Ethnography
Principles in practice
Third edition

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and Paul Atkinson
The more ancient of the Greeks (whose writings are lost) took up ... a position ... between the presumption of pronouncing on everything, and the despair of comprehending anything; and though frequently and bitterly complaining of the difficulty of inquiry and the obscurity of things, and like impatient horses champing at the bit, they did not the less follow up their object and engage with nature, thinking (it seems) that this very question — viz., whether or not anything can be known — was to be settled not by arguing, but by trying. And yet they too, trusting entirely to the force of their understanding, applied no rule, but made everything turn upon hard thinking and perpetual working and exercise of the mind.

(Francis Bacon 1620)
Prologue to the third edition

The first edition of this book, which appeared in 1983, was the result of a collaboration of several years' standing. In the late 1970s, when we started working together, there was only a small literature for us to draw on. Our book filled a very obvious gap. There were some influential texts, all deriving from the United States and reflecting the interactionist tradition in sociology. Key authors such as Anselm Strauss and John Lofland set the scene for our collective understanding of ethnography. There were also methodological appendices to well-known monographs, but there was little or nothing that combined general methodological principles with their practical applications. We constructed a shared approach to the appropriate research strategies and intellectual stances associated with ethnographic work, and brought together a wide range of sources and examples.

At the time of our first edition, there were influential strands of ethnographic research in key areas like deviance, education, medicine and studies of work. But in most of the social sciences (with the obvious exception of social anthropology) ethnography was a distinctly minority interest. Moreover, while anthropologists took its value for granted, or perhaps because of this, on the whole they paid singularly little attention to the documentation or discussion of research methods.

Much has changed since the early 1980s. The volume of methodological writing has expanded greatly and continues to do so unabated; though, of course, the pattern of this has varied in different countries, and rather different narratives concerning the history of ethnography and qualitative research have been provided (Burawoy et al. 2000: intro; Weber 2001; Denzin and Lincoln 2005: intro; McCall 2006). By the time we wrote our second edition, which appeared in 1995, the methodological landscape had already shifted. There were, by then, a great many methods texts and commentaries available. The social sciences seemed to have experienced a 'methodological turn'. Graduate students throughout the world were receiving more training in the techniques of social research. There was an increasing awareness of research methods as an area of special interest, as well as being the core of practising social scientists' craft skills. That trend has continued, fuelled by a virtuous circle of research funding, postgraduate and postdoctoral opportunities, and the interests of commercial publishers. The sheer number of methodology texts and papers has become quite overwhelming. The domain of what is broadly labelled 'qualitative research' now spans a wide range of disciplines and sub-fields, and incorporates a variety of research styles and strategies. Work of this kind has become a central feature of sociology, cultural and media studies, cultural geography, educational research, health and nursing research, business and organization studies; involving the use of participant observation, individual and group interviews,
focus groups, visual methods, conversation- and discourse-analytic techniques, and so on. Qualitative researchers have developed specialist literatures devoted to quite specific techniques—photographic and other visual methods (Pink 2006), narrative-analytic methods (Riessman 1993), interviewing of many sorts (Gubrium and Holstein 2002), the qualitative analysis of documentary sources (Prior 2003), and the exploration of virtual social realities (Hine 2000).

There are now available numerous major works on the variety of qualitative research methods and perspectives (e.g. Gubrium and Holstein 1997; Seale et al. 2004; Silverman 2004). The fashionable status of qualitative research has been encouraged by its alleged alignment with various tendencies within the social and cultural disciplines: such as ‘critical’ research, postmodernism, feminism and postfeminism, or postcolonialism. Qualitative inquiry has been promoted as having intrinsic political and ethical value, in giving voice to marginalized and otherwise muted groups and/or in challenging the powerful. The development and dissemination of qualitative research has been accompanied by recurrent announcements of innovation, renewal and paradigm-change within qualitative research itself.

Confronted with this exponential growth in writing about qualitative research, its increasing popularity, and the heady claims entered on its behalf, there is clearly a need for us to try to locate this third edition of our book within the broader methodological terrain. First, we need to reaffirm that it focuses on ethnography, rather than qualitative research in general; even though there are no hard and fast boundaries. This means that it is primarily concerned with field research involving a range of methods, with participant observation being given particular emphasis. It is not necessary to think naively in terms of naturally occurring communities or isolated populations, nor do we need to entertain romantic visions of social exploration, in order to insist on the continued importance of participant observation in, and first-hand engagement with, social worlds (Delamont 2004b).

As many reviews of ethnography reveal, this is a variegated approach that is amenable to different emphases and nuances (see, for instance, Atkinson et al. 2001). We are not in the business of trying to impose a single orthodoxy here. It is, after all, a particular virtue of ethnographic research that it remains flexible and responsive to local circumstances. We do, however, emphasize the importance of the tradition of ethnographic research, and what guidance can be drawn from it.

In restating the importance of the ethnographic tradition we remain sceptical about many claims for innovation and novelty in research methods. Some readers might assume that, in producing a third edition of a work that first appeared in 1983, we would have removed all ‘old’ sources and references in the process of ‘updating’. We have not done so, even though we have included more recent examples and developments. We are frequently disturbed by the widespread misapprehension that qualitative research in general, and ethnographic research in particular, are somehow novel approaches in the social sciences, or that research strategies that were current for most of the twentieth century are now redundant. Research communities that overlook their own past are always in danger of reinventing the wheel, and of assuming novelty when all that is really revealed is collective ignorance or amnesia (Atkinson et al. 1999). The collective memory of many research networks is far too shallow, in our view. We have, therefore, retained material that spans the first and second editions of our book.

The most visible preachers of novelty and change have been Norman Denzin and Yvonna Lincoln (Denzin and Lincoln 2005). They have helped to shape the current landscape of qualitative research more than most. The successive editions of their monumental *Handbook* have been remarkable not just for their scale and scope, but also for promoting a view of the history of qualitative research (and, by implication, of ethnography) as marked by a series of evolutionary transformations. They construct a developmental narrative of qualitative research that portrays sharp discontinuities and an increasing rate of change, following a broad trajectory from ‘modernist’ to ‘postmodernist’ standpoints.

Now there is no doubt that change has occurred and will continue to take place. And some of it has been of great value. The original influences of anthropology have been challenged and supplemented, and there has been major change within that discipline as well. The interactionist foundations of ethnographic work in sociology have been enriched by ideas stemming from many other sources. But we believe that current narratives of radical transformation are overstated, and sometimes simply wrong. Differences between past and current principles and practices are often exaggerated, and distorted views about the past are promoted. Equally important, in the championing of ‘new’ approaches, there has been a failure, often, to recognize the difficulty and complexity of the methodological issues that face ethnographers, along with other social scientists.

Recent trends, in some research fields, towards a re-emphasis on the importance of experimental method, and quantitative techniques more generally, sometimes labelled as a form of methodological fundamentalism, should not be met with an equivalent fundamentalism, in which the virtues of qualitative research are blindly extolled. Whatever the future has in store, in order to deal with it we must learn from the past as well as taking account of current circumstances and new ideas. In producing this third edition, we have tried to strike a balance between preserving the past and nurturing the new.
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1 What is ethnography?

Ethnography is one of many approaches that can be found within social research today. Furthermore, the label is not used in an entirely standard fashion; its meaning can vary. A consequence of this is that there is considerable overlap with other labels, such as ‘qualitative inquiry’, ‘fieldwork’, ‘interpretive method’, and ‘case study’, these also having fuzzy semantic boundaries. In fact, there is no sharp distinction even between ethnography and the study of individual life histories, as the example of ‘auto’ ethnography’ shows; this referring to an individual researcher’s study of his or her own life and its context (Reed-Danahay 1997, 2001; Holman Jones 2005). There is also the challenging case of ‘virtual ethnography’, whose data may be restricted entirely to what can be downloaded from the internet (Markham 1998, 2005; Hine 2000; Mann and Stewart 2000). While, for the purposes of this opening chapter, we will need to give some indication of what we are taking the term ‘ethnography’ to mean, its variable and sometimes contested character must be remembered; and the account we provide will inevitably be shaped by our own views about what form ethnographic work ought to take.

The origins of the term lie in nineteenth-century Western anthropology, where an ethnography was a descriptive account of a community or culture, usually one located outside the West. At that time ‘ethnography’ was contrasted with, and was usually seen as complementary to, ‘ethnology’, which referred to the historical and comparative analysis of non-Western societies and cultures. Ethnology was treated as the core of anthropological work, and drew on individual ethnographic accounts which were initially produced by travellers and missionaries. Over time, the term ‘ethnology’ fell out of favour because anthropologists began to do their own fieldwork, with ‘ethnography’ coming to refer to an integration of both first-hand empirical investigation and the theoretical and comparative interpretation of social organization and culture.

As a result of this change, since the early twentieth century, ethnographic fieldwork has been central to anthropology. Indeed, carrying out such work, usually in a society very different from one’s own, became a rite of passage required for entry to the ‘tribe’ of anthropologists. Fieldwork usually required living with a group of people for extended periods, often over the course of a year or more, in order to document and interpret their distinctive way of life, and the beliefs and values integral to it.

