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 clash with the fieldworker's actual or cultivated ignorance and incompetence. Smigel (1958), for instance, 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.

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, not everyone may be willing to talk, and 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 to develop and test the theory are to be acquired, 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. 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.

4

Field relations

Like gatekeepers and sponsors, people in the field will also seek to place or locate the ethnographer within their experience. This is necessary, of course, for them to know how to deal with him or her. Some individuals and groups have little or no knowledge of social research. As with Barrett (1974), anthropologists are frequently suspected, initially at least, of being government spies, tax inspectors, police informers, etc. Den Hollander provides an example of an apparently more favourable initial identification that nevertheless proved to be an insurmountable obstacle to his research:

> 'In a town in southern Georgia (1932) it was rumoured after a few days that I was a scout for a rayon concern and might help to get a rayon industry established in the town. My denial reinforced the rumour, everyone tried to convince me of the excellent qualities of the town and its population – the observer had turned into a fairy godmother and serious work was no longer possible. Departure was the only solution.'
>
> (Den Hollander 1967:13)

Even where people in a setting are familiar with research,

there may be a serious mismatch between their expectations of the researcher and his or her intentions. Like gatekeepers, they too may view the researcher as expert or critic. Furthermore they may be, or consider themselves to be, very sophisticated in their knowledge of research methodology without being familiar with ethnography. Where this is the case they may challenge the legitimacy of the research and the credentials of the researcher.

Whether or not people have knowledge of social research, they are often more concerned with what kind of person the researcher is than with the research itself. They will try to gauge how far he or she can be trusted, what he or she might be able to offer as an acquaintance or friend, and perhaps also how easily he or she could be manipulated or exploited. (For a striking analysis of this process see Edgerton 1965.) The management of 'personal front' (Goffman 1955) is important here. As in other situations where identities have to be created or established, much thought must be given to 'impression management'. Impressions of the researcher that pose an obstacle to access must be avoided or countered as far as possible, while those that facilitate it must be encouraged; within the limits set by ethical considerations.

Impression management

Personal appearance can be a salient consideration, as Liebow notes:

> 'Almost from the beginning, I adopted the dress and something of the speech of the people with whom I was in most frequent contact, as best I could without looking silly or feeling uncomfortable. I came close in dress (in warm weather, tee or sport shirt and khakis or other slacks) with almost no effort at all. My vocabulary and diction changed, but not radically. . . . Thus, while remaining conspicuous in speech and perhaps in dress, I had dulled some of the characteristics of my background. I probably made myself more accessible to others, and certainly more acceptable to myself. This last point was forcefully brought home to me one evening when, on my way to a professional meeting, I stopped off at the carry-out in a suit and tie. My loss of ease made me clearly aware that the change in dress, speech, and general carriage was as important for its effect on me as it was for its effect on others.'
>
> (Liebow 1967:255–56)

Liebow here stresses that while his demeanour and dress tended to reduce the social differences between himself and his companions, he did not strive to become exactly like them. Howard Parker, writing about his work with adolescent deviants, makes a very similar point:

> 'Dress regulations were not unduly strict, and a dark pair of cord jeans and a leather jacket were as acceptable as all-blue denim or combinations of leather, cord and denim. I never attempted to copy dress style completely, adapting only to the extent of blunting differences. My own black shirt, black jeans, burgundy leather, style was always acceptable and indeed my leather (acquired locally at a very reasonable price!) became a bit of a joke and it was agreed that I most probably not only slept in it but copulated in it also. "There's a leather going in the Block, Parker lad, 'bout time you went mod isn't it? They'll give you a needle at the Royal hospital to get that old one off" (*Joey*).'
>
> (Parker 1974:216)

Such forms of dress, then, can 'give off' the message that the ethnographer seeks to maintain the position of an acceptable marginal member. They thus declare the essential affinity between researcher and hosts, without any attempt on the part of the former to ape the style of the latter. Such considerations apply particularly under conditions of overt research, where an explicit research role must be constructed. Under conditions of secret research, of course, the fieldworker will be much more sharply constrained to match his or her personal front to that of the other participants.

Patrick's research on a Glasgow gang reveals the difficulty of 'passing' in this way:

> 'Clothes were another major difficulty. I was already aware of the importance attached to them by gang members in the school and so, after discussion with Tim, I bought . . . a midnight-blue suit, with a twelve-inch middle vent, three-inch flaps over the side pockets and a light blue handkerchief with

a white polka dot (to match my tie) in the top pocket. . . . Even here I made two mistakes. Firstly, I bought the suit outright with cash instead of paying it up, thus attracting both attention to myself in the shop and disbelief in the gang when I innocently mentioned the fact. Secondly, during my first night out with the gang, I fastened the middle button of my jacket as I was accustomed to. Tim was quick to spot the mistake. The boys in the gang fastened only the top button; with this arrangement they can stand with their hands in their trouser pockets and their jackets buttoned – ''ra gallous wae''.'

(Patrick 1973:13, 15)

There can be no clear prescription for dress other than to commend a degree of self-consciousness over self-presentation. A mistake over such a simple matter can jeopardize the entire enterprise. Having gained access to the Edinburgh medical school, for instance, Paul Atkinson (1976, 1981a) went to see one of the influential gatekeepers for an 'informal' chat about the actual fieldwork. He was dressed extremely casually (as well as having very long hair). He had absolutely no intention of going onto the hospital wards looking like that. But the gatekeeper was taken aback by his informal appearance, and started to get cold feet about the research altogether. It took a subsequent meeting, after a hair-cut and the donning of a lounge suit, to convince him otherwise.

To some extent we have already touched on more general aspects of self-presentation. Speech and demeanour will require monitoring, though, as we have seen, it is not necessarily desirable for them to be matched to those of participants. The researcher must judge what sort of impression he or she wishes to create, and manage appearances accordingly. Such impression management is unlikely to be a unitary affair, however. There may be different categories of participants, and different social contexts, which demand the construction of different 'selves'. In this, the ethnographer is no different in principle from social actors in general, whose social competence requires such sensitivity to shifting situations.

