

Field relations

Ethnographic research can take place, and has taken place, in a wide variety of types of setting: villages, towns, inner-city neighbourhoods, factory shop floors, deep-shaft mines, farms, retail stores, business offices of various kinds, hospital wards, operating theatres, prisons, public bars, churches, schools, colleges, universities, welfare agencies, courts, morgues, funeral parlours, etc. These settings vary from one another in all manner of respects that are relevant to the nature of the relationships that are possible and desirable with the people who live and/or work in them. Furthermore, there is much variation within each type of setting. Generalizations about field relations are therefore always subject to multiple exceptions. No set of rules can be devised which will produce good field relations. All that can be offered is discussion of some of the main methodological and practical considerations surrounding ethnographers' relations in the field.

INITIAL RESPONSES

Like gatekeepers and sponsors, people in the field will 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; and, partly as a result, field researchers are frequently suspected, initially at least, of being spies, tax inspectors, missionaries, etc., as we noted in the previous chapter. Thus, Kaplan reports that the New England fishermen she studied believed her to be either a government official or an insurance investigator (Kaplan 1991:233).

Generally, such suspicions quickly dissipate as contact increases; but this is not always the case. And, sometimes, given the nature of the research, it may be difficult to distance oneself from such labels. Hunt (1984:288) reports that the police officers she studied suspected that she was an undercover agent for the Internal Affairs Bureau or the FBI, a suspicion encouraged by officials in the police department in which she was working. But, over and above this, she was, and was known to be, a consultant hired by the city to evaluate the police, a role that could easily be seen as spying by those subject to the evaluation. Despite this, Hunt was able to build trust among the police officers she studied by proving herself reliable in emergencies on the street, and by explicitly criticizing the higher echelons of the police department.

By contrast, 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. Occasionally, they may be, or consider themselves to be, very sophisticated in their knowledge of research methodology, without being familiar with ethnography; and/or they may have a negative attitude towards it. This problem is especially acute, of course, where the people being studied are academics, even sociologists themselves (Platt 1981). Scott provides an example from research on the experience of postgraduate students in British universities. Along with her co-researcher, Scott was asked to present a paper at a graduate seminar in a sociology department in which they had conducted interviews:

Almost before we had finished speaking the professor leapt to his feet and began a diatribe, during which he evinced not simply disagreement with our presentation and methodology, but anger. He took us to task for writing an article in the British Sociological Association's magazine *Network*... because this 'made our research worthless' since we had published before completing the research.... We felt that we had been set up as an example of the 'dangers' of ethnographic research so that this professor could play the big man and knock us down in front of his graduate students. We found out later that the professor had been one of those most vociferous in preference for a large-scale survey when our project had first been mooted.

(Scott 1984:175)

Outside academia there may be less knowledge but equal or greater hostility. The comment of a constable in the Royal Ulster Constabulary, cited by Brewer (1991:16), provides an example: 'If anything gets me down it's bloody sociology. I think it's the biggest load of shite, simple as that.' Brewer notes that for many police officers the word 'sociologist' sounds too much like 'socialist'. But this is not the only source of problems; he quotes a senior police officer:

I think most policemen can't relate to sociology at all, because, you see, the way we're taught everything is black and white: those who do bad should be punished, those who do good should be rewarded. Sociology just seems to turn all that on its head. It would seem to say that all those who are right and honest are wrong. Just to say a man doesn't earn as much money as me and he has to steal to keep his family, well, sociology says that's OK. Another thing, sociology would seem to be saying that those who have wealth and do well do so at the expense of the poor unfortunate.

Where such attitudes prevail, people may challenge the legitimacy of the research and the credentials of the researcher, as Brewer's colleague Kathleen Magee found in their research on the RUC:

pc 1. Look, just hold on a wee minute. What gives you the right to come here and start asking us these personal questions about our families and that?... You're not going to

learn anything about the police while you're here. They're not going to tell you anything... And you know why? Because you're always walking around with that bloody notebook writing everything down, and you're not getting anywhere near the truth... Like, what use is this research you're doing anyway? Is it going to do me or my mates any good? What you doing it for? 'Cos let me tell you, the only people who are going to be interested in your bloody research are the authorities.

This verbal assault continued for some time, but it ended on a less hostile note:

pc 1.... Maybe the police has made me this way, but do you not see that if you're going to come in here asking me questions about my family, if you're going to want to know all these things, I've got to be able to trust you? Like, after this tonight, I'd let you come out in a vehicle with me.

(Brewer 1991:21-2)

As this example shows, whether or not people have knowledge of social research, and whatever attitude they take towards it, they will often be more concerned with what kind of person the researcher is than with the research itself. They will try to gauge how far the ethnographer 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 by the ethnographer to 'impression management'. Impressions 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. Sometimes it may be necessary for the researcher to dress in a way that is very similar to the people to be studied. This is most obviously true in the case of covert research, where 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 what 'passing' in this way can involve:

Clothes were another major difficulty. I was already aware of the importance attached to them by gang members ... 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 – 'ra gallous wae'.

(Patrick 1973:15)

Much the same sort of attention to dress is required in research that is destined to be overt, but where an initial period of gaining trust is necessary. However, in the case of Wolf's research on 'outlaw bikers', it was important not only that he looked like a biker – shoulder-length hair and a heavy beard, leather jacket and studded leather wrist bands, a cut-off denim jacket with appropriate patches, etc. – but also that he had a 'hog', a bike, that would stand scrutiny by experts (Wolf 1991:214).