Moreover, during the twentieth century, anthropological ethnography came to be one of the models for some strands of research within Western sociology. One of these was the community study movement. This involved studies of villages and towns in the United States and Western Europe, often concerned with the impact of urbanization and industrialization. A landmark investigation here was the work of the Lynds in documenting life in Muncie, Indiana, which they named ‘Middletown’ (Lynd and Lynd 1929, 1937).
2 What is ethnography?

In a parallel development, many sociologists working at the University of Chicago from the 1920s to the 1950s developed an approach to studying human social life that was similar to anthropological research in some key respects, though they often labelled it ‘ethnography’. The ‘Chicago School’ was concerned with documenting the range of different patterns of life to be found in the city, and how these were shaped by the developing urban ecology.

From the 1960s onwards, forms of sociological work influenced by these developments, especially by Chicago sociology, spread across many sub-fields of the discipline, and into other disciplines and areas of inquiry as well; and they also migrated from the United States to Europe and to other parts of the world. Furthermore, for a variety of reasons, an increasing number of anthropologists began to do research within Western societies, at first in rural areas but later in urban locales too.1 Another relevant development in the latter half of the twentieth century was the rise of cultural studies as an area of investigation distinct from, but overlapping with, anthropology and sociology. Work in this field moved from broadly historical and textual approaches to include the use of ethnographic method, notably in studying audiences and the whole issue of cultural consumption. Furthermore, in the later decades of the twentieth century, ethnography spread even further, for example into psychology and human geography. Indeed, it tended to get swallowed up in a general, multidisciplinary, movement promoting qualitative approaches; though the term ‘ethnography’ still retains some distinctive connotations.2

This complex history is one of the reasons why ‘ethnography’ does not have a standard, well-defined meaning. Over the course of time, and in each of the various disciplinary contexts mentioned, its sense has been reinterpreted and reconceptualized in various ways, in order to deal with particular circumstances. Part of this remoulding has arisen from the fact that ethnography has been associated with, and also put in opposition to, various other methodological approaches. Furthermore, it has been influenced by a range of theoretical ideas: anthropological and sociological functionalism, philosophical pragmatism and symbolic interactionism, Marxism, phenomenology, hermeneutics, structuralism, feminism, constructionism, post-structuralism and post-modernism. Increasingly, it has been compared and contrasted not just with experimental and survey research but also with interview-based studies, macro-historical analysis, political economy, conversation and discourse analysis, and psycho-social approaches.

In short, ‘ethnography’ plays a complex and shifting role in the dynamic tapestry that the social sciences have become in the twenty-first century. However, this term is by no means unusual in lacking a single, standard meaning. Nor does the uncertainty of sense undermine its value as a label. And we can outline a core definition, while recognizing that this does not capture all of its meaning in all contexts. In doing this we will focus, initially, on a fairly practical level: on what ethnographers actually do, on the sorts of data that they usually collect, and what kind of analysis they deploy to handle those data. Later we will broaden the discussion to cover some of the ideas that have informed, and continue to inform, ethnographic practice.

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1 For an account of the development and reconfiguration of ethnographic work within British anthropology, see Macdonald (2001).
2 Diverse strands and trends of the qualitative research movement are exemplified in the various editions of the Handbook of Qualitative Research: Denzin and Lincoln (1994, 2000, 2005).

3 These methods can include those that are ‘unobtrusive’. Lee (2000). There has been some dispute about whether ethnographic studies can rely entirely on interview or documentary data, without complementary participant observation. See Atkinson and Coffey (2002).
collection of data to pursue answers to those questions more effectively, and to test these against evidence.

Collecting data in 'natural' settings, in other words in those that have not been specifically set up for research purposes (such as experiments or formal interviews) also gives a distinctive character to ethnographic work. Where participant observation is involved, the researcher must find some role in the field being studied, and this will usually have to be done at least through implicit, and probably also through explicit, negotiation with people in that field. Access may need to be secured through gatekeepers, but it will also have to be negotiated and renegotiated with the people being studied; and this is true even where ethnographers are studying settings in which they are already participants. In the case of interviewing, too, access cannot be assumed to be available automatically; relations will have to be established, and identities co-constructed.

The initial exploratory character of ethnographic research means that it will often not be clear where, within a setting, observation should begin, which actors need to be shadowed, and so on. Sampling strategies will have to be worked out, and changed, as the research progresses. Much the same is true of the use of interviews. Here, decisions about whom to interview, when, and where, will have to be developed over time, and the interviewing will normally take a relatively unstructured form, though more structured or strategic questioning may be used towards the end of the fieldwork. Furthermore, as already noted, the data will usually be collected in an unstructured form, by means of fieldnotes written in concretely descriptive terms and also through audio- or video-recordings, plus the collection of documents. Given the nature of these data, a considerable amount of effort, and time, will need to go into processing and analysing them. In all these respects, ethnography is a demanding activity, requiring diverse skills, including the ability to make decisions in conditions of considerable uncertainty.

This is true despite the fact that, as a set of methods, ethnography is not far removed from the means that we all use in everyday life to make sense of our surroundings, of other people's actions, and perhaps even of what we do ourselves. What is distinctive is that it involves a more deliberate and systematic approach than is common for most of us most of the time, one in which data are specifically sought to illuminate research questions, and are carefully recorded; and whereas the process of analysis draws on previous studies and involves intense reflection, including the critical assessment of competing interpretations. What is involved here, then, is a significant development of the ordinary modes of making sense of the social world that we all use in our mundane lives, in a manner that is attuned to the specific purposes of producing research knowledge.

In the remainder of this chapter we will explore and assess a number of methodological ideas that have shaped ethnography. We shall begin by looking at the conflict between quantitative and qualitative method as competing models of social research, which raged across many fields in the past and still continues in some even today. This was often seen as a clash between competing philosophical positions. Following some precedent we shall call these 'positivism' and 'naturalism': the former privileging quantitative methods, the latter promoting ethnography as the central, if not the only legitimate, social research method. After this we will look at more recent ideas that have shaped the thinking and practice of ethnographers, some interpretations of which are at odds with the earlier commitment to naturalism.

Positivism versus naturalism

Positivism has a long history in philosophy, but it reached its high point in the 'logical positivism' of the 1930s and 1940s (Kolakowski 1972; Halfpenny 1982; Friedman 1991; Hammersley 1995: ch. 1). This movement had a considerable influence upon social scientists, notably in promoting the status of experimental and survey research and the quantitative forms of analysis associated with them. Before this, in both sociology and social psychology, qualitative and quantitative techniques had generally been used side by side, often by the same researchers. Nineteenth-century investigators, such as Mayhew (1861), LePlay (1879) and Booth (1902–3), treated quantitative and qualitative data as complementary. Even the sociologists of the Chicago School, often portrayed as exponents of participant observation, employed both 'case-study' and 'statistical' methods. While there were recurrent debates among them regarding the relative advantages and uses of the two approaches, there was general agreement on the value of both (Bulmer, 1984; Harvey 1985; Hammersley 1989a; Deegan 2001). It was only later, with the rapid development of statistical methods and the growing influence of positivist philosophy, that survey research came to be regarded by some of its practitioners as a self-sufficient methodological tradition.

Today, the term 'positivism' has become little more than a term of abuse among social scientists, and as its meaning has become obscured. For present purposes, the major tenets of positivism can be outlined as follows:

1. **The methodological model for social research is physical science, conceived in terms of the logic of the experiment.** While positivists do not claim that the methods of all the physical sciences are the same, they do argue that these share a common logic. This is that of the experiment, where quantitatively measured variables are manipulated in order to identify the relationships among them. This logic is taken to be the defining feature of science.

2. **Universal or statistical laws as the goal for science.** Positivists adopt a characteristic conception of explanation, usually termed the 'covering law' model. Here events are explained in deductive fashion by appeal to universal laws that state regular relationships between variables, holding across all relevant circumstances. However, it is the statistical version of this model, whereby the relationships have only a high probability of applying across relevant circumstances, that has generally been adopted by social scientists; and this has encouraged great concern with sampling procedures and statistical analysis, especially in survey research. Here, a premium is placed on the generalizability of findings.

3. **The foundation for science is observation.** Finally, positivists give priority to phenomena that are directly observable, or that can be logically inferred from what is observable; any appeal to intangibles runs the risk of being dismissed as metaphysical speculation. It is argued that scientific theories must be founded upon, or tested by appeal to, descriptions that simply correspond to the state of

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4. 'Naturalism' is a term which is used in a variety of different, even contradictory, ways in the literature; see Matus (1969). Here we have simply adopted the conventional meaning within the ethnographic literature.

5. In social psychology this process started rather earlier, and it was the experiment which became the dominant method.
under criticism as lacking scientific rigour. Ethnography was sometimes dismissed as quite inappropriate to social science, on the grounds that the data and findings it produces are 'subjective', mere idiosyncratic impressions or personal views. In reaction, ethnographers developed an alternative view of the nature of social research, which they often termed 'naturalism' (Lofland 1967; Blumer 1969; Matza 1969; Denzin 1971; Schatzman and Strauss 1973; Guba 1978). Like positivism, this appealed to natural science as a model, but the latter's method was conceptualized differently, and the exemplar was usually a nineteenth-century biology rather than twentieth-century physics.

