they were very curious and very friendly. As I walked along country paths I was constantly being bothered by inquisitive peasants who had no inhibitions in talking about their problems, especially in relation to the land. It took at least an hour to cross from one side of the village to the other due to the constant need to stop and converse. This contrasts markedly to reports I had received from anthropologists who have worked in Quechua-speaking areas of Peru and have found people dour and uncommunicative. I believe one reason for this is that my introductions into the area were exceptionally good. On the one hand, my official introductions through the Ministry of Agriculture had come through the one official who was not distrusted. He was referred to as 'a good person, he didn't try to cheat us like the other officials'. On the other hand, I had introductions through, and for a time lived in the same building as, members of the progressive Catholic Church. They also happened to be Europeans. Their identification with the peasants, and people's identification of me with them, was extremely valuable.
>
> (Rainbird 1990:89)

However, even the most friendly and co-operative of gatekeepers or sponsors will shape the conduct and development of the research. To one degree or another, the ethnographer will be channelled in line with existing networks of friendship and enmity, territory and equivalent 'boundaries'. Having been 'taken up' by a sponsor, the ethnographer may find it difficult to achieve independence from such a person, discovering that his or her research is bounded by the social horizon of a sponsoring group or individual. Such social and personal commitments may, like gatekeepers' blocking tactics, close off certain avenues of inquiry. The fieldworker may well find him- or herself involved in varieties of 'patron–client' relationship with sponsors, and in so doing discover influence exerted in quite unforeseen ways. The ambiguities and contingencies of sponsorship and patronage are aptly illustrated by two similar studies from rural Spain (Barrett 1974; Hansen 1977).

Barrett reports that the members of his chosen village, Benabarre, were initially reserved. This was partially breached when a village baker started to take Barrett round and introduce him to others. However, the big breakthrough came when the village was visited by a Barcelona professor who was descended from a Benabarre family. The professor was interested in Barrett's work and spent a good deal of time with him:

> Nothing could have had a more beneficial effect on my relations with the community. Don Tomás enjoys immense respect and popularity among the villagers, and the fact that he found my work significant was a behavioural cue to a great many people. The reasoning was apparently that if I were someone to beware of, Don Tomás would not be fooled; if he believed I was the genuine article, then I must be! The response was immediate. Doors which until then had been closed to me opened up; new people greeted me on the streets and volunteered their services.
>
> (Barrett 1974:7)

Barrett realized that this was not simply a lucky breakthrough; it was also an important clue to social relationships in the village. Hierarchical relationships were of fundamental importance. Initially, Barrett had avoided close association with the 'upper crust' families:

> I thought that if there were polarization between the social strata this might make it more difficult later to win acceptance among the peasants. It was virtually the opposite! The fact that I was not associating with those who were considered my peers was simply confusing, and made it vastly more difficult to place me in the social order. Once Don Tomás extended his friendship, and introduced me to other families of similar social rank, this served almost as a certificate of respectability.
>
> (Barrett 1974:8)

Hansen's experiences in rural Catalonia are equally revealing about the hierarchical assumptions of village life:

> Initially, the interviewing process went very slowly because I was overly polite and solicitous about seeking interviews with people I hardly knew. I made the error of being too formal, which made these people suspicious of me. My mistake was brought home to me forcefully by one of the few nobles remaining in the Alto Panadés, whom I had interviewed by chance. He explained in no uncertain terms that I was behaving like a servant or client to these individuals when my own wealth, looks and education meant that I was superior to them. He proceeded to accompany me to more than twenty bourgeois landholders, and ordered them to give me what I wanted, on the spot, including details of business scandals, etc. All complied, some with obeisance towards the Count, and all with both deference and expansiveness toward me. The Count checked all their answers to see if they were concealing vital information. Astonished and embarrassed as I was, the Count had a point. After these twenty interviews, I was swamped by volunteers. It had suddenly become fashionable to be interviewed by *el distinguido antropólogo norteamericano*.
>
> (Hansen 1977:163–4)

Gatekeepers, sponsors, and the like (indeed, most of the people who act as hosts to the research) will operate in terms of expectations about the ethnographer's identity and intentions. As the examples of Hansen and Barrett make clear, these can have serious implications for the amount and nature of the data collected. Many hosts have highly inaccurate, and lurid, expectations of the research enterprise, especially of ethnographic work. Two closely related models of the researcher tend to predominate in this context, 'the expert' and 'the critic'. Both images can conspire to make the gatekeeper uneasy as to the likely consequences of the research, and the effects of its conduct.