The construction of a working identity may be facilitated in some circumstances if the ethnographer can exploit relevant

skills or knowledge he or she already possesses. Parker illustrates the use of social skills in the course of his work with a Liverpool gang. He wrote that:

'blending in was facilitated by certain basic skills. One of the most important involved being ''quick'': although I was regarded as normally ''quiet'' and socially marginal, this placidity is not always a good idea. Unless you are to be seen as something of a ''divvy'' you must be able to look after yourself in the verbal quickfire of the Corner and the pub. . . . Being able to kick and head a football reasonably accurately was also an important aspect of fitting into the scheme. Again, whilst I was ''no Kevin Keegan'' and indeed occasionally induced abuse like ''back to Rugby Special'', I was able to blend into a scene where kicking a ball around took up several hours of the week. I also followed The Boys' football team closely each week and went to ''the match'' with them when I could. This helped greatly. Indeed when everyone realized I supported Preston (as well as Liverpool, of course) it was always a good joke since they were so often getting beaten. ''Why don't you play for them they couldn't do any worse?''; ''Is there a blind school in Preston?'' (*Danny*).'

(Parker 1974:217–19)

One sort of expertise, of a rather different sort, that anthropologists often find themselves trading on is that of superior technical knowledge and resources. Medical knowledge and treatment constitutes one form of this. The treatment of common disorders, usually by simple and readily available methods, has long been one way in which anthropologists in the field have succeeded in ingratiating themselves. This can create problems, of course, as McCurdy (1976) found out, with surgery time capable of taking up the whole day. Nevertheless, this is one way in which the fieldworker can demonstrate that he or she is not an exploitative interloper, but has something to give. Legal advice, the writing of letters, and the provision of 'lifts', for example, can perform the same role, though the value of pure sociability should not be underestimated. Indeed, the researcher must often try to find ways in which 'normal' social intercourse can be established. One often has to try to find some neutral ground with participants, where mundane small-talk can

take place. It may be very threatening to hosts if one pumps them *constantly* about matters relating directly to research interests. Especially in the early days of field negotiations it may be advantageous to find more 'ordinary' topics of conversation with a view to establishing one's identity as a 'normal', 'regular', 'decent' person.

Beynon (1983) comments on this aspect of his ethnography in an urban secondary school for boys, in his attempts to establish rapport with the teaching staff:

> 'Although I did not consciously search these out, I stumbled upon topics in which they and I shared a certain degree of interest to serve as a backcloth, a resource to be referred to for "starters", or for "gap fillers" to keep the conversational door ajar.'
>
> (Beynon 1983:40)

Needless to say, such 'neutral' topics are not actually divorced from the researcher's interests at hand, since they can throw additional and unforeseen light on informants, and yield fresh sources of data.

Beynon also lists as a 'way-in' his own local connections:

> 'being regarded as "a local" was an important step forward, especially when it became known that I lived within comfortable walking distance of Victoria Road. This considerably lessened the sense of threat which some felt I posed.'
>
> (Beynon 1983:41)

This would not lessen such 'threats' in all cases, however. In some settings the participants might feel less threatened by a 'stranger', and feel more uneasy about the possible significance of an observer's local knowledge. The same applies to another of Beynon's 'ways in':

> 'More significantly by far, however, was my own background in teaching and experience in secondary schools, which I unashamedly employed to show staff that I was no stranger to teaching, to classrooms, and to school life in general. I was too old to adopt the now-familiar ethnographic persona of "naive student", and found it best to present myself as a former teacher turned lecturer/researcher.'
>
> (Beynon 1983:41)

Beynon goes on to quote the following exchange, which illustrates how such experience was a 'bonus' in his particular circumstances. At the same time, the extract illustrates a reaction to the attentions of a research worker typical of many settings.

> 'MR. BUNSEN: Where did you teach in London?
>
> J.B.: South London and then Hertfordshire.
>
> MR. PIANO: (who had been reading the Staff notice board) Good Lord, I didn't realise you were one of us! I thought you were one of the "experts" who never taught, but knew all about it.
>
> J.B.: I don't know all about it, but I have taught.
>
> MR. PIANO: How long?
>
> J.B.: Ten years, in a Grammar and then a Comprehensive.
>
> MR. PIANO: That's a fair stretch. Well, well, I can start thumping them now!'
>
> (Beynon 1983:42)

We can note in passing the common resentment on the part of some occupational practitioners, and especially teachers, of detached, often invisible 'experts'; though a fieldworker's willingness to stay and learn can often overcome such hostilities, irrespective of prior membership or expertise.

Beynon himself goes on to note that the employment of such strategies in establishing 'mutuality' was more than him pandering for the teachers' approval. Not only did such exchanges facilitate the collection of data, but they were 'data' in their own right. He also notes some feelings of personal disquiet, wondering whether he was unduly exploitative in offering 'friendship' in return for data.

The problem that the ethnographer often faces in such circumstances is deciding how much self-disclosure is appropriate or fruitful. It is hard to expect 'honesty' and 'frankness' on the part of participants and informants, whilst never being frank and honest about oneself. Nevertheless, just as in many everyday situations, one often has to suppress or play down one's own personal beliefs, commitments, and political sympathies. Again, this is not necessarily a matter of gross deception. The normal requirements of tact, courtesy, and 'interaction ritual' in general (Goffman 1972), mean that in some ways 'everyone has to lie' (Sacks 1975). For the researcher this may be particularly a

matter of self-conscious impression management, and may thus become an ever-present aspect of social interaction in the field. One cannot bias the fieldwork by talking only with people one finds most congenial or politically sympathetic: one cannot choose one's informants on the same basis as one chooses friends (for the most part) (Hammersley 1983c).

The fieldworker may find him- or herself being 'tested' and pushed towards disclosure, particularly when the group or culture in question is founded upon beliefs and commitments (such as religious convictions, political affiliations, and the like). Here the process of negotiating access and rapport may be a matter of progressive initiation. The fieldworker may find the management of disclosure a particularly crucial feature of this delicate procedure. The same may apply with particular force to the investigation of deviance, where deviants may require reassurance that the ethnographer does not harbour feelings of disapproval, nor intends to initiate action against them.