Naturalism proposes that, as far as possible, the social world should be studied in its 'natural' state, undisturbed by the researcher. Hence, 'natural' not 'artificial' settings, like experiments or formal interviews, should be the primary source of data. Furthermore, the research must be carried out in ways that are sensitive to the nature of the setting and that of the phenomena being investigated. The primary aim should be to describe what happens, how the people involved see and talk about their own actions and those of others, the contexts in which the action takes place, and what follows from it. A key element of naturalism is the demand that the social researcher should adopt an attitude of 'respect' or 'appreciation' towards the social world. In Matza's (1969: 5) words, naturalism is 'the philosophical view that remains true to the nature of the phenomenon under study'. This is contrasted with the positivists' primary and prior commitment to a conception of scientific method reconstructed from the experience of natural scientists:

Reality exists in the empirical world and not in the methods used to study that world; it is to be discovered in the examination of that world. . . . Methods are mere instruments designed to identify and analyze the obdurate character of the empirical world, and as such their value exists only in their suitability in enabling this task to be done. In this fundamental sense the procedures employed in each part of the act of scientific enquiry should and must be assessed in terms of whether they respect the explicit, standardized set of data elicitation procedures. This also allows replication by others so that an assessment of the reliability of the findings can be made. In survey research, for example, the behaviour of interviewers is typically specified down to the wording of questions and the order in which they are asked. In experiments the conduct of the experimenter is closely defined. It is argued that if it can be ensured that each survey respondent or experimental subject in a study and its replication is faced with the same set of stimuli, then their responses will be comparable. Where such explicit and standardized procedures are not employed, as in participant observation, so the argument goes, it is impossible to know how to interpret the responses once one has no idea what they are responses to. In short, positivists argue that it is only through the exercise of physical or statistical control of variables, and their rigorous measurement, that science is able to produce a body of knowledge whose validity is conclusive; and thus can justifiably replace the myths and dogma of traditional views or common sense.

Ethnography, and many kinds of qualitative research, do not match these positivist canons. As a result, especially in the middle part of the twentieth century, they came
of positivism. In the view of interactionists, people interpret stimuli, and these interpretations, continually under revision as events unfold, shape their actions. As a result, the 'same' physical stimulus can mean different things to different people—and, indeed, to the same person at different times. Many years ago, Mehan (1974) provided a striking example that relates directly to the sort of data collection method supported by positivism:

A question from a [a] language development test instructs the child to choose 'the animal that can fly' from a bird, an elephant, and a dog. The correct answer (obviously) is the bird. Many first grade children, though, chose the elephant along with the bird as a response to that question. When I later asked them why they chose that answer they replied: 'That's Dumbo'. Dumbo (of course) is Walt Disney's flying elephant, well known to children who watch television and read children's books as an animal that flies. (Mehan 1974: 249)

Such indeterminacy of interpretation undermines attempts to develop standard measures of human behaviour. Interpretations of the same set of experimental instructions or interview questions will undoubtedly vary among people and across occasions; and, it is argued, this undermines the value of standardized research methods. Equally important, naturalists argue that because people's behaviour is not caused in a mechanical way, it is not amenable to the sort of causal analysis and manipulation of variables that are characteristic of the quantitative research inspired by positivism. Any hope of discovering laws of human behaviour is misplaced, it is suggested, since human behaviour is continually constructed, and reconstructed, on the basis of people's interpretations of the situations they are in.

According to naturalism, in order to understand people's behaviour we must use an approach that gives us access to the meanings that guide their behaviour. Fortunately, the capacities we have developed as social actors can give us such access. As participant observers we can learn the culture or subculture of the people we are studying. We can come to interpret the world more or less in the same way that they do. In short, we not only can but also must learn to understand people's behaviour in a different way from that in which natural scientists set about understanding the behaviour of physical phenomena.

The need to learn the culture of those we are studying is most obvious in the case of societies other than our own. Here, not only may we not know why people do what they do, but often we may not be able to recognize even what they are doing. We are in much the same position as Schütz's (1964) stranger: Schütz notes how, in the weeks and months following an immigrant's arrival in a host society, what he or she previously took for granted as knowledge about that society turns out to be unreliable, if not obviously false. In addition, areas of ignorance previously of no importance come to take on great significance; and overcoming them is necessary for the pursuit of important goals, perhaps even for the stranger's very survival in the new environment. In the process of learning how to participate in the host society, the stranger gradually acquires an inside knowledge of it, which supplements his or her previous 'external' knowledge. But Schütz argues that by virtue of being forced to come to understand a culture in this way, the stranger acquires a certain objectivity not normally available to culture members. The latter live inside the culture, and tend to see it as simply a reflection of 'how the world is'. They are often not conscious of the fundamental presuppositions that shape their vision, many of which are distinctive to their own culture.

Schütz's (1964) account of the experience of the stranger matches most obviously the work of anthropologists, who typically study societies very different from their own. However, the experience of the stranger is not restricted to those moving to live in another society. Movement among groups within a single society can produce the same effects; generally, though not always, in a milder form. There are many different layers or circles of cultural knowledge within any society. Indeed, this is particularly true of modern industrial societies with their complex divisions of labour, multifarious lifestyles, ethnic diversity, and deviant communities; and the subcultures and perspectives that maintain, and are generated by, these social divisions. This was, of course, one of the major rationales for the research of the Chicago School sociologists. Drawing on the analogy of plant and animal ecology, they set out to document the very different patterns of life to be found in different parts of the city of Chicago, from the 'high society' of the so-called 'gold coast' to slum ghettos such as Little Sicily. Later, the same kind of approach came to be applied to the cultures of occupations, organizations, and social groups of various kinds.

According to the naturalist account, the value of ethnography as a social research method is founded upon the existence of such variations in cultural patterns across and within societies, and their significance for understanding social processes. Ethnography exploits the capacity that any social actor possesses for learning new cultures, and the objectivity to which this process gives rise. Even where he or she is researching a familiar group or setting, the participant observer is required to treat this as 'anthropologically strange', in an effort to make explicit the presuppositions he or she takes for granted as a culture member. In this way, the culture can be turned into an object available for study. Naturalism proposes that through marginality, in social position and in perspective, it is possible to construct an account of the culture under investigation that both understands it from within and captures it as external to, and independent of, the researcher: in other words, as a natural phenomenon. Thus, the description of cultures becomes the primary goal. The search for universal laws is downplayed in favour of detailed accounts of the concrete experience of life within a particular culture and of the beliefs and social rules that are used as resources within it. Indeed, attempts to go beyond this, for instance to explain particular cultural forms, are sometimes discouraged. Certainly, as Denzin (1971: 168) noted, 'the naturalist resists schemes or models which over-simplify the complexity of everyday life'; though some forms of theory, especially those which are believed to be capable of capturing social complexity, are often recommended, most notably the kind of grounded theory proposed by Glaser and Strauss. 10

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7 For useful accounts of interactionism, see Mair (2001), Atkinson and Housley (2003) and Reynolds and Herman-Kinney (2003).
8 Cooper and Dunne (2000) provide a similar and more developed analysis of the processes of interpretation involved in mathematical tests.
9 This form of understanding social phenomena is often referred to as Verstehen. See Tröper (1974) for a discussion and illustrations of the history of this concept, and O’Hea (1996) for a more recent discussion of its role across the social sciences and humanities.
10 See Glaser and Strauss (1968); Strauss and Corbin (1998); Pidgeon and Henwood (2004); for critical commentaries, see Williams (1976) and Dey (1999).
What is ethnography?

Over the last decades of the twentieth century, the influence of positivism waned and with it, in many areas, the dominance of quantitative methods; though there are currently some signs of a revival. At the same time, various aspects of naturalism came under attack from within the ranks of qualitative researchers. In the next section we shall explore the ideas that stimulated this.

Anti-realist and political critiques of naturalism

The field of social research methodology nowadays is a complex one. There has been considerable diversification in qualitative research, including the rise of discourse and narrative analysis, of various kinds of action research, of autoethnography and performance studies, etc. So, at the same time, there have been growing calls to combine qualitative methods with quantitative techniques. These have often been met with charges that this neglects the conflicting philosophical and political presuppositions built into qualitative and quantitative approaches (Smith and Heshusius 1986; Smith 1989; Guba 1990; Hodkinson 2004). Along with this, there has been criticism of older forms of ethnographic work on the grounds that these still betray the influence of positivism and scientism. What is pointed to here is that, despite their differences, positivism and naturalism share much in common. They each appeal to the model of natural science, albeit interpreting it in different ways. As a result, both are committed to trying to understand social phenomena as objects existing independently of the researcher. And they therefore claim that research can provide knowledge of the social world that is superior in validity to that of the people being studied. Equally important, they both regard practical and political commitments on the part of the researcher as, for the most part, extraneous to the research process – indeed, as a source of potential distortion whose effects have to be guarded against to preserve objectivity.