Even where the research is overt, the researcher's appearance can be an important factor in shaping relationships with people in the field. Van Maanen reports that, having done participant observation as a student at the police academy, in studying the police on the street he

still carried a badge and a gun. These symbols of membership signified to others my public commitment to share the risks of the police life. Aside from a few special events, parades, and civic ceremonies where uniformed bodies were in short supply, I was, as the police said, out of the bag. I dressed for the street as I thought plainclothes officers might – heavy and hard-toed shoes, slit or clip-on ties, and loose-fitting jackets that would not make conspicuous the bulge of my revolver.

I carried with me chemical Mace, handcuffs, assorted keys, extra bullets, and sometimes a two-way portable radio and a concealed two-inch revolver loaned to me by co-workers who felt that I should be properly prepared.

(van Maanen 1991:37–8)

He reports that his 'plainclothes but altogether coplike appearance' caused some confusion for citizens, who tended to assume he was a high-ranking police officer!

Similar considerations, but a rather different outfit, were involved in Henslin's research on the homeless. He sought to dress in a way that would allow him to 'blend in' with the inhabitants of the skid rows he visited. This was necessary both to facilitate rapport and to avoid marking himself out as a target for muggers. At the same time, he needed to look sufficiently like a researcher to have his announcement of that identity believed by people working in shelters for the homeless whom he wished to interview. He solved this problem by carrying an old briefcase that was cheap-looking and whose stitching had unravelled at one corner 'making it look as though I had just snatched it up out of the trash'. He reports:

When I would announce to shelter personnel that I was a sociologist doing research on the homeless, they immediately would look me over – as the status I had announced set me apart from the faceless thousands who come trekking through the shelters – making this prop suddenly salient. To direct their attention and help them accept the announced identity, I noticed that at times I would raise the case somewhat, occasionally even obtrusively setting it on the check-in counter (while turning the side with the separating stitching more toward myself to conceal this otherwise desirable defect).

(Henslin 1990:56–8)

In her research on an elite girls' school in Edinburgh, Delamont recounts a similar concern with dressing in a way that enabled her to preserve relationships with multiple audiences:

I had a special grey dress and coat for days when I expected to see the head and some pupils. The coat was knee-length and very conservative-looking, while the dress was mini-length, to show the pupils I knew what the fashion was. I

would keep the coat on in the head's office, and take it off before I first met pupils.

(Delamont 1984:25)

While those engaged in overt research do not have to copy closely the dress and demeanour of the people they are researching, they may need to alter their appearance and habits a little in order to reduce any sharp differences. In this way they can make people more at ease in their presence; but this is not the only reason for such adjustments, as Liebow notes:

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 acceptable 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 1967:255-6)

In some situations, however, it may be necessary to use dress to mark oneself off from particular categories to which one might otherwise be assigned. Thus, in her research in Nigeria, Niara Sudarkasa found that in order to be able to get answers to her questions in settings where the people did not already know her she had to avoid dressing like a Yoruba woman: 'People were suspicious of the woman with the notebook, the more so because she did not look like the American student she claimed to be.' They suspected she was a Yoruba collecting information for the government:

I was so often 'accused' of being a Yoruba that when I went to a market in which I was not certain I would find a friend to identify me, I made a point of speaking only American-sounding English (for the benefit of the English speakers there) and of dressing 'like an American'. On my first trip to

such a market, I even abandoned my sandals in favour of moderately high heels and put on make-up, including lipstick. (Sudarkasa 1986:175)

In overt participant observation, then, where an explicit research role must be constructed, forms of dress, can 'give off' the message that the ethnographer seeks to maintain the position of an acceptable marginal member, perhaps in relation to several audiences. They may declare affinity between researcher and hosts, and/or they may distance the ethnographer from constraining identities.

There can be no clear prescription for dress other than to commend a high degree of awareness about self-presentation. A mistake over such a simple matter can jeopardize the entire enterprise. Having gained access to the Edinburgh medical school, for instance, Atkinson (1976 and 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 on to 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 constitute 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. Moreover, sometimes providing such services can directly aid the research. In his study of 'survivalists' Mitchell (1991:100)

offered to compose a group newsletter on my word processor and, in doing so, became the recipient of a steady stream of members' written opinions and perceptions. Being editor of 'The Survival Times', as the newsletter came to be known, in turn, legitimated the use of tape recorders and cameras at

group gatherings, [and] provided an entrée to survivalist groups elsewhere around the country.

Participants sometimes come to expect the provision of services, and it may be costly to disappoint them. While in his study of a political campaign organization Corsino often helped out stuffing envelopes, delivering materials, clipping newspapers, etc., on one occasion he refused to scrub floors and help prepare someone's home for a fund-raising reception, on the grounds that he could more usefully spend his time observing the organizational preparations for the event. He describes the result:

The reactions of the campaign manager and volunteer director were more antagonistic than I expected. Over the next several days, I noticed a polite but unmistakable cooling in my relationship with these officials. . . . I began to feel more and more like an ingrate. . . . This, in turn, resulted in a rather barren period of fieldwork observations. . . . At best, I had to become a passive observer.

(Corsino; quoted in Adler and Adler 1987:18)

This is not to say that all the expectations of those in the field are legitimate or should be honoured. Sometimes the ethnographer will have to refuse requests and live with the consequences. Indeed, one must take care not to offer too much, to the detriment of the research.

The value of pure sociability should not be underestimated as a means of building trust. Indeed, the researcher must often try to find ways in which 'normal' social intercourse can be established. This requires finding 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 research in an urban secondary school for boys, outlining the strategies he used 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 significant 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. However, he also notes some feelings of personal disquiet, wondering whether he was unduly exploitative in offering 'friendship' in return for data.