There are, of course, aspects of personal front that are not open to 'management' and that may limit the negotiation of identities in the field, and these include so-called 'ascribed' characteristics. Although it would be wrong to think of the effects of these as absolutely determinate or fixed, such characteristics as gender, age, and ethnic identification may shape relationships with gatekeepers, sponsors, and people under study in important ways.

The researcher cannot escape the implications of gender: no position of genderless neutrality can be achieved. This is a feature of social research that has recently come under close scrutiny, in part as a consequence of the Women's Movement (see for example Roberts 1981). In the context of field research, Golde's (1970) collection of papers by women anthropologists highlights a number of recurrent themes that relate specifically to gender, some of which have been further amplified by Warren and Rasmussen (1977). (Revealingly, the issue of gender as such has only been raised in relation to female fieldworkers; the implications of gender have always been there, but have rarely been rendered visible and available for reflection.)

Common cultural stereotypes of females can work to their advantage in some respects. In so far as women are seen as unthreatening, then they may gain access to settings and information with relative ease. By the same token, however, their gender may limit women's access to particular domains – the domestic world of fellow women, children, the elderly, and so on. Male researchers may find it equally difficult to gain access to the world of women, especially in cultures where there is a strong division between the sexes.

Easterday *et al.* (1977) also remark on some of the ways in which female researchers may enter into field relationships, comparing different settings that are marked by varieties of sex roles. (Their paper is, incidentally, a useful example of how the systematic comparison of researchers' own experiences can potentially lead to a more general understanding of the research process, rather than the one-off autobiographical account.) In male-dominated settings, for instance, women may come up against the male 'fraternity', from which they are excluded; women may also find themselves the object of 'hustling' from male hosts; they may be cast in the role of the 'go-fer' runner of errands, or may be adopted as a sort of mascot. These possibilities all imply a lack of participation, or non-serious participation on the part of the woman. Not only will the female researcher find it difficult to be taken seriously by male hosts, but other females may also display suspicion and hostility in the face of her intrusion. Easterday *et al.* (1977) also recognize that for such reasons, female researchers may find advantageous trade-offs. The 'hustling' informant who is trying to impress the researcher may prove particularly forthcoming to her, and males may be manipulated by femininity. In some circumstances it may be easier for females to present themselves as socially acceptable incompetents, in many ways the most favourable role for a participant observer to adopt in the early stages of fieldwork.

There can be little doubt that much of the particular character of ethnographic writing has been coloured by the (male) gender of the great majority of writers. In her commentary on urban studies, Lyn Lofland has drawn attention to the 'thereness' of women; that is, like domestic servants, they are present, but as part of the background and are rarely taken notice of:

> 'There is really nothing in urban sociology on women quite comparable to the finely textured, closely grained, empiri-

> cally loving, portrayal of "the boys' world" in Suttles' *The Social Order of the Slum* (1968), or of "corner boys and college boys" in Whyte's *Street Corner Society* (1955), or of "cats" in Finestone's *Cats, Kicks and Colour* (1967), or of "negro streetcorner men" in Liebow's *Tally's Corner* (1967), or of "urban nomads" in Spradley's *You Owe Yourself a Drunk* (1970).'
>
> (Lofland 1975:145)

Lofland is too astute a sociologist to attribute such emphases to gender bias alone; she also implicates particular analytic preoccupations and assumptions concerning the formulation of 'social problems'. Her essay is a valuable discussion of an important sociological blind spot.

Ethnicity, like gender, sets its limits and poses its problems. Ethnicity is, of course, not merely a matter of physical characteristics, but also implies matters of culture, power, and personal style. Keiser (1970), reflecting on his work with the 'Vice Lords', a Chicago street gang, notes that it was difficult for him, as a white man, to establish relationships with black informants. While some were willing to accept him as a 'white nigger', others displayed strong antagonisms.

On the other hand, belonging to a different ethnic or even national group can sometimes have distinct advantages. Hannerz (1969), discussing his research on a black ghetto area in the United States, points out that while one of his informants jokingly suggested that he might be the real 'blue-eyed blond devil' that the Black Muslims talked about, his Swedish nationality distanced him from other whites.

Papanek (1964) draws attention to the two aspects of the fieldworker's identity we have referred to above. Reflecting on her experience in studying purdah, she points out that as a woman she had access to the world of women, which no man could ever attain, while her own foreignness helped to remove her from the most restricting demands of female modesty. As an outsider, the woman fieldworker in such a culture may be able to establish relatively 'neutral' roles *vis-à-vis* men in a way that no female *member* of the society could. (This view is endorsed by Jeffery (1979) on the basis of her own work on women in purdah.) The 'foreign' woman may almost become an 'honorary man' (Warren and Rasmussen 1977). This latter is particularly the case if the researcher is no longer young.

Age is another important aspect of the fieldworker's persona. Although it is by no means universally true, there appears to be a tendency for ethnography to be the province of younger research workers. In part this may be because the younger person has more time to commit to the fieldwork (often for a higher degree); in part it may suggest that junior people find it easier to adopt the 'incompetent' position of the 'outsider' or 'marginal' person. This is not to imply that ethnography is properly restricted to younger investigators, but one must at least entertain the possibility that age will have a bearing on the kinds of relationships established and the data collected. The junior research student may well establish quite different working relationships from those available to, say, the middle-aged professor.