Many ethnographers have begun to question the commitment to naturalism, challenging these assumptions. Doubts have been raised about the capacity of ethnography to portray the social world in the way that naturalism claims it does. Equally, the commitment of the older kinds of ethnography to some sort of value neutrality has been questioned, and politically interventionist forms of ethnography have been recommended. We shall look at these two aspects of the critique of naturalism separately, though they are sometimes closely related.

Questioning realism

Many critics of naturalism today reject it on the grounds that, like positivism, it assumes that the task of social research is to represent social phenomena in some literal fashion: to document their features and explain their occurrence. What is being questioned here is sometimes referred to as realism. In part, criticism of realism stems from a tension within ethnography between the naturalism characteristic of ethnographers’ methodological thinking and the constructionism and cultural relativism that shape their understanding of the perspectives and behaviour of the people they study (Hammersley 1992: ch. 3). As we saw, ethnographers portray people as constructing

the social world, both through their interpretations of it and through actions based on those interpretations. Furthermore, those interpretations sometimes reflect different cultures, so that there is a sense in which through their actions people create distinct social worlds (Blumer 1969: 11). But this constructionism and relativism is compatible with naturalism only so long as it is not applied to ethnographic research itself. Once we come to see ethnographers as themselves constructing the social world through their interpretations of it, thereby producing incommensurable accounts that reflect differences in their background cultures, there is a conflict with the naturalistic realism built into older ethnographic approaches.

This internal source of doubts about realism was reinforced by the impact of various external developments. One was changes in the field of the philosophy of science. Whereas until the early 1950s positivism had dominated this field, at that time its dominance began to be undermined, eventually producing a range of alternative positions, some of which rejected realism. A sign of this change was the enormous influence of Thomas Kuhn’s book The Structure of Scientific Revolutions (Kuhn 1996; first published in 1962). Kuhn argued against views of the history of science that portray it as a process of cumulative development towards the truth, achieved by rational investigation logically founded on evidence. He, and others, showed that the work of those involved in the major developments of scientific knowledge in the past was shaped by theoretical presuppositions about the world that were not themselves based on empirical research, and many of which are judged by scientists today as false. Kuhn further claimed that the history of science, rather than displaying the gradual build-up of knowledge, is punctuated by periods of revolution when the theoretical presuppositions forming the ‘paradigm’ in terms of which scientists in a particular field have previously operated are challenged and replaced. An example is the shift from Newtonian physics to quantum mechanics in the early part of the twentieth century. The replacement of one paradigm by another, according to Kuhn, does not, because it cannot, occur on the basis simply of the rational assessment of evidence. Paradigms are incommensurable, they picture the world in incompatible ways, so that the data themselves are interpreted differently by those working within different paradigms. This implies that judgements of the validity of scientific claims is always relative to the paradigms within which they operate are judged; they are never simply a reflection of some independent domain of reality.

Kuhn’s work embodied most of the arguments against positivism that had become influential: that there is no theory-neutral observational foundation against which theories can be tested, and that judgements about the validity of theories are never fully determined by any evidence. He also proposed an alternative conception of science that contrasted sharply with the positivist model. However, his critique counted as much against naturalism, against the idea of the researcher getting into direct contact with reality, as it did against positivism. On his account, all knowledge of the world is mediated by paradigmatic presuppositions. Furthermore, the alternative view he offered made natural scientists look very similar to the people that ethnographers had long portrayed in their accounts as constructing diverse social worlds. And sociologists of science have subsequently produced ethnographies of the work of natural scientists and technological innovators along these lines (see Hess 2001). In this way, natural


12 Some have argued that mixed methods research can be a new paradigm that transcends the distinction between the other two: see, for example, Tashakkori and Teddlie (2000).

13 There is some ambiguity in Kuhn’s work, and this has led to disputes about its interpretation. For a detailed discussion see Sharrock and Read (2002).
science moved from being primarily a methodological model for social research to being an object of sociological investigation; and in many ways this brought the conflict between naturalism and constructionism to a head. As important as developments within the philosophy of science for the generation of doubts about realism was the influence of various continental European philosophical trends. Naturalism had been influenced by nineteenth-century ideas about hermeneutics, about the interpretation of historical texts, notably the work of Dilthey (see Makkreel 1975). This was the source of the idea, mentioned earlier, that socio-cultural understanding takes a different form from how natural scientists go about understanding physical phenomena. In the twentieth century, however, this earlier hermeneutic tradition came to be challenged by a new form of ‘philosophical hermeneutics’, developed by Gadamer (see Howard 1982; Warnke 1987; Dostal 2002). Where, previously, understanding human texts had been presented as a rigorous task of recovering the meaning intended by the author and locating it within relevant cultural settings, philosophical hermeneutics viewed the process of understanding as inevitably reflecting the ‘prejudices’, the pre-understandings, of the interpreter. Interpretation of texts, and by extension understanding of the social world too, could no longer be seen as a matter of capturing social meanings in their own terms; the accounts produced were regarded as constructions that inevitably reflected the socio-historical position and background assumptions of the researcher.

Another powerful influence on ethnography has been post-structuralism and postmodernism. These labels refer to a diverse set of ideas and work, but we shall mention just two of the most influential figures: Derrida’s ‘deconstruction’ and the work of Foucault. Like philosophical hermeneutics, deconstruction has also led to a questioning of the idea that ethnographers can capture the meanings on the basis of which people act. It does this because it argues that meanings are not stable; nor are they properties of individuals. Rather, they reflect the shifting constitutive role of language. Also important has been deconstruction’s undermining of the distinctions between different genres of writing: its advocates have sought to erase the differentiation between fiction and non-fiction, indeed between literary and technical writing generally. This has led to a recognition of the fact that the language used by ethnographers in their writing is not a transparent medium allowing us to see reality through it, but rather a construction that draws on many of the rhetorical strategies used by journalists, travel writers, novelists, and others. Some commentators have drawn the conclusion from this that the phenomena described in ethnographic accounts are created in and through the rhetorical strategies employed, rather than being external to the text; in short, this concern with rhetoric has often been associated with forms of anti-realism.15

Foucault’s work is also based on a rejection of realism: he is not concerned with the truth or falsity of the ideas that he studies – for example about madness or sex – but rather with the ‘regimes of truth’ by which they are constituted and how they have structured institutional practices during the development of Western society.16

He stresses the fact that the psychological and social sciences are socio-historical in character, and claims that they function as part of the process of surveillance and control, which he sees as the central feature of modern society. Their products reflect this social character, rather than representing some world that is independent of them. Foucault argues that different regimes of truth are established in different contexts, reflecting the play of diverse sources of power and resistance. Thus, what is treated as true and false, in social science as elsewhere, is constituted through the exercise of power.17

The reception of post-structuralist and postmodernist ideas in the context of Anglo-American qualitative research has involved diverse readings and responses to what was, of course, by no means a coherent set of texts; these extending well beyond those of Derrida and Foucault. Typically, these readings and responses have reinforced tendencies towards anti-realism of some kind, encouraged the adoption of non-Marxist Leftist political orientations, and involved the idea that some discourses/voices are suppressed and that the function of research should be to liberate them. Much less commonly, this influence has also led to the subversion of conventional ethnographic textual strategies.

While realism has not been completely abandoned by most ethnographers, the idea that ethnographic accounts can represent social reality in a relatively straightforward way (for example, through the ethnographer getting close to it) has been widely rejected; and doubt has been thrown on the claims to scientific authority associated with realism. Moreover, in the work of Foucault especially, we have a direct link with the second criticism of naturalism: its neglect of the politics of social research.

The politics of ethnography

Naturalists shared with positivists a commitment to producing accounts of factual matters that reflect the nature of the phenomena studied rather than the values or political commitments of the researcher. Of course, both recognized that, in practice, research is affected by the researcher’s values, but the aim was to limit the influence of those values as far as possible, so as to produce findings that were true independently of any particular value stance. Since the mid-1980s, any such striving after value neutrality and objectivity has been questioned, sometimes being replaced by advocacy of ‘openly ideological’ research (Lather 1986), ‘militant anthropology’ (Scheper-Hughes 1995), or research that is explicitly carried out from the standpoint of a particular group, for example women, those suffering racism, indigenous peoples, or people with disabilities (see Denzin and Lincoln 2005).

In part this has resulted from the continuing influence of Marxism and ‘critical’ theory, but equally important has been the impact of feminism and of post-structuralism. From a traditional Marxist point of view the very distinction between facts and values is a historical product, and one that can be overcome through the future development of society. Values refer to the human potential that is built into the unfolding of history. In this sense values are facts, even though they may not yet have been realized in the social world. Moreover, they provide the key to any understanding of the nature of current social conditions, their past, and their future. From this point of view, a science

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14 For an excellent account of the rise of these ideas in the context of French philosophy, see Gutting (2001).