The model of the 'expert' often seems to suggest that the social researcher is, or should be, a person who is extremely well informed as to 'problems' and their 'solutions'. The expectation may be set up that the ethnographer seeking access is claiming such expertise, and is expecting to 'sort out' the organization or community. This view therefore leads directly to the second image, that of the 'critic'. Gatekeepers may expect the ethnographer to try to act as an evaluator. (Sometimes, of course, the ethnographer may be officially engaged in evaluation: see Fetterman 1984; Fetterman and Pittman 1986. However, even in this situation, it may still be advisable to distance oneself from the roles of both expert and critic.)

Under some circumstances, these expectations may have favourable connotations. Evaluation by experts, leading to improvements in efficiency, interpersonal relations, planning, and so on, may have at least the overt support of those at the top (though not necessarily of those in subordinate positions). On the other hand, the expectation of expert critical surveillance may create anxieties, on the part of gatekeepers and others. Even if permission for the research is not withheld altogether, gatekeepers may, as we have suggested, attempt to guide the research in directions they prefer, or away from potentially sensitive areas.

On the other hand, it may be very difficult for the ethnographer to establish credibility if hosts expect some sort of 'expertise'. Such expectations may clash with the fieldworker's actual or cultivated ignorance and incompetence. Smigel (1958), for example, has commented on the propensity of lawyers to try to 'brush off' researchers who appear to be legally ill-informed, a point confirmed to some extent by Mungham and Thomas (1981). Ethnographers are sometimes conspicuous for an apparent lack of activity as well. This, too, can militate against their being treated seriously by their hosts.

From a variety of contexts researchers report hosts' suspicions and expectations often proving barriers to access. Such suspicions may be fuelled by the very activities of the fieldworker. Barrett (1974), for instance, remarks on how the inhabitants of his Spanish village interpreted his actions. He was not sensitive to the possibility that villagers might be frightened by someone making notes, when they did not know what was being written down. Rumours about him included beliefs that he was a communist spy, a CIA agent, a Protestant missionary, or a government tax agent. Relatedly, in her fieldwork in Brazil in the late 1930s, Landes was accused of seeking 'vigorous' men to do more than carry her luggage. She was labelled a prostitute during her research because she inadvertently broke the local rules about the proper behaviour of a woman (Landes 1986:137). As might be expected, this created problems for her research and for her personal relationships in the field.

At the same time, it is possible to misread the responses of gatekeepers and participants as more negative than they are. In the case of his research on Hasidic Jews, Shaffir comments:

> My suspicion that I was not fully welcomed resulted from a basic misinterpretation: I mistook an indifferent reaction for a negative one. As much as I wished for people to be curious and enthusiastic about my research, the majority could not have cared less. My research did not affect them, and they had more important matters to which to attend.
>
> (Shaffir 1991:76)

Such indifference is not uncommon, nor is a tendency towards paranoia on the part of the ethnographer!

As we noted early on in this chapter, the problem of access is not resolved once one has gained entry to a setting, since this by no means guarantees access to all the data available within it. Not all parts of the setting will be equally open to observation, and not everyone may be willing to talk. Moreover, even the most willing informant will not be prepared, or perhaps even able, to divulge all the information available to him or her. If the data required are to be obtained, negotiation of access is therefore likely to be a recurrent preoccupation for the ethnographer. Negotiation here takes two different but by no means unrelated forms. On the one hand, explicit discussion with those whose activities one wishes to study may take place, much along the lines of that with sponsors and gatekeepers. But the term 'negotiation' also refers to the much more wide-ranging and subtle process of manoeuvring oneself into a position from which the necessary data can be collected. Patience and diplomacy are at a premium here. The ethnographer's negotiation of a role in the setting, and the implications of different roles for the nature of the data collected, will be examined in the next chapter.