Honigmann (1970) illustrates the effects that age can have in comparing his research on the Kaska Indians at the age of thirty with his work on the Eskimos of Baffin Island which he carried out when he was forty-nine:

> 'Many experiences, both social and professional, brought special rewards, and consequently our stay in Frobisher Bay was not unpleasant, but there was not the kind of pleasurable excitement that in Western Canada had come from identifying with the community and participating intensely in the libidinous tenor of its behaviour. In Frobisher Bay I danced with my wife, except for old-fashioned group dances, joined no illicit drinking parties, and in the tavern drank carefully and watched the clock in order to have time to catch the last bus back to Apex [the suburb where he lived]. Perhaps by the time I reached Frobisher Bay I had lost some of the adaptability I possessed in my youth, and perhaps this limited the behaviour I could adopt in participant observation. Changes that time had brought in my professional status may also have been a factor, for my age seems to have alienated me from the younger men and women, especially those deviant from the larger society's norms.'
>
> (Honigmann 1970:61–2)

However, the effects of age, as of all personal characteristics, must not be overestimated. It is often possible to overcome

them as Corsaro (1981) found in his research on nursery schoolchildren:

'Two four-year-old girls (Betty and Jenny) and adult researcher (Bill) in a nursery school:

BETTY: You can't play with us!
BILL: Why?
BETTY: Cause you're too big.
BILL: I'll sit down. (sits down)
JENNY: You're still too big.
BETTY: Yeah, you're "Big Bill"!
BILL: Can I just watch?
JENNY: OK, but don't touch nuthin!
BETTY: You just watch, OK?
BILL: OK.
JENNY: OK, Big Bill?
BILL: OK.

(Later Big Bill got to play.)'

(Corsaro 1981:117)

In the course of fieldwork, then, people who meet, or hear about, the researcher will cast him or her into certain identities on the basis of 'ascribed characteristics', as well as aspects of appearance and manner. This 'identity work' (Goffman 1959) must be monitored for its effects on the kinds of data collected. At the same time, the ethnographer will generally try to shape the nature of his or her role, as it is emerging in the setting, in such a way as to try to ensure that access to the necessary data is achieved.

Field roles

In the early days of fieldwork, the conduct of the ethnographer is often little different from the sort of activity that any layperson engages in when faced with the practical need to make sense of a particular social setting. Consider the position of the novice or recruit – a freshman student, a military rookie, a person starting a new job, say – who finds him- or herself in relatively strange surroundings. How do such novices get to 'know the ropes' and

become 'old hands'? Obviously, there is nothing magical about this process of learning. Novices watch what other people are doing, ask other people to explain what is going on, try things out for themselves – occasionally making mistakes – and so on. The novice thus acts like a social scientist: making observations and inferences, asking informants, constructing hypotheses, and acting on them.

When studying an unfamiliar setting, the ethnographer is also a novice. Wherever possible he or she must put him- or herself into the position of being an 'acceptable incompetent', as Lofland (1971) neatly describes it. It is only through watching, listening, asking questions, formulating hypotheses, and making blunders that the ethnographer can acquire some sense of the social structure of the setting and begin to understand the culture of participants.

The crucial difference between the 'lay' novice and the ethnographer in the field is that the latter attempts to maintain a self-conscious awareness of what is learned, how it has been learned, and the social transactions that inform the production of such knowledge. As we saw in Chapter 1, it is an important requirement of ethnography that we suspend a wide range of common-sense and theoretical knowledge in order to minimize the danger of taking on trust misleading preconceptions about the setting and the people in it. 'Strange' or 'exotic' settings quickly demolish the ethnographer's faith in his or her preconceptions just as Schutz's (1964) stranger finds that what he or she knows about the new country will not suffice for survival in it.

Laura Bohannon (under the *nom de plume* Elenore Bowen) has written a vivid, semi-fictionalized account of her own initial encounters with an African culture. She captures the sense of alienation and 'strangeness' experienced by the fieldworker, and a feeling of being an 'incompetent':

'I felt much more like a backward child than an independent young woman. My household supported me, right or wrong against outsiders, but made their opinions known after the fact, and so obviously for my own good that I could not be justifiably angry. I felt even less like a trained and professional anthropologist pursuing his researches. I was hauled around from one homestead to another and scolded for my lack of

> manners or for getting my shoes wet. Far from having docile informants whom I could train, I found myself the spare-time amusement of people who taught me what they considered it good for me to know and what they were interested in at the moment, almost always plants or people.'
>
> (Bowen 1954:40–1)

She documents the personal and emotional difficulties of coming to terms with such estrangement, but it is apparent from her account that this is integral to the process of learning. For instance, in the following account of a form of greeting that is exchanged when people meet on a path:

> 'The situation allows of fine calculation. To play fair, both should walk on at an even rate: a sudden sprint of speed may disconcert the other party, but such stratagems are resorted to only by the inexperienced or by those greedy for social victory at any price. A nervous eye and a half-open mouth indicate arrival within the six yards leeway zone of possible greeting. Whoever then first says "Where are you going?" should be answered in full; the loser, that is, must fill with explanation the time it takes to pass one another and walk on to just that point at which the winner can hear, "And where are you going?" as he walks out of earshot and out of the obligation to reply.
>
> At first, everyone beat me to the draw. By the time I had skill enough to have a fifty-fifty chance, I had discovered refinements of the game. One far beyond my ability at that point and not altogether within the rules anyhow, was to begin a really interesting story of where one was going and why, timed to make the point of the story coincide with walking out of hearing. The other, also my own invention, was made in ignorance on that market day's walk. The reply to the path greeting is a simple phrase which means "I'm going travelling" only when one adds one's destination; otherwise the same phrase means "I'm walking" – not running or riding. At first I was unable to explain where I was going; later, I didn't always want to. Then, the greetings went like this:
>
> "Where are you going?"
> "I'm *walking*."

> "So I see. *Where* are you going?"
> "I'm *walking*. Where are *you* going?"
>
> Half the time the victim of this unorthodox approach was flustered enough to answer and thus forfeit the information he had won by asking first.'
>
> (Bowen 1954:35–6)

Bowen's comments here show how she came to understand this particular fragment of local 'interaction ritual' (Goffman 1972) through a process of trial-and-error, and to progress from incompetence to a (somewhat idiosyncratic) expertise in manipulating the 'rules of the game'.

This process of estrangement is what is often referred to as 'culture shock' and it is the stock-in-trade of social and cultural anthropology. That confrontation of the ethnographer and the 'alien' culture is the methodological and epistemological foundation of the anthropological enterprise, whether it be from the point of view of a romantically-inspired search for exotic cultures, or the less glamorous sort of encounter such as described by Chagnon, from his fieldwork among the Yanomamö.