15 See, for example, Tyler (1986), Ashmore (1989); Piper and Strouach (2004).

16 The statement that Foucault rejects realism, while not fundamentally misleading, does obscure both the, probably writing, ambiguities in his work in this respect, and its emergence out of the tradition of rationalist epistemology; see Gutting (1989). On Foucault more generally, see Gutting (1994).

17 For discussions of the implications of Foucault’s work for ethnography, see Gubrium and Silverman (1989); Kendall and Wickham (2004).
of society should provide not only abstract knowledge but also the basis for action to transform the world so as to bring about human self-realization. On this argument, ethnography, like other forms of social research, cannot but be concerned simultaneously with factual and value matters, and its role inevitably involves political intervention (whether researchers are aware of this or not).

A similar conclusion about the political character of social research has been reached in other ways, for example by those who argue that because research is always affected by values, and always has political consequences, researchers must take responsibility for their value commitments and for the effects of their work. It has been suggested that ethnography and other forms of social research have had too little impact, that their products simply lie on library shelves gathering dust, and that as a result they are worthless. To be of value, it is suggested, ethnographic research should be concerned not simply with understanding the world but with applying its findings to bring about change (see, for example, Gewirtz and Cribb 2006).

There are differences in view about the nature of the change that should be aimed at. Sometimes the concern is with rendering research more relevant to national policy-making or to one or another form of professional practice (see, for example, Hustler et al. 1986; Hart and Bond 1995; Healy 2001; Taylor et al. 2000). Alternatively, or as part of this, it may be argued that research should be emancipatory. This has been proposed by feminists, where the goal is the emancipation of women (and men) from patriarchy (Fonow and Cook 1991; Lather 1991; Olesen 2005); but it is also to be found in the writings of critical ethnographers and advocates of emancipatory action research, where the goal of research is taken to be the transformation of Western societies so as to realize the ideals of freedom, equality, and justice (Gitlin et al. 1989; Kendall and McTaggart 2005). Simultaneously, problems have occurred in the field of disability studies (Barnes 2003) and in the context of queer theory (Plummer 2005).

Of course, to the extent that the very possibility of producing knowledge is undermined by the sort of anti-realist arguments we outlined earlier, a concern with the practical or political effects of research may come to seem an essential alternative goal to the traditional concern with truth. This too has led to the growth of more interventionist conceptions of ethnography. In this way post-structuralism and postmodernism have contributed to the politicization of social research, though in a far from unambiguous way because they seem simultaneously to undermine all political ideals (Dews 1987). For example, they threaten any appeal to the interests or rights of Humanity; and in the context of feminist research they challenge the concept of woman.

**Reflexivity**

The criticisms of naturalism we have outlined are sometimes seen as arising from what has been called the reflexive character of social research.18 It is argued that what both positivism and naturalism fail to take into account is the fact that social researchers are part of the social world they study. A sharp distinction between science and common

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18 'Reflexivity' is a term that has come to be used in a variety of different ways, and the meaning we are giving to it here is by no means uncontested, see Lynch (2000). For discussions of some of the problems with reflexivity, see Troyna (1994); Puencur (1996); Adkins (2002); Finlay (2002); Hecoy (2002).

19 For an influential epistemological analysis that recognizes the fallible character of any evidence but retains a commitment to realism, see Hauk (1993). See also Hammersley (2004).
is no way in which we can escape the social world in order to study it. Fortunately, though, this is not necessary from a realist point of view. There is as little justification for rejecting all common-sense knowledge out of hand as there is for treating it as all ‘valid in its own terms’: we have no external, absolutely conclusive standard by which to judge it. But we can work with what we currently take to be knowledge, while recognizing that it may be erroneous; and engaging in systematic inquiry where doubt seems justified. And in doing this we can still make the reasonable assumption that we are able to describe phenomena as they are, and not merely how we perceive them or how we would like them to be (Hammersley 1992: ch. 3). All of us, in our everyday activities, rely on presuppositions about the world, few of which we have subjected to test ourselves, and none of which we could fully and independently test. Most of the time this does not and should not trouble us, and social research is no different from other activities in this respect. We need to reflect only on what seems – or can be shown to be – problematic, while leaving open the possibility that what currently is not problematic may in the future become so.

It is also important to recognize that research is an active process, in which accounts of the world are produced through selective observation and theoretical interpretation of what is seen, through asking particular questions and interpreting what is said in reply, through writing fieldnotes and transcribing audio- and video-recordings, as well as through writing research reports. And it is true that some aspects of this process have not been given the attention they deserve until recently. However, to say that our findings, and even our data, are constructed does not automatically imply that they do not or cannot represent social phenomena. To believe that this is implied is to assume that the only true form of representation would involve the world imprinting its characteristics on our senses without any activity on our part, a highly implausible account even of the process of perception (Gregory 1970).

Similarly, the fact that as researchers we are likely to have an effect on the people we study does not mean that the validity of our findings is restricted to the data elicitation situations on which we relied. We can minimize reactivity and/or monitor it. But we can also exploit it: how people respond to the presence of the researcher may be as informative as how they react to other situations. Indeed, rather than engaging in futile attempts to eliminate the effects of the researcher completely, we should set about understanding them, a point that Schuman (1982) made in relation to social surveys:

The basic position I will take is simple: artifacts are in the mind of the beholder. Barring one or two exceptions, the problems that occur in surveys are opportunities for understanding once we take them seriously as facts of life. Let us distinguish here between the simple survey and the scientific survey. ... The simple approach to survey research takes responses literally, ignores interviewers as sources of influence, and treats sampling as unproblematic. A person who proceeds in this way is quite likely to trip and fall right on his artifact. The scientific survey, on the other hand, treats survey research as a search for meaning, and ambiguities of language and of interviewing, discrepancies between attitude and behaviour, even problems of non-response, provide an important part of the data, rather than being ignored or simply regarded as obstacles to efficient research. (Schuman 1982: 23)

In short, ‘what is an artifact if treated naïvely reflects a fact of life if taken seriously’ (Schuman 1982: 24). In order to understand the effects of the research and of research procedures, we need to compare data in which the level and direction of reactivity vary. Once we abandon the idea that the social character of research can be standardized out or avoided by becoming a ‘fly on the wall’ or a ‘full participant’, the role of the researcher as active participant in the research process becomes clear. As has long been recognized by ethnographers, he or she is the research instrument par excellence. The fact that behaviour and attitudes are often not stable across contexts and that the researcher may influence the context becomes central to the analysis. Indeed, it can be exploited for all it is worth. Data should not be taken at face value, but treated as a field of inferences in which hypothetical patterns can be identified and their validity tested. Different research strategies can be explored and their effects compared with a view to drawing theoretical conclusions. Interpretations need to be made explicit and full advantage should be taken of any opportunities to test their limits and to assess alternatives. Such a view contrasts sharply with the image of social research projected by naturalism, though it is closer to some other models of ethnographic research such as ‘grounded theorizing’, ‘analytic induction’, and the strategy model to be found alongside naturalism in the work of Schatzman and Strauss (1973). And in this way the image of the researcher is brought into parallel with that of the people studied, as actively making sense of the world, yet without undermining the commitment of research to realism.

Reflectivity and the political character of research

Positivism and naturalism, in the forms we have discussed them, tend to present research as an activity that is done for its own sake and in its own terms. By contrast, as we have seen, some critics insist that research has a social function, for instance serving to legitimize and preserve the status quo. And on this basis they argue that researchers must by and large try to make their research serve a different function, such as challenging the status quo, in some respect. Often, this point of view is organized around the question: whose side is the researcher on? (Becker 1967b; Troyia and Carrington 1989; but see Hammersley 2000: ch. 3).

As we saw earlier, others argue that what is wrong with ethnography is its lack of impact on policy-making and practice, its limited payoff in the everyday worlds of politics and work. Here it is dismissed as an idle pastime, a case of fiddling while the world burns; one that is engaged in by intellectual dilettantes who live off the taxes paid by hard-working citizens.

These criticisms of naturalist ethnography seem to us to involve an overestimation of the actual and potential contribution of research to policy and practice, and an associated failure to value the more modest contributions it offers (Rule 1978; Hammersley 2002). It is also worth pointing out that one may believe that the only justification for research is its contribution to policy and practice, and recognize that it inevitably has effects on these, without concluding that it should be directed towards the achievement of particular political or practical goals. Indeed, there are good reasons for research not being directed towards such goals. The most important one is that this would increase the chances of the findings being distorted by ideas about how the world ought to be, or by what it would be politic for others to believe. When we are
engaged in political or practical action, the truth of what we say is not always our principal concern, even though we may prefer to be honest. We are more interested in the practical effects of our actions, and sometimes this may lead us to be ‘economical’ with the truth, at the very least; perhaps even in relation to ourselves (Benson and Stangroom 2006: ch. 1). Moreover, even where the truth of our beliefs is the main issue, in practical activities judgement of factual and value claims as more or less reliable will be based on somewhat different considerations than in research directed towards producing knowledge: we will probably be concerned above all with whether the information is sufficiently reliable for our current purposes. Of course, if one believes, as Marx and others did and do, that (ultimately at least) the true and the good are identical, one might deny the significance of this difference in orientation between research and other practical activities. But this view relies on an elaborate and unconvincing philosophical infrastructure (Hammersley 1992: ch. 6, 1993).