A 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, while never being frank and honest about oneself. And feminists have stressed the importance of this from an ethical point of view also (see, for example, Oakley 1981). At the same time, just as in many everyday situations, as a researcher one often has to suppress or play down one's own personal beliefs, commitments, and political sympathies. 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 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 the 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).

Particular problems arise where the researcher's own religious or political attitudes differ markedly from those of the people being studied. This is illustrated by Klatch's research on women involved in right-wing organizations. She comments:

I often faced an uneasy situation in which the women concluded that because I did not challenge their ideas, I must

agree with them. Nodding my head in understanding of their words, for example, was interpreted as acceptance of their basic beliefs. Thus, the women I interviewed often ended up thanking me for doing the study, telling me how important it was for a like-minded person to convey their perspective. As one pro-family activist told me, 'We need people like you, young people, to restore the faith.' Having successfully gained her trust, this woman then interpreted that trust, and my enthusiasm for learning, as concurrence with her own beliefs. (Klatch 1988:79)

Sometimes, 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 can apply with particular force to the investigation of deviance, where members of stigmatized groups may require reassurance that the ethnographer does not harbour feelings of disapproval, nor intends to initiate action against them.

THE PERSONAL CHARACTERISTICS OF THE RESEARCHER

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, 'race', 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, though the implications of gender vary according to setting and are intertwined with sexual orientation (Roberts 1981, Golde 1986; Whitehead and Conaway 1986; Warren 1988). Revealingly, most concern with the effects of gender has focused on the role of

women fieldworkers: in particular, the way in which their gender bars them from some situations and activities, while opening up others that are not accessible to men. This has long been a theme in the methodological writings of anthropologists, where it has been noted that women may find themselves restricted to the domestic world of fellow women, children, the elderly, and so on. In Golde's study of the Nahua the problem was exacerbated by other characteristics:

What was problematic was that I was unmarried and older than was reasonable for an unmarried girl to be, I was without the protection of my family, and I traveled alone, as an unmarried, virginal girl would never do. They found it hard to understand how I, so obviously attractive in their eyes, could still be single. . . . Being an unmarried girl meant that I should not drink, smoke, go about alone at night, visit during the day without a real errand, speak of such topics as sex or pregnancy, entertain boys or men in my house except in the presence of older people, or ask too many questions of any kind.

(Golde 1986:79-80)

In much the same way, male researchers may find it difficult to gain access to the world of women, especially in cultures where there is a strong division between the sexes.

However, the anthropologist's status as a foreigner can allow some distance to be created from such restrictions. Reflecting on her experience in studying *purdah*, Papanek (1964) 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. Rainbird's experience was similar:

Being female affected my relations in the field insofar as certain activities were exclusive to one sex or the other. Nevertheless, the fact that I towered over most peasants, wore trousers and was an outsider of high social status placed me in a rather ambiguous category that allowed me to attend meetings and visit people freely around the countryside as men did, but not to drink with the men unless other women were present. . . . On the other hand, I had good access to

women's activities and gossip networks, their warmth and affection.

(Rainbird 1990:78–9)

Similar problems and freedoms tied to gender can also arise in research within Western societies. Easterday *et al.* (1977) note that in male-dominated settings women may come up against the male 'fraternity', from which they are excluded; that women may find themselves the object of 'hustling' from male hosts; that 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 may the female researcher sometimes find it difficult to be taken seriously by male hosts, but other females may also display suspicion and hostility in the face of her intrusions. At the same time, Easterday *et al.* also recognize that 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. Similarly, in so far as women are seen as unthreatening, they may gain access to settings and information with relative ease. Thus, common cultural stereotypes of females can work to their advantage in some respects.

Warren provides illustrations of both the restrictions and the leeway that can arise from being a woman researcher:

When I did my dissertation study of a male secretive gay community during the late 1960s and early 1970s, I was able to do fieldwork in those parts of the setting dedicated to sociability and leisure – bars, parties, family gatherings. I was not, however, able to observe in those parts of the setting dedicated to sexuality – even quasi-public settings such as homosexual bath houses . . . and 'tearooms' Thus, my portrait of the gay community is only a partial one, bounded by the social roles assigned to females within the male homosexual world.

She contrasts this with research in a drug rehabilitation centre:

This institution was open to both male and female residents. But as a female researcher, and over several months of observation, I found that men were generally much more ready to talk to me than women. Furthermore, I was generally per-

ceived as harmless by the males, and afforded access bordering on trespass. I vividly remember one day deciding to go upstairs, an action expressly forbidden to anyone not resident in the facility. Someone started to protest; the protest was silenced by a male voice saying, 'aah, what harm can she do, she's only a broad'. Upstairs I went.

(Warren 1988:18)

'Race', ethnicity, and religious affiliation, like gender, can also set limits and pose problems. 'Race' is, of course, not merely a matter of physical characteristics, but relates to 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. Similar problems may arise, however, even where both researcher and researched are black. Whitehead (1986) was seen by the Jamaicans he studied as a 'big', 'brown', 'pretty-talking man'. 'Big' referred not to his size, but to his status as an educated foreigner, and 'pretty-talking' indicated his use of standard rather than dialect English. 'Brown' was the term used by local Jamaicans to refer to a combination of light skin colour and desirable economic and social characteristics. He reports that one of the effects of his being seen in this way was that

when I tried to hold casual conversations or formal interviews with a number of low-income men, they avoided looking me in the face and often suggested that I talk to someone else who was considered a bigger man than they. Frequently they answered me with meaningless 'yes sirs' and 'no sirs'.