Chagnon (1977) describes, with engaging frankness, how he set off into 'the field' with a mixture of assumptions. On the one hand, he confesses to a Rousseau-like expectation as to his future relations with the Yanomamö: that they would like him, even adopt him, and so on. At the same time, by virtue of his seven years of training as an anthropologist, he carried with him a considerable load of social-scientific assumptions: as he puts it, that he was about to encounter 'social facts' inhabiting the village, all eager to recount their genealogies to him. In contrast to his romantic phantasies, and his social-scientific assumptions, he did not encounter a collection of social facts, nor indeed were his chosen people the noble or welcoming savages of his imagination. Quite the reverse:

> 'I looked up and gasped when I saw a dozen burly, naked, filthy, hideous men staring at us down the shafts of their drawn arrows! Immense wads of green tobacco were stuck between their lower teeth and lips making them look even more hideous, and strands of dark green slime dripped or hung from their noses. . . . I was horrified. What sort of welcome was this for the person who came here to live with you and learn your way of life, to become friends with you?'
>
> (Chagnon 1977:4)

It is worth noting in passing here that Chagnon's self-revelation shows not only the 'culture clash' of the Westerner encountering an 'exotic' culture, but also the problem of the social scientist who expects to uncover 'social facts', 'rules', 'institutions', 'organizations', and so on by direct observation of the social world. This is perhaps one of the hardest lessons to learn at the outset. One does not 'see' everyday life laid out like a sociology or anthropology textbook, and one cannot read off analytic concepts directly from the phenomena of everyday life. Some researchers, setting out on fieldwork, may even feel a sense of betrayal when they discover this, or alternatively experience a panic of self-doubt, believing themselves to be inadequate research workers because their observations do not fall neatly into the sorts of categories suggested by the received wisdom of 'the literature'.

In researching settings that are more familiar, it is, of course, much more difficult to suspend one's preconceptions, whether these derive from social science or from everyday knowledge. One reason for this is that what one finds is so obvious. Becker provides a classic example:

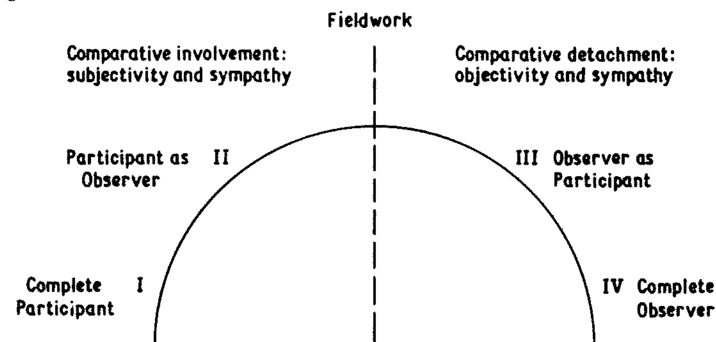
> 'We may have understated a little the difficulty of observing contemporary classrooms. It is not just the survey method of educational testing or any of those things that keeps people from seeing what is going on. I think, instead, that it is first and foremost a matter of it all being so familiar that it becomes impossible to single out events that occur in the classroom as things that have occurred, even when they happen right in front of you. I have not had the experience of observing in elementary and high school classrooms myself, but I have in college classrooms and it takes a tremendous effort of will and imagination to stop seeing only the things that are conventionally ''there'' to be seen. I have talked to a couple of teams of research people who have sat around in classrooms trying to observe and it is like pulling teeth to get them to see or write anything beyond what ''everyone'' knows.'
>
> (Becker 1971:10)

Another problem with settings in one's own society is that one may not be allowed to take on a novice role. We noted in the previous chapter how researchers are sometimes cast into the role

of expert or critic. Moreover, ascribed characteristics, notably age, and latent identities – as in the case of Beynon's (1983) research on teachers – may reinforce this. In studying such settings the ethnographer is faced with the difficult task of rapidly acquiring the ability to act competently, which is not always easy even within familiar settings, while simultaneously privately struggling to suspend for analytic purposes precisely those assumptions that must be taken for granted in relations with participants.

The 'acceptable incompetent' is not, then, the only role that ethnographers may take on in the field, and, indeed, even where it is adopted it is often abandoned, to one degree or another, as the fieldwork progresses. There have been several attempts to map out the various roles that ethnographers may adopt in settings. Junker (1960) and Gold (1958), for example, distinguish between the 'complete participant', 'participant-as-observer', 'observer-as-participant', and 'complete observer' (see *Figure 1*).

Figure 1 Theoretical social roles for fieldwork



Source: Junker 1960:36. (Reproduced by permission of Chicago University Press.)

In the 'complete participant' role, the ethnographer's activities are wholly concealed. Here the researcher may join an organization or group – Alcoholics Anonymous (Lofland and Lejeune 1960), Pentecostalists (Homan 1980), an army unit (Sullivan *et al.* 1958), a mental hospital (Rosenhahn 1982) – as though they were ordinary members but with the purpose of carrying out research. Alternatively, complete participation

may occur where the putative researcher is already a member of the group or organization that he or she decides to study. This was the case with Holdaway's (1982) research on the police, and Dalton's (1959) work on 'Men Who Manage'. An extreme example is Bettelheim's (1970) account of life in German concentration camps.

'Complete participation' is, then, approximated in some circumstances. Some commentators have suggested that it is the ideal to which researchers should aim. Jules-Rosette (1978), for instance, has argued for the necessity of 'total immersion' in a native culture. That is, not simply 'passing' as a member but actually *becoming* a member. In Jules-Rosette's case this was accompanied by conversion to the Apostolic Church of John Maranke, an indigenous African movement. This indeed is the criterion Jules-Rosette demands for what she calls 'reflexive ethnography': a usage of the term 'reflexive' that is very different from our own (for yet a third view, see Sharrock and Anderson 1980).

To the inexperienced, 'complete participation' might seem very attractive. Such identification and immersion in the setting may appear to offer safety: one may travel incognito, obtain 'inside' knowledge, and avoid the trouble of access negotiations. There is some truth in this, and indeed in some settings complete participation may be the only strategy by which the data required can be obtained. However, 'passing' as a member over a protracted period may place great strain on the fieldworker's dramaturgical capacities; and should the ethnographer's cover be 'blown', then the consequences could be disastrous, both for the researcher personally and for the completion of the fieldwork project.