It is worth emphasizing that to deny that research should be directed towards political goals is not to suggest that researchers could, or should, abandon their political convictions. It is to insist that as researchers their primary goal must always be to produce knowledge, and that they should try to minimize any distortion of their findings by their political convictions or practical interests. Nor are we suggesting that researchers should be unconcerned about the effects of their work on the world. The point is that acknowledging the reflexivity of research does not imply that it must be primarily directed towards changing (or for that matter preserving) the world in some way or other. And, as we have indicated, there are good reasons why it should not be so directed.

Conclusion

We began this chapter by examining two contrasting accounts of the logic of social research and their implications for ethnography. Neither positivism nor naturalism provides an adequate framework. Both neglect its fundamental reflexivity: the fact that we are part of the social world we study, and that there is no escape from reliance on common-sense knowledge and methods of investigation. All social research is founded on the human capacity for participant observation. We act in the social world and yet are able to reflect upon ourselves and our actions as objects in that world. However, rather than leading to doubts about whether social research can produce knowledge, or to the desire to transform it into a political enterprise, for us this reflexivity provides the basis for a reconstructed logic of inquiry that shares much with positivism and naturalism but goes beyond them in important respects. By including our own role within the research focus, and perhaps even systematically exploiting our participation in the settings under study as researchers, we can produce accounts of the social world and justify them without placing reliance on futile appeals to empiricism, of either positivist or naturalist varieties.

Reconstructing our understanding of social research in line with the implications of its reflexivity also throws light on the relationship between quantitative and qualitative approaches. Certainly there is little justification for the view, associated with naturalism, that ethnography represents a superior, alternative paradigm to quantitative research. On the other hand, it has a much more powerful contribution to make to social science than positivism allows. And, while combining different methods, for particular purposes,

may often be of value, this should not be done at the expense of forgetting the important methodological ideas associated with ethnography, and with qualitative research more generally.

Reflexivity is an aspect of all social research. It is one that has been given increasing attention by ethnographers and others in recent years, notably in the production of ‘natural histories’ of particular studies.20 The remainder of this book is devoted to spelling out what we take to be the implications of reflexivity for ethnographic practice.

20 For a listing of examples of natural histories of social research, see Hammersley (2003b).
Research design: problems, cases and samples

necessary data will need to be developed. Similarly, not all the people we wish to observe or talk to, nor all the contexts we wish to sample, may be accessible – certainly not at the times we want them to be. The problem of gaining access to data is particularly serious in ethnography, since one is operating in settings where the researcher generally has little power, and people have pressing concerns of their own which often give them little reason to cooperate. It is to this problem that we turn in the next chapter.

3 Access

The problem of obtaining access to the data looms large in ethnography; and Feldman et al. (2003: vii) suggest that it often comes ‘as a rude surprise’ to researchers who have not anticipated the difficulties that could be involved. It is often at its most acute in initial negotiations to enter a setting and during the ‘first days in the field’; but the problem, and the issues associated with it, persist, to one degree or another, throughout the data collection process. For example, Sampson and Thomas (2003) found that, in gaining access to carry out fieldwork on board ship, obtaining permission from the owners was only the very first step: the captain was an even more important gatekeeper; and, despite the sharply hierarchical character of ship life, even his support was far from sufficient. They comment that ‘negotiating access is something of a full-time occupation in a shipboard context’ (Sampson and Thomas 2003: 173). To one degree or another, this is true of most settings.

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

Thus, in negotiating access for a study of public commemoration of a terror attack at the Bologna railway station, Tota (2004) found that key figures in the Bolognese Victims’ Association suspected her of being an infiltrator on behalf of the Italian secret service (Tota 2004: 134). And in trying to contact victims of punishment beatings in Northern Ireland, Knox (2001) discovered that doing this through community organizations had limited success because these organizations were suspected by victims of having links with the paramilitary groups that were responsible for their treatment. Instead, it turned out that a more productive route was through probation officers, whose cases included young people who had been punished for ‘anti-social’ behaviour by the paramilitaries (Knox 2001: 209).

The work of Barbera-Stein (1979) also illustrates how negotiating access can generate important knowledge about the field. She sought to investigate several different therapeutic or daycare centres for preschool children, but the original research design

1 Feldman et al.’s (2003) book provides a general discussion of access problems, and a collection of accounts from particular projects, focused mainly on gaining access to individuals rather than to institutions.
Access

...founded because access was denied to several settings. She writes in retrospect of her experience: 'The access negotiations can be construed as involving multiple views of what is profane and open to investigation vs what is sacred or taboo and closed to investigation unless the appropriate respectful stance or distance is assumed' (Barbera-Stein 1979: 15). She ties this observation to particular settings and particular activities in them:

'I had requested the permission to observe what the psychoanalytic staff considered sacred. In their interactions with emotionally disturbed children, they attempted to establish effective bonds modelled after the parent-child bond. This was the first step in their attempts to correct the child's faulty emotional development. This also was the principal work of the social workers at the day-care centre. Formal access to the day-care centre initially was made contingent upon my not observing on Tuesdays and Thursdays when the social workers engaged the children in puppet play sessions. Puppet play was used as a psychological projective technique in monitoring and fostering the emotional development of the children.'

(Barbera-Stein 1979: 15)

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

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

Negotiating access also involves ethical considerations, for example to do with whose permission ought to be asked, as well as whose needs to be obtained if initial access is to be granted. This issue arises most obviously in relation to those who occupy subordinate positions within the settings investigated, for example children or prisoners. But it often applies to all members within many organizations.

Equally important is what people are told about the research in the process of negotiating access, both as regards its purpose and what it will involve for them, including possible consequences stemming from the publication of findings. As we shall see, fully informed consent is often neither possible nor desirable in ethnographic (or, for that matter, other) research. However, this does not mean that issues of consent and the provision of information are unimportant.

Dealing with these issues is increasingly complex in practical terms today, not least because very often permission has to be sought from relevant ethics committees or institutional review boards (one or more) before the research can go ahead. Informed consent is one of the key guiding principles on which such committees operate; and, at the present time at least, their deliberations are typically based on a biomedical, psychological, or survey research model; in which the researcher is presumed to be dealing with individual cases. Since ethnographers frequently study situations and groups, many of the guidelines on which ethics committees operate, such as opt-in consent and the right of withdrawal at any time, are inapplicable. These problems can often be overcome, but usually this is not without considerable, time-consuming negotiation; and it may involve ethnographic work being subjected to a greater degree of surveillance than other kinds of research – for example being subjected to continual review (Economic and Social Research Council (ESRC) 2005: section 2.2, pp. 22–3).

It may also lead, sometimes, to research plans being altered simply to ensure smooth passage through the ethics committee procedures.

The rights and wrongs of ethical committees, in principle and in relation to their particular current orientation, are the subject of considerable, and continuing, dispute, but the task of dealing with them is a necessity for many researchers today. This arises both because universities increasingly police research done by their employees, and because the requirements associated with gaining access for research purposes to some settings or informants (particularly in the health field) demand compliance with local ethics committee procedures.3

Approaching the field

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

In many settings, while physical presence is not in itself problematic, appropriate activity may be so.

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

Some examples of such problems are provided in Karp's (1980) account of his investigation of the 'public sexual scene' in and around Times Square in New York, particularly in pornographic bookshops and cinemas. Admittedly, this is a very particular sort of public setting, in that a good deal of what goes on may be 'disreputable' and the behaviour in public correspondingly guarded. Karp tried various strategies for achieving access and initiating interaction. He tried to negotiate openly with some bookshop managers, but failed. Similarly, after a while, regulars on the street interpreted...

2 The complexity of the ethical issues surrounding ethnographic research will be addressed in Chapter 10.

3 For arguments about current ethical regulation regimes, see Lincoln (2005) and Hammersley (2006a).
his hanging around in terms of his being a hustler, or a cop. He also reports failure to
establish relationships with prostitutes, although his fieldnotes display what seems a
rather clumsy and naive approach to this.
Karp (1980) resolved his problems to some extent by realizing that they directly
paralleled the interactional concerns of the participants themselves, and he was able
to draw on his access troubles for analytical purposes in that light. He quotes a research
note to this effect:

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

(Karp 1980: 94)

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

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

Anderson (2006) reports a rather more protracted process in gaining access to Jelly’s
bar in the Southside of Chicago, and illustrates the difference between being in a place
and having access to the social relations that take place there. He provides a fieldnote
from his first visit:

As I entered, sat down at the bar, and ordered my first drink, I drew the attention,
direct and indirect, of most of the other patrons. Their eyes followed me, they
lowered their voices and stopped interacting so freely with one another, as they
listened attentively to what I was saying to the barmaid, even observing the way
I said it. They desired information about me, and I gave it, however unwillingly,
through my interactions with them. … As the barmaid brought my beer, I promptly
paid her, just as the sign (‘Please pay when served’) on the large mirror in front
of me directed. She eagerly accepted my payment. As a stranger, I had to pay

when served, while the people around me, presumably regular customers, received
round after round on credit. It was clear that the rule was for outsiders like me.