(Whitehead 1986:215)

Similarly, Peshkin's experience researching a fundamentalist Protestant school showed that the ethnicity and religious affiliation of the ethnographer can be an important factor in the establishment of field relations:

At Bethany I wanted to be the non-Christian scholar interested in learning about the fundamentalist educational phenomenon that was sweeping the country. [But] I discovered . . . that being Jewish would be the personal fact bearing most on my research; it became the unavoidably salient aspect of my

subjectivity. Bethanyites let me define my research self, but could never rest easy with my unsaved self. I became forcibly aware that the threats to my identity as a Jew were not just a matter of history.

For in the course of inculcating their students with doctrine and the meaning of the Christian identity, Bethany's educators taught us both that I was part of Satan's rejected, humanist world; I epitomized the darkness and unrighteousness that contrasts with their godly light and righteousness. They taught their children never to be close friends, marry, or to go into business with someone like me. What they were expected to do with someone like me was to proselytize.

(Peshkin 1985:13-15)

While this did not force Peshkin out of the setting, it did shape the whole character of the fieldwork.

A similar problem was faced by Magee, a Catholic woman, studying the (predominantly Protestant) Royal Ulster Constabulary in Northern Ireland; but she too managed to establish good relations with many of those in the field:

Over a twelve-month period a field-worker's persistent inquisitiveness is bound to become something of an irritant. . . . But leaving aside instances of momentary irritation, of which there were many . . . most respondents became confident enough in the field-worker's presence to express what were undoubtedly widely held fears about the research. Sometimes these concerns were expressed through humour and ribaldry. The field-worker became known as 'Old Nosebag', and there were long-running jokes about spelling people's names correctly in Sinn Féin's *Republican News*.

(Brewer 1991:21)

Sometimes, belonging to a different ethnic or national group can even 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.

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 studying full time 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.

One reason for this is the effects of age on the researcher's *modus operandi*, as Henslin illustrates, comparing his research on cab drivers, at age 29, with that on the homeless, at age 47:

[In the participant observation study of cab drivers] I gave little thought to danger, as I was caught up in the excitement of the sociological pursuit. Although two or three cabbies were stabbed the first week that I drove a cab, certain that such a thing would not happen to me, I gave the matter little thought.

Now, however, I was once again face to face with street realities, and at this point in my life things no longer looked the same. Age had accomplished what it is rumored to accomplish: It had brought with it a more conservative . . . approach to street experiences. I found myself more frequently questioning what I was doing, and even whether I should do it.

He goes on to describe his hesitation in approaching a group of runaways:

Down the block I saw about half a dozen or so young males and two females clustered in front of a parking lot. Somehow they did not look like the midwestern suburban youth I had come to know. What was most striking about this group was the amount of 'metal' they were displaying, notably the studs protruding from various parts of their bodies.

A few years back those youths would have struck me as another variant group that likely had engrossing experiences

to relate. No longer. They now impressed me as a group that discretion would indicate as being better off left alone.

(Henslin 1990:69–70)

He did in fact make contact with them. They told him that they slept in abandoned buildings, and he immediately began to wonder about how they found these, how they protected themselves from other intruders, etc. However, despite his curiosity he decided that to stay with them at night would be too dangerous.

Age and its associated features can also affect the way people react to the researcher, along with what he or she is and is not allowed to do. An extreme example is provided by Corsaro's (1981) research on nursery school children:

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)

We have limited discussion here to some of the standard face-sheet characteristics of the ethnographer and their implications for research relationships. It is perhaps worth emphasizing that this discussion has not exhausted the personal characteristics that can make a difference. Oboler provides a striking example of this, discussing her husband's acceptance among the Nandi of Kenya:

His first trip to the river to bathe was a crucial test. In a spirit

of camaraderie, as same-sex communal bathing is customary, he was accompanied by a number of young men. Tagging along was an enormous group of curiosity-seeking children and younger adolescents... everyone wanted to know the answer... Was Leon circumcised? In Nandi, male initiation involving adolescent circumcision is the most crucial event in the male life-cycle, without which adult identity, entry into the age-set system, and marriage are impossible. It is also viewed as an important ethnic boundary marker. . . . Fortunately Leon, a Jew by ancestry and rearing, passed the test. I believe that an uncircumcised husband would have made fieldwork in Nandi extremely difficult for me.

(Oboler 1986:37)

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, through adaptation of dress and demeanour, in order to facilitate gaining the necessary data.

FIELD ROLES

In the early days of fieldwork, the conduct of the ethnographer is often little different from that of any layperson faced with the practical need to make sense of a particular social setting. Consider the position of the novice or recruit – a student fresher, a military rookie, a person starting a new job – 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 others to explain what is happening, 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(s) of participants.

Styles provides an example of the early stages of learning to be a participant observer in his research on gay baths. He comments that before he started he assumed that as a gay man he was 'among the "natural clientele" of the baths. It never occurred to me that I might not understand what was going on' (Styles 1979:151). Before going to the bath house he consulted a gay friend who frequented it:

From this conversation, I saw no major problems ahead and laid some tentative research plans. I would first scout out the various scenes of sexual activity in the bath and diagram the bath's physical and sexual layout. After observing the interaction in the various areas, I would start conversations with one or two of the customers, explaining that I was a first-time visitor, and ask them questions about their bath-going. To write fieldnotes, I could use the isolation of some of the downstairs toilets, described by my friend, which had doors that could be locked to ensure privacy.