More fundamentally still, the strategy of 'complete participation' will normally prove extremely limiting. The range and character of the data that can be collected will often prove restricted in practice. The participant will, by definition, be implicated in existing social practices and expectations in a far more rigid manner than the known researcher. The research activity will therefore be hedged round by these pre-existing social routines and realities. It will prove hard for the fieldworker to arrange his or her actions in order to optimize data collection possibilities. Some potentially fruitful lines of inquiry

may be rendered practically impossible, in so far as the complete participant has to act in accordance with existing role expectations.

This is a point well made by Pollert (1981) in relation to her study of female factory workers. She discusses the possible strategy of obtaining a factory job herself. In the first place, the management of her chosen industrial setting would not permit it, but:

> 'Second, had I got a job, the advantages of experiencing for myself what it felt like, and possibly becoming very close to a small work-group around me, would have been heavily outweighed by the disadvantages of restricted movement, abiding by the rules preventing entry into other departments (without permission), and losing the privileges of the outsider, of speaking to other employees in the factory, including chargehands, supervisors and managers.'
>
> (Pollert 1981:6)

The limitations of complete participation are also indicated by Gregor (1977). During the early days of fieldwork in a Brazilian Indian village, Gregor and his wife attempted – in the interests of 'good public relations' – to live out their lives as villagers:

> 'Unfortunately we were not learning very much. Each day I would come back from treks through the forest numb with fatigue, ill with hunger, and covered with ticks and biting insects. My own work was difficult to pursue, for fishing and hunting are serious business and there is no time to pester men at work with irrelevant questions about their mother's brothers. Meanwhile, my wife was faring little better with the women.'
>
> (Gregor 1977:28)

Hence Gregor and his wife stopped 'pretending' that they were 'becoming' Brazilian villagers, and turned to systematic *research* activity, collecting data for census material, genealogies, residence patterns, hammock arrangements, and so on.

In contrast to the 'complete participant', the 'complete observer' has no contact at all with those he or she is observing. Observation may take place through a one-way mirror. Covert

observation from a window of public behaviour in the street (Lofland 1973) also falls into this category, and perhaps also research like that by Karp (1980) on the 'public sexual scene' in Times Square.

Paradoxically, complete observation shares many of the advantages and disadvantages of complete participation. In their favour they can both minimize problems of reactivity: in neither case will the ethnographer interact *as a researcher* with members being studied. On the other hand, there may be severe limits on what can and cannot be observed and the questioning of participants may be impossible. Adopting either of these roles alone would make it very difficult to generate and test theory in a rigorous manner, though both may be useful strategies to adopt during particular phases of the fieldwork, and in some situations may be unavoidable.

Most field research involves roles somewhere between these two poles. Whether the distinction between participant-as-observer and observer-as-participant is of any value is a moot point. Indeed, in examining this distinction a serious problem with Junker's (1960) typology arises: it runs together several dimensions of variation that are by no means necessarily related. One of these, touched on in the previous chapter, is the question of secrecy and deception. Another is the issue of whether the ethnographer takes on a role already existing in the field or negotiates a new role; though no hard and fast distinction can be made here, and indeed we should beware of treating the roles already established in the setting as rigid and fixed in character (Turner 1962).

Nevertheless, there is an important point at issue. In secret research one has little option but to take on an existing role, though it may be possible to extend and modify it somewhat to facilitate the research (Dalton 1959). Sometimes even in open research there may be no choice but to take on an established role, as Freilich (1970) found out in his research on Mohawk steelworkers in New York. Having become adopted as a friend of one of the Mohawks, he tried to revert to the role of anthropologist. As he remarks:

> 'It was soon clear that any anthropological symbol was tabu . . . I could use no pencils, notebooks or questionnaires. I even failed in attempts to play the semianthropologist. For example I tried saying, "Now that is really interesting; let me write that down so that I don't forget it". Suddenly my audience became hostile, and the few words I jotted down cost me much in rapport for the next few days.'
>
> (Freilich 1970:193)

Generally, though, in open research the ethnographer has some choice over whether or not to take on one of the existing roles in the field. Thus, for example, in research on schools, ethnographers have sometimes adopted the role of teacher (see for example Hargreaves 1967 and Lacey 1976), sometimes not; though they have rarely taken on the role of pupil (but see Corsaro 1981 and Llewellyn 1980).

Decisions about the role to adopt in a setting will depend on the purposes of the research and the nature of the setting. In any case, anticipation of the likely consequences of adopting different roles can rarely be more than speculative. Fortunately, shifts in role can often be made over the course of fieldwork. Indeed, there are strong arguments in favour of moving among roles so as to allow one to discount their effects on the data. In studying nursery-school children, Corsaro (1981) not only sought to become a participant in the children's games, but also during a later phase of the research he used a one-way mirror to observe their behaviour. Similarly, Sevigny (1981), studying art classes in a college, collected data by surreptitiously taking on the role of student, by acting as tutor, as well as adopting a variety of researcher roles.

Different roles within a setting can be exploited, then, in order to get access to different kinds of data, as well as to acquire some sense of the various kinds of bias characteristic of each.

Managing marginality

There is a third dimension of variation in research roles built into the typology developed by Junker and Gold: from the 'external' view of the observer to the 'internal' view of the participant. The 'complete participant' gets access to inside information and experiences the world in ways that may be quite close to the ways other participants experience it. In this way greater access

to participant perspectives may be achieved. At the same time, there is the danger of 'going native'. Not only may the task of analysis be abandoned in favour of the joys of participation, but even where it is retained bias may arise from 'over-rapport'. Miller outlines the problem in the context of a study of local union leadership:

> 'once I had developed a close relationship to the union leaders I was committed to continuing it, and some penetrating lines of inquiry had to be dropped. They had given me very significant and delicate information about the internal operation of the local [union branch]: to question closely their basic attitudes would open up severe conflict areas. To continue close rapport and to pursue avenues of investigation which appeared antagonistic to the union leaders was impossible. To shift to a lower level of rapport would be difficult because such a change would induce considerable distance and distrust.'
>
> (Miller 1952:98)

Having established friendly relations Miller found the possibilities of data collection limited. Indeed, he suggests that the leaders themselves might have fostered such close relationships as a strategy to limit his observations and criticisms. Miller also notes that over-rapport with one group leads to problems of rapport with others: in his study, his close rapport with union leaders limited his rapport with rank and file members.