(Anderson 2006: 40)

So, individuals and groups whom one might want to study may be available in
public settings, but they are not always welcoming to researchers, or indeed to outsiders
of any kind. Sometimes very extensive ‘hanging about’, along with lucky breaks, is
necessary before access is achieved, as Wolf’s (1991) experience illustrates:

As a new graduate student in anthropology at the University of Alberta, Edmonton,
I wanted to study the ‘Harley tribe’. It was my intent to obtain an insider’s perspective
of the emotions and the mechanics that underlie outlaw bikers’ creation of a
subcultural alternative … I customised my Norton, bought some biker clothing,
and set off to do some fieldwork. My first attempts at contacting an outlaw club
were near-disasters. In Calgary I met several members of the Kings Crew MC in
a motorcycle shop and expressed an interest in ‘hanging around’. But I lacked
patience and pushed the situation by asking too many questions. I found out
quickly that outsiders, even bikers, do not rush into a club, and that anyone who
doesn’t show the proper restraint will be shut out.

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

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

(Wolf 1991: 212–15)

Making contact in public settings with people one wishes to study can be a difficult
and protracted process, then; though Wolf’s experience is undoubtedly extreme. Also
sometimes involved is a testing out of the researcher to see whether he or she is genuine and can be trusted, and perhaps also whether being researched will be interesting or boring. Ryan (2006) provides an example from a life history study of Irish gay men:

Darren . . . would not discuss the prospect of a series of interviews until we both had a number of drinks together. I recorded the incident afterwards in a journal:

I talked briefly again about the research but he wondered if I had seen Channel Four’s ‘Queer as Folk on Tuesday? It had made him horny and he wondered if it had made me horny? I said I thought it was daring. I’m embarrassed and my face has reddened. I again start to talk about the research but he interrupts to ask what my longest relationship was? Where did I go to school and what age was I when I had my first sexual encounter?’ I said it was one of those drunken things but this was in fact a lie, the first one that I told . . . I considered that he was trying to ‘test’ me with more and more sexually explicit conversation to gauge my reaction and I was determined to see it through to the end. He tells me that I’m a very open person, an honest man that he’d have no difficulty in talking with. If it was a test, I appear to have passed.

(Ryan 2006: 155–6)

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

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

(Liebow 1967: 238)

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

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

(Liebow 1967: 236–7)

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

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

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

Liebow’s and Whyte’s contacts with their sponsors were quite fortuitous. However, sponsorship of a similar kind may be gained through the mobilization of existing social networks, based on acquaintanceship, kinship, occupational membership, and so on. This is not always straightforward, however. Cassell (1988) reports the difficulties she had in negotiating access in a study of surgeons, and her reliance on personal and occupational networks:

When I decided to study surgeons, I negotiated for the better part of a year with a representative of the Department of Surgery, at a hospital where my ex-husband was an attending physician, before the Chief of Surgery definitely refused to allow me access to his department.

At the same time, after spending six months obtaining an interview with a representative of the American College of Surgeons, I flew to Chicago to ask for advice and possible sponsorship from this prestigious group. After a charming Southern surgeon, in his sixties, indulged in an hour of small talk, I broke in and asked if he thought my study was worth doing. Silence. ‘Your husband is a doctor?’ he finally inquired. When I assented, he said: ‘Have you ever thought of . . . I mean, with your background, you’d be such an asset . . . has it ever occurred to you to become active in the Ladies Auxiliary of your husband’s hospital?’ This was the only advice I received.

Eventually, at almost the last minute, when a reviewer for the agency that eventually funded my study asked for proof that I had access to surgeons, a friend of my ex-husband said that I could do research in the hospital where he was Chief of Surgery (and wrote a letter to that effect).

(Cassell 1988: 94)
Hoffman (1980) also provides insight into the way in which personal networks can be used, while drawing attention once more to the relationship between problems of access and the quality of the data subsequently collected. Her research was concerned with a locally influential elite: members of boards of hospital directors in Quebec. In the first place she notes a general problem of access to such an elite:

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

(Hoffman 1980: 46)

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

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

(Hoffman 1980: 46–7)

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

Hoffman graphically juxtaposes typical responses to illustrate the point:

Response to an Unknown Sociologist
Board Member A

Q. How do you feel in general about how
the board has been organized?
I think the basic idea of participation is good. We need better communication with the various groups. And I think they probably have a lot to offer.

Response to a Known Individual
Board Member B

Q. How do you feel in general about how the board has been organized?
This whole business is unworkable. It’s all very nice and well to have these people on the board, they might be able to tell us something here and there, or describe a situation, but you’re not going to run a hospital on that!

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

In this context it is perhaps worth acknowledging that sometimes the problems can be such that the attempt to gain access to a particular setting, or to a particular informant or group of informants, may fail completely. For example, Ryan reports from his study of Irish gay men how his attempt to engage with one informant failed both because the latter was suspicious about the sort of story the research would tell, and because of personal dislike on both sides (Ryan 2006: 157).

Gatekeepers

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

It should be said, though, that identifying the relevant gatekeepers is not always straightforward. Indeed, the distinction between sponsors and gatekeepers is by no means clear-cut. Even in formal bureaucratic organizations it is not always obvious whose permission needs to be obtained, or whose good offices it might be advisable to secure. Much the same is true in studying local communities. In his study of violence in Northern Ireland, Knox (2001) recognized that his research ‘required the imprimatur
of paramilitaries, or, at the very least, making them aware that fieldwork of this nature was being undertaken and its purpose. He comments that ‘their approval’ was secured by contacts with key political representatives in both communities [republican and loyalist], ostensibly to “keep them informed” of our work, in reality it amounted to securing their unofficial endorsement’ (Knox 2001: 212).

There are sometimes several possible routes by which access might be achieved, and some judgement needs to be made about the viability, advantages and disadvantages of each. Sanders (2006: 203) discusses the various means by which researchers concerned with prostitution have sought to overcome the "problem of locating and contacting sellers, buyers and organizers, who are cleverly hidden in the urban landscape". These range from making contact with those who have been imprisoned, gaining an 'assisted passage' via the police or specialist health and welfare services, or capitalizing on personal contacts.

Equally, there will sometimes be a need for negotiation with multiple gatekeepers, as Gouldner (1954) notes on the basis of his research at the Oscar Center gypsum plant. He recounts that the research team made a ‘double-entry’ into the plant, coming in almost simultaneously by way of the Company and the Union. But it soon became obvious that we had made a mistake, and that the problem had not been to make a double-entry, but a triple entry; for we had left out, and failed to make independent contact with a distinct group — the management of that particular plant. In a casual way, we had assumed that main office management also spoke for the local plant management and this, as a moment's reflection might have told us, was not the case. In consequence our relations with local management were never as good as they were with the workers or the main office management.

(Gouldner 1954: 255–6)

Sampson (2004) reports a similar problem in her research aboard ship: 'my presence was not welcomed by the Captain (again despite full access being granted by the company) and this resulted in an extremely unpleasant and personally threatening experience that lasted for a period of 16 days isolated from the land' (Sampson 2004: 390).

Knowing who has the power to open up or block off access, or who consider themselves and are considered by others to have the authority to grant or refuse access, is, of course, an important aspect of sociological knowledge about the setting. However, this is not the catch-22 situation it might appear. For one thing, as we argued in Chapter 1, research never starts from scratch; it always relies on common-sense knowledge to one degree or another. We may already know sufficient about the setting to be able to judge what the most effective strategy is likely to be for gaining entry. If we do not, we may be able to 'case' the setting beforehand, for example by contacting people with knowledge of it or of other settings of a similar type. This will often solve the problem, though as Whitten (1970) found out in his research on black communities in Nova Scotia, there is no guarantee that the information provided is sound. He was told by local people that he should phone the councillor for the largest settlement, that to try to meet him without phoning would be rude. He did so, 'with disastrous results'.

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

(Whitten 1970: 371–2)

Whitten discovered that he had made two basic mistakes:

First, when Nova Scotians tell one to first call the official responsible for a community, they are paying due respect to the official, but they do not expect the investigator to take this advice. They expect that the investigator will establish an enduring contact with someone who can introduce him to the official. Crucial to this procedure is that the investigator be first known to the person who will make the introduction, for the middleman may be held responsible for the investigator’s mistakes... Second, it is not expected that one will use the term ‘Negro’ in referring to Nova Scotians ethnically identified as colored. The use of ethnic terminology (including the term ‘colored’) is reserved for those who are already a part of the system... The most effective way to approach an official, we found, is to ignore ethnic distinctions whatever, thereby forcing the official to make the preliminary distinction (e.g. between colored community and white community). By so doing the investigator is in a position to immediately inquire as to the significance of ethnicity. Had we acted a bit more slowly, and ignored ethnic differences, we might have succeeded in gaining early entry, but we erred by assuming that we knew the best way to do things in Anglo-America. By talking too much, and not reflecting carefully on the possible connotations attached to our instructions', our work bogged down for a time.