As might have been expected, his plans did not work out as intended:

The bath was extremely crowded, noisy, and smelly. My first project – scouting out the layout of the bath itself – consisted of twenty or thirty minutes of pushing my way between, around, and beside naked and almost-naked men jamming the hallways. . . . I gave up on field notes when I saw the line to the downstairs toilets had half a dozen men in it . . . more lining up all the time. I did identify the major sexual arenas . . . but these were, for the most part, so dimly lit that I could see few details of behavior and gave up on the orgy room when, after squeezing through a mass of bodies, I stumbled around in the dark, bumped into a clutch of men engaging in group sexual activity, and had my towel torn off while one of them grabbed for my genitals. I gave up on the steam room after the steam poured in and my glasses fogged over. The blaring rock Muzak, the dour looks of the cus-

tomers, and the splitting headache I developed (from what I later learned was the odor of amyl nitrite, a drug inhaled to enhance the sexual experience) effectively killed any desire I had for conversation.

(Styles 1979:138)

He comments that it was 'only through a slow trial-and-error process [that] I gradually came to understand some of the patterns of behavior in the bath' (Styles 1979:139).

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) wrote 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 backyard 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.

This experience of estrangement is what is often referred to as 'culture shock' and it is the stock-in-trade of social and cultural anthropology. Confrontation of the ethnographer with an '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 described by Chagnon from his fieldwork among the Yanomamö. He reports, 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 fantasies, 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 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 con-

cepts directly from the phenomena one experiences in the field. 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 can be 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 researchers 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

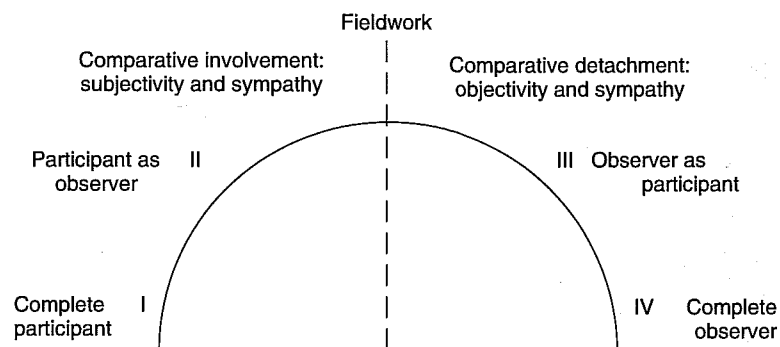


Figure 1 Theoretical social roles for fieldwork

Source: Junker 1960:36; reproduced by permission of University of Chicago Press

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).

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), Pentecostals (Homan 1980), an army unit (Sullivan *et al.* 1958), a mental hospital (Rosenhahn 1973) – as though he or she were an ordinary member 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 (1978a and

b), 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 her 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 somewhat different from our own.

'Complete participation' may seem very attractive. Such identification and immersion in the setting may appear to offer safety: one can 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 usually places great strain on the fieldworker's dramaturgical capacities. And should the ethnographer's cover be 'blown', the consequences may be disastrous for the completion of the fieldwork project, and perhaps also for the researcher personally. Severe embarrassment is the least of the problems that can be expected:

Athena appeared again, and excitedly told me some people wanted to talk to me. . . and she led me into a room where five members of the Council were gathered – the Priests Armat and Wif, and the Masters Firth, Huf and Lare. The latter was the chairman of the Council.

At first, as I walked in, I was delighted to finally have the chance to talk to some higher-ups, but in moments the elaborate plotting that had taken place behind my back became painfully obvious.

As I sat down on the bed beside Huf, Lare looked at me icily. 'What are your motives?' she hissed.

At once I became aware of the current of hostility in the room, and this sudden realization, so unexpected, left me almost speechless.

'To grow,' I answered lamely. 'Are you concerned about the tapes?'

'Well, what about them?' she snapped.

'It's so I can remember things,' I said.

'And the questions? Why have you been asking everyone

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about their backgrounds? What does that have to do with growth?'

I tried to explain. 'But I always ask people about themselves when I meet them. What's wrong with that?'

However, Lare disregarded my explanation. 'We don't believe you,' she said.

Then Firth butted in. 'We have several people in intelligence in the group ... We've read your diary ...'.

At this point ... I couldn't think of anything to say. It was apparent now they considered me some kind of undercover enemy or sensationalist journalist out to harm or expose the Church, and they had gathered their evidence to prove this. ... Later Armat explained that they had fears about me or anyone else drawing attention to them because of the negative climate towards cults among 'humans'. So they were afraid that any outside attention might lead to the destruction of the Church before they could prepare for the coming annihilation. However, in the tense setting of a quickly convened trial, there was no way to explain my intentions or try to reconcile them with my expressed belief in learning magic. Once Firth said he read my diary, I realized there was nothing more to say.

'So now, get out,' Lare snapped. 'Take off your pentagram and get out.'

As I removed it from my chain, I explained that I had driven up with several other people and had no way back.

'That's your problem,' she said. 'Just be gone by the time we get back.' Then, threateningly, she added: 'You should be glad that we aren't going to do anything else.'

(Scott 1983:132-3)

Fortunately, Scott had already collected a substantial amount of data before her identity as a researcher was discovered; and the group she was involved with decided against violent reprisals.

Even if successfully maintained, the strategy of 'complete participation' will normally prove rather limiting. The range and character of the data that can be collected will often be quite restricted. 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 possibilit-

ies. 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.