The question of rapport applies in two senses, both of which may be glossed as issues of 'identification'. In the sort of case outlined by Miller, one may be identified with particular groups or individuals so that one's social mobility in the field, and relationships with others, become impaired. More subtly, perhaps, is the danger of 'identifying with' such members' perspectives, and hence of failing to treat these as problematic.

One recent British ethnography that appears to many readers to be flawed by such 'partial perspectives' is Paul Willis's (1977) study of working-class adolescent boys. Willis's work is based primarily on conversations with twelve pupils who display 'anti-school' attitudes. These particular working-class boys describe themselves as 'lads' and distinguish themselves from those they call the 'ear'oles', who subscribe to the values of the

school. The 'lads' see little chance of obtaining 'middle-class' jobs, and enthusiastically seek working-class employment. Willis argues that the counter-culture 'fits' with the culture of the workplace for manual workers, even suggesting that the more conformist pupils are less well adapted to the culture of working-class jobs.

There are two senses in which 'over-rapport' seems to be indicated in Willis's treatment of these youngsters. In the first place he seems to have devoted his attention almost entirely to the 'lads'; in many respects to have taken over their views without question in the analysis. Hence, the book becomes as much a celebration of the 'lads' as anything else: Willis seems unable or unwilling adequately to distance himself from the 'lads'' accounts. Second, the 'lads' are endorsed by Willis since he treats them more or less as spokesmen for 'the working class'. While Willis explicitly recognizes that working-class culture is variable, he nonetheless seems to identify the 'lads'' views, or some of them, as representative of the working class in general. Since the 'ear'oles' or conformists are also from working-class backgrounds, this is problematic, to say the least. It seems clear that Willis is guilty of 'identifying' with his chosen twelve, and his work is deeply flawed as a result.

In a striking parallel, Stein (1964) provides a reflexive account of his own identification with one set of workers, the miners in the gypsum plant he studied with Gouldner (1954):

> 'Looking back now I can see all kinds of influences that must have been involved. I was working out authority issues, and clearly I chose the open expression of hostile feelings that was characteristic in the mine rather than the repression that was characteristic on the surface. I came from a muddled class background which involved a mixture of lower-, upper-, and middle-class elements that I have not yet been able to disentangle fully. The main point is that I associate working-class settings with emotional spontaneity and middle-class settings with emotional restraint. I never quite confronted the fact that the surface men were as much members of the working class as were the miners. . . .
>
> The descriptive writing became an act of fealty since I felt that writing about life in this setting was my way of being

loyal to the people living in it. This writing came more easily than most of my other writing. But the efforts at interpreting the miners' behavior as a product of social forces, and especially seeing it as being in any way strategic rather than spontaneous, left me with profound misgivings.'

(Stein 1964:20–1)

The 'complete observer' generally escapes the danger of 'going native' of course, but only at the risk of failing to understand the perspective of participants. Moreover, this is not simply a matter of missing out on an important aspect of the setting: it may well lead to serious misunderstanding of the behaviour observed.

While ethnographers may adopt a variety of roles, the aim throughout is to maintain a more or less marginal position. As Lofland (1971:97) points out, the researcher generates 'creative insight' out of this marginal position of simultaneous insider-outsider. The ethnographer must be intellectually poised between 'familiarity' and 'strangeness', while socially he or she is poised between 'stranger' and 'friend' (Powdermaker 1966; Everhart 1977). He or she is, in the title of the collection edited by Freilich (1970), a 'marginal native'.

Marginality is not an easy position to maintain, it engenders a continual sense of insecurity. Johnson (1976), for instance, has recorded in some detail his emotional and physical reactions to the stresses of fieldwork. Some of his fieldnotes document his response with remarkable frankness:

> 'Every morning around seven forty-five, as I'm driving to the office, I begin to get this pain in the left side of my back, and the damn thing stays there usually until around eleven, when I've made my daily plans for accompanying one of the workers. Since nearly all of the workers remain in the office until around eleven or twelve, and since there's only one extra chair in the two units, and no extra desks as yet, those first two or three hours are sheer agony for me every damn day. Trying to be busy without hassling any one worker too much is like playing Chinese checkers, hopping to and fro, from here to there, with no place to hide.'

(Johnson 1976:152–53)

The physical symptoms that Johnson describes are perhaps rather extreme examples of fieldwork stress. But the phenomenon in general is by no means unusual: many fieldworkers report that they experience some degree of discomfiture by virtue of their 'odd', 'strange', or 'marginal' position. Some flavour of this can be gleaned from Wintrob's (1969) psychological appraisal of the anxieties suffered by anthropologists in the field: it is based on the experiences of a number of graduate students, and published autobiographical accounts.

Wintrob identifies a number of sources of stress, including what he glosses as the 'dysadaptation syndrome', which includes a wide range of feelings – incompetence, fear, anger, frustration. He cites one graduate student's account:

> 'I was afraid of everything at the beginning. It was just fear, of imposing on people, of trying to maintain a completely different role than anyone else around you. You hem and haw before making a leap into the situation. You want to retreat for another day. I'd keep thinking: am I going to be rejected? Am I really getting the data I need? I knew I had to set up my tent but I'd put it off. I'd put off getting started in telling people about wanting to give a questionnaire. I was neatly ensconced in . . .'s compound (an area of tents comprising one kin group). Everybody there knew what I was doing. I found it hard to move over to the other camp (a few miles away). I rationalised that a field worker shouldn't jump around too much.'

(Wintrob 1969:67)

Malinowski's own diaries reveal many such indications of stress and anxiety: indeed they are a remarkable document for what they reveal about his ambivalent feelings towards the Trobriand Islanders, his own intense self-absorption, and his preoccupation with his own well-being (Malinowski 1967). In a similar vein, Wax (1971) has provided an excellent account of her difficulties in working in a relocation centre for Japanese Americans after the Second World War. Wax describes her initial difficulties with collecting data, in the face of (understandable) suspicion and hostility: 'At the conclusion of the first month of work I had obtained very little data, and I was discouraged,

bewildered and obsessed by a sense of failure' (1971:70).