These limitations of complete participation are 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 the Gregors stopped 'pretending' that they were 'becoming' Brazilian villagers, and turned to systematic research activity.

In contrast to the 'complete participant', the 'complete observer' has no contact at all with those he or she is observing. Thus, Corsaro (1981) complemented his participant observation with nursery school children by observing them 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 accounts 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 earlier, 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 one – 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 completely fixed in character (Turner 1962).

Of course, 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). And sometimes even in open research there may be no choice but to adopt an established role, as Freilich (1970a and b) found out in his research on Mohawk steelworkers in New York. Having become friends with 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 taboo. . . . I could use no pencils, notebooks or questionnaires. I even failed in attempts to play the semi-anthropologist. 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 1970a and b:193)

Currer (1992) reports much the same experience in negotiating access to Pathan women informants:

Once permission to visit was given, the visits were on social terms: my agenda and public domain purpose were never referred to. When once I did so, the women concerned were very offended and our relationship was jeopardized. Yet the women, no less than the men, knew of my research purpose. Only in two cases did the relationship more closely combine the personal and the professional. In these cases I was able to take notes and to lead the exchange.

She concludes that she 'had to choose between insisting on my

rules and being denied any real access or [visiting] on the women's terms' (Currer 1992:17–18).

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, Aggleton 1987; Mac an Ghaill 1991), but sometimes they have not (Brown 1987; Walker 1988; Stanley 1989; Riddell 1992). Perhaps not surprisingly, they have rarely taken on the role of school student (but see Llewellyn 1980), although in studies of higher education ethnographers do sometimes enrol as students (Moffat 1989; Tobias 1990).

Decisions about the sort of 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. Thus, Seigney (1981), studying art classes in a college, collected data by surreptitiously taking on the role of student, and 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: it ranges from the 'external' view of the observer to the 'internal' view of the participant. However, this dimension is surrounded by what Styles refers to as outsider and insider myths:

In essence, outsider myths assert that only outsiders can conduct valid research on a given group; only outsiders, it is held, possess the needed objectivity and emotional distance. According to outsider myths, insiders invariably present their group in an unrealistically favourable light. Analogously, insider myths assert that only insiders are capable of doing

valid research in a particular group and that all outsiders are inherently incapable of appreciating the true character of the group's life.

Insider and outsider myths are not empirical generalizations about the relationship between the researcher's social position and the character of the research findings. They are elements in a moral rhetoric that claims exclusive research legitimacy for a particular group.

(Styles 1979:148)

Of course, it is true that outsiders and insiders are likely to have immediate access to different sorts of information. And they are also exposed to different kinds of methodological dangers. The danger that attends the role of complete observer is that of failing to understand the perspectives of participants. Where this strategy is used alone, these perspectives have to be inferred from what can be observed plus the researcher's background knowledge, without any possibility of checking these interpretations against what participants say in response to questions. The risk here is not simply of missing out on an important aspect of the setting, but rather of seriously misunderstanding the behaviour observed.

A more common danger in ethnographic research, one that attends the other three roles in Junker's typology, is '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 subtle, perhaps, is the danger of identifying with such members' perspectives, and hence of failing to treat these as problematic.

One well-known British ethnography that is 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 'ear-oles', who subscribe to the values of the school. The 'lads' not only see little chance of obtaining middle-class jobs but have no desire for them, enthusiastically seeking 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 appears 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', and to have taken over their views in the analysis, where they did not conflict with his own. Hence, the book becomes as much a celebration of them as anything else: Willis appears unable or unwilling adequately to distance himself from their accounts. Second, the 'lads' are endorsed by Willis, since he treats them more or less as spokesmen for the working class. While he explicitly recognizes that working-class culture is variable, he nonetheless seems to identify the views held by the 'lads', or those of some of them, as representative in important respects of true working-class consciousness. Since the 'ear-oles' or conformists are also from working-class backgrounds,

this is problematic, to say the least. To a large extent, Willis is guilty of identifying with his chosen twelve, and his theoretical description of schooling is distorted by this.

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)

While ethnographers may adopt a variety of roles, the usual aim throughout is to maintain a more or less marginal position, thereby providing access to participant perspectives but at the same time minimizing the dangers of over-rapport. As Lofland (1971:97) points out, the researcher can also generate creative insight out of this marginal position of simultaneous insider-outsider. The ethnographer needs to be intellectually poised between familiarity and strangeness; and, in overt participant observation, socially he or she will usually be poised between stranger and friend (Powdermaker 1966; Everhart 1977). As the title of the collection edited by Freilich (1970b) suggests, the ethnographer is typically a 'marginal native'.

THE STRAINS AND STRESSES OF FIELDWORK

Marginality is not an easy position to maintain, however, because it engenders a continual sense of insecurity. It involves living simultaneously in two worlds, that of participation and that of research. In covert research there is the constant effort to maintain one's cover and at the same time to make the most of whatever research opportunities arise. In overt participant observation there is the strain of living with the ambiguity and uncertainty of one's social position on the margin, and doing so in a way that serves the research but is also ethically acceptable. To one degree or another, as Thorne (1983:221) puts it, one is often 'running against the grain' of the settings in which one works.

Johnson (1975) has recorded in some detail his emotional and physical reactions to the stresses of fieldwork. Some of his fieldnotes document his response with notable 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 1975:152-3)

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 discomfort 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 various 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 diaries reveal many indications of similar kinds 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. She 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. At the same time, 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 (1971:108–9) draws attention to the 'poignancy' of this experience. There is a sense of schizophrenia that the disengaged/engaged ethnographer may suffer. But this feeling, and equivalent feelings, should be managed for what they are. Such feelings 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 labelling 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 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 principle 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 now move on to a new social setting. Alternatively, it may be the case that a sense of familiarity has been produced 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.

Marginality is not the only source of strain and stress in fieldwork, of course. Another is finding oneself in physical and social situations that one might not otherwise encounter and would normally avoid. Henslin provides an example from his participant observation research on the homeless:

It was not the shelter's large size and greater impersonality . . . that brought culture shock. It was, rather, its radically different approach to the homeless. For example, at check-in each man was assigned a number. At the exact designated time the man located a bed marked with that number, one that held at its foot a similarly-numbered basket. Each man then undressed at his bedside and waited in the nude until his number was called. Still nude, he then had to parade in front of the other hundred and nine men, carrying his clothing . . . to a check-in center operated by clothed personnel. . . . After showering, but still standing in the nude and surrounded by nude strangers, each man was required to shave, using the common razors laid out by the sinks. Finally, still nude, he took the long walk back to his assigned bed.

This routine burst upon me as a startling experience. . . . For me . . . to parade nude in front of strangers, . . . and to witness man after man parading nude was humiliating and degrading, a frontal assault on my sensibilities.

Nor was that night spent peacefully. Gone now was my cuddly sleeping partner of the past dozen years. Gone were my familiar surroundings. And, especially, gone was the lock that protected me from the unknown. . . .

Then my mind insisted on playing back statements made by one of the directors of the shelter. Earlier that day, as I was interviewing him, . . . he mentioned homosexual rapes

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that had occurred in the dormitories. Then during the interview two men had to be removed from the dining hall after they drew a knife and a pistol on one another. When I told him that I was planning to spend the night and asked him if it was safe, instead of the reassurance I was hoping for, he told me about a man who had pulled a knife on him and added, 'Nothing is really safe. You really have to be ready to die in this life.'

That was certainly not the most restful night I have ever spent, but by morning I was sleeping fairly soundly. I knew that was so because in the early hours, at 5.35 to be exact, the numerous overhead lights suddenly beat onto my upturned face while simultaneously over the loudspeaker a shrieking voice trumpeted, 'Everybody up! Everybody up! Let's get moving!'

(Henslin 1990:60-1)

Women fieldworkers are sometimes thought to be especially vulnerable to attack, particularly sexual attack. As Warren (1988:30) notes, the question of sexuality in fieldwork first arose in the context of safety from rape of 'white women' alone in 'primitive' societies. She argues for a wider perspective, noting the reports of fieldworkers' sexual participation in the field (see also Fine 1993). Nevertheless, sexual harassment, at the very least, can be a problem. Warren reports the research of one of her students Liz Brunner among the homeless:

During her fieldwork, Liz slept, drank, talked, and shared meals with the homeless on Los Angeles streets – almost all of whom were male. After several episodes of unwanted physical touching, she learned to avoid being alone with particular men, or going into dark areas of the street with those she did not know well. . . . These homeless men – some of them de-institutionalised mental patients – often did not share, or perhaps know about, Liz's middle class, feminist values and beliefs concerning sexual expression and male-female relationships.

(Warren 1988:33-4)

Such problems are not, of course, restricted to contacts with the homeless on the streets, as Gurney reports from her research on lawyers:

One clear-cut example of a problem related to my gender was an instance of sexual hustling on the part of one of the prosecutors. He tried, on several different occasions, to get me to come over to his apartment on the pretense of having me use his computer. . . . When that failed, he asked me if I knew anyone who might be willing to come to his apartment to help him program his computer to analyze bank accounts in embezzlement cases. I said I did not know anyone, but offered to post an advertisement for him at the university. He rejected that idea and never raised the issue again.

(Gurney 1991:58-9)

Unpleasant fieldwork experiences do not arise solely from what may be done *to* the ethnographer, however. Even more distressing can be what the participant observer feels it necessary to do in order to maintain the participant role. This is a problem that is especially likely to occur where the complete participant observation role has been adopted, since here, as we noted earlier, there is usually less scope for manoeuvre. The situation is also exacerbated where the people with whom one is involved are prone to violence. In such circumstances, one may find oneself drawn deep into activities that are obnoxious and dangerous, as Mitchell found in his research on survivalists:

Alone, two thousand miles away from home, on the third day of the Christian Patriots Survival Conference, I volunteered for guard duty. . . . The Aryan nations were there, with the Posse Comitatus, and the Klan. In the names of Reason and Patriotism and God they urged repudiation of the national debt, race revolution, economic assistance to small farmers, and genocide. . . . Four of us were assigned the evening gate watch. Into the dusk we directed late-arriving traffic, checked passes, and got acquainted. The camp settled. Talk turned to traditional survivalist topics. First, guns: They slid theirs one by one from concealed holsters to be admired. 'Mine's in the car,' I lied. Then, because we were strangers with presumably a common cause, it was time for stories, to reconfirm our enemies and reiterate our principles. We stood around a small camp fire. . . . Our stories went clockwise. Twelve O'clock told of homosexuals who frequent a city park in his home community and asked what should be done with them in 'the future'. His proposal involved chains and trees and long-

fused dynamite taped to body parts. Understand these remarks. They were meant neither as braggadocio nor excessive cruelty, but as a reasoned proposal. We all faced the 'queer' problem didn't we? And the community will need 'cleansing' won't it? In solemn agreement we nodded our heads. Three O'clock reflected for a moment, then proposed a utilitarian solution regarding nighttime and rifle practice. 'Good idea,' we mumbled supportively. . . . One more car passed the gate. It grew quiet. It was Nine O'clock. My turn. I told a story, too. As I began a new man joined us. He listened to my idea and approved, introduced himself, then told me things not everyone knew, about plans being made, and action soon to be taken. He said they could use men like me and told me to be ready to join. I took him seriously. Others did, too. He was on the FBI's 'Ten Most Wanted' list. If there are researchers who can participate in such business without feeling, I am not one of them nor do I ever hope to be. What I do hope is someday to forget, forget those unmistakable sounds, my own voice, my own words, telling that Nine O'clock story.