We do not wish to convey the impression that the experience of fieldwork is one of unrelieved misery: for many it is often a matter of intense personal reward and satisfaction. Yet the stress experienced by the 'marginal native' is a very common aspect of ethnography, and it is an important one. In so far as he or she resists over-identification or surrender to 'hosts', then it is likely that there will be a corresponding sense of 'betrayal', or at least of divided loyalties. Lofland (1971:108–09) draws attention to the 'poignancy' of this experience. There is a sense of schizophrenia that the disengaged/engaged ethnographer may suffer. But this feeling, or equivalent feelings, should be managed for what they are. They are not necessarily something to be avoided, or to be replaced by more congenial sensations of comfort. The comfortable sense of being 'at home' is a danger signal. From the perspective of the 'marginal' reflexive ethnographer, there can thus be no question of total commitment, 'surrender', or 'becoming'. There must always remain some part held back, some social and intellectual 'distance'. For it is in the 'space' created by this distance that the analytic work of the ethnographer gets done. Without that distance, without such analytic space, the ethnography can be little more than the autobiographical account of a personal conversion. This would be an interesting and valuable document, but not an ethnographic study.

Ethnographers, then, must strenuously avoid feeling 'at home'. If and when all sense of being a 'stranger' is lost, one may have allowed the escape of one's critical, analytic perspective. The early days of fieldwork are proverbially problematic, and may well be fraught with difficulties: difficult decisions concerning fieldwork strategy have to be made, working relationships may have to be established quickly, and social embarrassment is a real possibility. On the other hand, it would be dangerous to assume that this is just a difficult phase that the researcher can simply outgrow, after which he or she can settle down to a totally comfortable, trouble-free existence. While social relations and working arrangements will get sorted out, and gross problems of strangeness will be resolved, it is important that this should not result in too cosy a mental attitude.

Everhart (1977) illustrates the danger from his research on college students and teachers:

> 'saturation, fieldwork fatigue, and just plain fitting in too well culminated, toward the end of the second year, in a diminishing of my critical perspective. I began to notice that events were escaping me, the significance of which I did not realize until later. For example, previously I had recorded in minute detail the discussions teachers had on categorizing students and those conversations students had on labeling other students. While these discussions continued and were especially rich because of the factors that caused these perspectives to shift, I found myself, toward the end of the study, tuning out of such discussions because I felt I had heard them all before when, actually, many dealt with dimensions I had never considered. On the one hand I was angry at myself for not recording and analyzing the category systems, on the other hand I was tired and found it more natural to sit with teachers and engage in small talk. The inquisitiveness had been drained from me.'
>
> (Everhart 1977:13)

This is not to deny that there will be occasions, many occasions, when one will need to engage in social interaction for primarily social and pragmatic reasons, rather than in accordance with the research interests and strategies. Rather, the point is that one should never surrender oneself entirely to the setting or to the moment. In principle, one should be constantly on the alert, with more than half an eye on the research possibilities that can be seen or engineered from any and every social situation.

If one does start to feel at ease, and the research setting takes on the appearance of routine familiarity, then one needs to ask oneself some pertinent questions. Is this sense of ease a reflection of the fact that the research is actually finished? Have all the necessary data already been collected? (Obviously in theory there is always something new to discover, unforeseen events to investigate, unpredictable outcomes to follow up, and so on; but the line has to be drawn somewhere.) This is always a useful question to ask: there is no point in hanging on in the field to no good purpose, just for the sake of being there, just

'for interest', or from a lack of confidence that one has enough information.

Sometimes you will tell yourself that you are done: that you should either finish the fieldwork, or that you should now move on to a new social setting. Alternatively, it may be the case that a sense of familiarity has been engendered by sheer laziness. Further questions may be in order, if the research does not seem to be finished. Do I feel at ease because I am being too compliant? That is, am I being so 'nice' to my hosts that I *never* get them to confront any potentially troublesome or touchy topics? Likewise, does my social ease mean that I am avoiding some people, and cultivating others with whom I feel more comfortable? In many social contexts, we find ourselves in need of formal or informal sponsors, helpful informants, and so forth. But it is important not to cling to them. From time to time one should evaluate whether the research is being unduly limited by such a possibility. In general, it is well worth pausing to consider whether a sense of comfort and familiarity may be an artefact of laziness, and a limitation imposed on the research by a failure to go on asking new questions, by a reluctance ever to go against the grain, a fear of ever making mistakes, and an unwillingness to try to establish new or difficult social relationships. It is possible to carve out an inhabitable niche in the field during the early stages of a project: it is important not to stay there, and never try one's wings in other contexts.

In Chapter 1 we argued that the role of the researcher in generating the data collected must be recognized. Rather than seeking, by one means or another, to eliminate reactivity, its effects should be monitored and, as far as possible, brought under control. By systematically modifying one's role in the field, different kinds of data can be collected whose comparison may greatly enhance interpretation of the social processes under study. Relevant here is the use of interviewing and the analysis of documents, and these are the subject of the next two chapters.

5

Insider accounts: listening and asking questions

It is a distinctive feature of social research that the 'objects' it studies are in fact 'subjects', and themselves produce accounts of their world. As we saw in Chapter 1 this fact is interpreted rather differently by positivism and naturalism. For the former these common-sense accounts are subjective and must be replaced by science; at most they are simply social products to be explained. For naturalism, by contrast, common-sense knowledge constitutes the social world: it must be appreciated and described, not subjected to critical scrutiny as to its validity, nor explained away.

We argued in Chapter 1 that these paradigms share the mistaken assumption that only false beliefs can be explained sociologically, though the conclusions they draw from this are diametrically opposed. Once we reject this assumption, it becomes clear that there are two equally important ways in which accounts, both those of the researcher and of the people under study, can be interpreted. On the one hand they can be read for what they tell us about the phenomena to which they