(Mitchell 1991:107)

Here we are reminded that field researchers do not always leave the field physically and emotionally unscathed, and they rarely leave unaffected by the experience of research. But even where very distressing, the experience is rarely simply negative, as Cannon indicates on the basis of her research on women with breast cancer:

It would sound overdramatic to say that it 'changed my life' (although it has a lasting effect) but it certainly 'took over' my life in terms of emotional involvement in ways I was not altogether prepared for, and taught me a number of 'extra curricular' lessons about life and death, pain and endurance, and human relationships.

(Cannon 1992:180)

LEAVING THE FIELD

With all research there comes a time when the fieldwork needs to be terminated. Often this is determined by the non-availability of further resources, or by the approach of deadlines for

the production of written reports. With the exception of those who are doing research in a setting within which they normally live or work, ending the fieldwork generally means leaving the field – though sometimes the setting itself disintegrates, as Gallmeier (1991:226) found in his research on a professional hockey team:

Compared to some other field researchers . . . I had a less difficult time disengaging from the setting and the participants. This was attributable largely to the fact that once the season is over the players rapidly disperse and return to summer jobs and families in the 'Great White North'. In late April the Rockets were eliminated in the third round of the playoffs and the season was suddenly over. In just a few days the majority of the Rockets left Summit City.

Virtually overnight, the people he had been studying dispersed geographically, though he was able to follow up individuals subsequently.

Most ethnographers, however, must organize leaving the field, and this is not always a straightforward matter. Like all other aspects of field relations it usually has to be negotiated. Indeed, sometimes participants are reluctant to let the researcher go, for a variety of reasons. David Snow's first attempts at disengagement from a group of Nichiren Shosnu Buddhists were met with a flurry of reconversion activity:

No sooner had I finished (telling my group leader about my growing disillusionment) than he congratulated me, indicating that (such feelings) were good signs. He went on to suggest that . . . something is really happening in my life. . . . Rather than getting discouraged and giving up, I was told to chant and participate even more. He also suggested that I should go to the Community Center at 10:00 this evening and get further guidance from the senior leaders. . . . Later in the evening my group leader stopped by the apartment at 10:00 – unannounced – to pick me up and rush me to the Community Center to make sure that I received 'guidance'.

While I was thus trying to curtail my involvement and offer what seemed to be legitimate reasons for dropping out, I was yet being drawn back at the same time.

(Snow 1980:110)

Leaving the field is not usually as difficult as this; it is generally more a matter of saying goodbye to those with whom one has established relationships, making arrangements for future contacts (for example in order to feed data or findings back to them), and generally smoothing one's departure. And leaving does not necessarily mean breaking off all relationships with those one has come to know while working there. Most ethnographers retain friends and acquaintances from their periods of fieldwork, sometimes for a long time. A sad exception is Cannon, whose friends from her research were progressively depleted as they died from cancer (Cannon 1992).

However smoothly managed, though, leaving can be an emotional experience. It can sometimes be strange and disorienting for people in the setting to find that the ethnographer is no longer going to be a part of their everyday world. Informants must adjust to the fact that someone they have come to see as a friend is going to turn back into a stranger, at least to some degree. For the ethnographer too the experience may sometimes be traumatic. An extreme case is that of Young, where the end of the fieldwork coincided with his retirement from the police:

In the months since I retired and have been compiling the material for this book, I have become crucially aware that . . . I have been . . . involved in what I have decided can only be a deconstruction of an identity. Shedding the institutional framework and the heavy constraints of a disciplined organization after thirty-three years, like the snake sheds his skin, has been another culture shock. . . . During this time I have dreamed regularly (in full colour) of situations where I am in half or partial uniform, often, for example, in police tunic but civvy trousers, and without epaulettes on the jacket or buttons and badges of rank. In these dreams, in which I was often with ex-colleagues from the distant past, I somehow was aware that I was now standing outside my police identity, but had still to throw off the last vestiges of it.

(Young 1991:391)

Frequently, the ethnographer leaves the field with mixed feelings, but sometimes with not a little relief.

CONCLUSION

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. As we have seen, there is a variety of roles the ethnographer may adopt in the field, carrying with them a range of advantages and disadvantages, opportunities and dangers. In addition, by systematically modifying field roles, it may be possible to collect different kinds of data whose comparison can greatly enhance interpretation of the social processes under study. However, establishing and maintaining field relations can be a stressful as well as an exciting experience, and ethnographers must learn to cope with their own feelings if they are to sustain their position as a marginal native and complete the fieldwork.

The various roles which ethnographers establish within settings are, of course, the bases from which data can be collected. One form of data is researchers' descriptions of people's behaviour, of what they do and say in various circumstances. Equally important, though, is information that people in the setting can provide about their own beliefs and feelings, and about their own and others' behaviour now and in the past. In the next chapter we consider the role of such insider accounts in ethnographic research.