(Whitten 1970: 371–2)

Whether or not they grant entry to the setting, gatekeepers will generally, and understandably, be concerned as to the picture of the organization or community that the ethnographer will paint, and they will usually have practical interests in seeing themselves and their colleagues presented in a favourable light. At least, they will wish to safeguard what they perceive as their legitimate interests. Gatekeepers may therefore attempt to exercise some degree of surveillance and control, either by blocking off certain lines of inquiry, or by shepherding the fieldworker in one direction or another. Bogdan and Taylor (1975) provide an example of one way in which gatekeepers may try to influence things:

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

(Bogdan and Taylor 1975: 44–5)

Although Bogdan and Taylor report this as happening to a novice, it often remains a problem for even the most experienced fieldworker. In this instance, the ethnographer needs to explain that he or she is willing or even eager to sample the mundane, the routine, or perhaps the boring aspects of everyday life. But, nevertheless, the source of the problem is that it is often precisely the most sensitive things that are of most prima facie interest. Periods of change and transition, for example, may be perceived as troublesome by the participants themselves, and they may wish, therefore, to steer observers away from them: the conflict of interest arises from the fact that such disruptions can be particularly fruitful research opportunities for the fieldworker.

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

The problem is that most researchers, with limited time and money available to them, are forced to organise their classroom observations into short periods of time. This usually involves moving into already established classroom situations where teachers and pupils have considerably greater experience of their interactional encounters than does the observer. Even where the researcher is available to monitor the initial encounters between a teacher and pupils, the teacher is, not unreasonably, reluctant to be observed at this stage.

But the reasons for the teacher’s reluctance are exactly the reasons why the researcher should be there. These earlier encounters are of crucial significance not only for understanding what comes later but in actually providing for what comes later.

(Ball 1980: 143–4)

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

What is required of the researcher by gatekeepers in order to grant access may not be simply a matter of their judgement and power but may be covered by institutional regulations and even the law; though, of course, these regulations and laws are also resources that gatekeepers can use for their own, and for institutional, purposes. For example, it is common today, in some countries, that research in schools, especially that focusing on the students, requires parents’ consent for their children to be included in the study. Thus, it is necessary to send home consent forms with the students and (as far as is possible) to restrict the focus of inquiry to those students whose parents have agreed. One problem here is that the return rates of such forms are not usually high: and this is not always because parents are unwilling to give consent but rather that the forms get forgotten or lost. In her study of a large English urban comprehensive school, Hudson (2004) adopted an effective (if not entirely politically correct) strategy of dealing with this problem: she set up a raffle with a prize of McDonald’s vouchers into which all students who brought back their forms (whether with consent agreed or refused) would be entered (Hudson 2004: 267).

To deceive or not to deceive?

Sometimes, of course, it may be judged that the relevant gatekeepers will almost certainly block entry altogether. Here, resort may be made to secret research.4 This was the case, for example, with Calvey’s (2000) research on ‘bouncers’. He judged that entry would almost certainly be barred or that his relations with bouncers would be undermined if his research identity were known; though his choice of a covert strategy was also informed by an ethnographically-inspired concern with minimizing reactivity and gaining access to the spontaneous lived experience of the people he was studying (Calvey 2000: 46–7). A very similar justification was offered by Graham for her covert study of a Japanese car factory in the United States (Graham 1995).

Covert research does not always involve an outsider entering the field by ‘passing’ or by formally taking on a job there. It may also be carried out by those who are already participants in the relevant context. A case in point is Holdaway’s (1982) study of the police. As a serving officer who was seconded to university to read sociology and returned to the force wishing to do research on it, he reports that he was faced with six options:

A. Seek the permission of the chief officer to research, giving full details of method and intention.
B. Seek permission as above, so phrasing the research description that it disguised my real intentions.
C. Seek permission of lower ranks, later requesting more formal acceptance from senior officers.
D. Do no research.
E. Resign from the police service.
F. Carry out covert research.

I chose the final option without much difficulty. From the available evidence, it seemed the only realistic option; alternatives were unrealistic or contained an element of the unethical which bore similarity to covert observation. I believe that my senior officers would have either refused permission to research or obstructed me. Option B is as dishonest a strategy as covert research, if the latter is thought dishonest. For example, if I were a Marxist and wanted to research the police and declared my Marxism, I know that I would be denied research access; yet to ‘front’ myself in a different research guise is surely dishonest. Option C could not have been managed. D denies the relevance of my studies, and Option E would have been its logical progression – yet I felt an obligation to return to the police who had financed my [university studies].

(Holdaway 1982: 63)

4 We discuss the ethical issues surrounding covert research in Chapter 10.
Holdaway knew the setting he wanted to research, and the gatekeepers who would have had to be approached to get permission, very well indeed. Often, however, judgements to the effect that access to a setting is impossible are less well founded. There are some settings to which one might expect entry to be blocked but that have nevertheless been shown to be accessible, at least to some degree. For example, Fielding (1982) approached an extreme right-wing political organization in the United Kingdom, the National Front, for permission to carry out research on their organization, and received it; though he felt it necessary to supplement official access with some covert observation. Some years later, Back (2004) discovered that, despite his own associations with anti-racist work, it was possible to gain an interview with the leader of a similar organization, the British National Party.

Indeed, there is often a considerable amount of uncertainty and variation in the scope for negotiating access. Shaffir (1985) was told that the Tasher Hassidic community he was interested in studying would not agree to be researched. He was advised to get a job in the community and do covert research, which he did:

Since I suspected that members of the community would not sanction my sociological investigation, I did not inform the Tasher that I was collecting data about them. (Neither did I tell them about my connection with the Lubavitcher, a community they disapproved of because of the involvement of its members with non-Orthodox Jews.) I did, however, tell those who were interested that I was a sociology student at McGill University. Invariably, I was asked to explain the meaning of ‘sociology’, a term that was entirely foreign to the Tasher.

But I was able to define it sufficiently to use my interest in sociology to add legitimacy to the kinds of questions I regularly asked about the organization of the community. . . . Some people were surprised at my curiosity about topics unconnected with my clerical duties. However, others seemed convinced by my explanations and volunteered information about themselves which they believed might interest an outsider. But several members looked at me so oddly I felt they considered me an intruder and were (quite rightly!) suspicious of my presence.

(Shaffir 1985: 126)

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

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

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

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

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

(Rather surprisingly, perhaps, Chambless (1975) recounts a more straightforward process of gaining access to the world of organized crime, but once again one relying on an initial covert approach:

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

MILLIE: To play cards.
ME: Back there?
MILLIE: Yes, that’s where the poker games are.
ME: Can I play?

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

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

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

MILLIE: He’s the bag man.
ME: The what?
MILLIE: The bag man. He collects the payoff for the people downstairs
ME: Oh.
I spent the next two months talking informally to people I met at different games, in pornography shops, or on the streets. I soon began to feel that I was at a dead end... I had discovered the broad outlines of organized crime in Seattle, but how it worked at the higher level was still a mystery. I decided it was time to 'blow my cover'.

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

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

(Chambless 1975: 36-8)

The work of Calvey, Graham, Holdaway, Fielding, Shafir, and Chambless raises the question of deception in negotiations over access. Where the research is secret to all those under study, and to gatekeepers too, the practical problem of access may be 'solved' at a stroke, providing the deception is not discovered. Nor need the covert researcher be restricted to a single role, and adopting more than one may improve the quality of the data. In the initial stages of her research on shop work, Pettinger (2004) took a job as a part-time store assistant, which enabled her to discover something of what went on behind the scenes, even while she realized that it was not providing the full story. To complement this data, she subsequently engaged in covert observation as a shopper. This

entailed visiting stores on a regular basis and looking at how many people were working, their gender, ethnic backgrounds, class and age, the tasks being done and by whom. I did not only observe, I also tried to manipulate events. I 'tested' customer service provision by demanding customer services, as any other shopper might, seeing how stores had different norms and regulations.

(Pettinger 2005: 356)

She comments that whereas 'taking the role of the worker involved a continual need to be active in order to avoid the condemnation of colleagues and customers',

as a shopper, I had the cultural freedom to be a 'flaneur', an activity that intrinsically involves looking. Thus, the social role of the customer provided a different form and style of data gathering that could add new layers of meaning.

(Pettinger 2005: 356)

Even when 'cover' is successfully maintained, the researcher engaging in covert research has to live with the moral qualms, anxieties, and practical difficulties to which the use of this strategy may lead. However, research carried out without the knowledge of anyone in, or associated with, the setting is quite rare. Much more common is that some people are kept in the dark while others are taken into the researcher's confidence, at least partly. Thus, for example, in studying 'the role of faculty senate in shared governance', Labarge sought to protect his 'insiderism' by operating in a covert way, but found it necessary to share the information that he was researching the situation with some senate colleagues (Labarge 2002: 98).

What is at issue here, though, is not just whether permission to carry out the research is requested, and from whom, but also what those concerned are told about it. Some commentators recommend that an explicit research bargain, spelling out in full the purposes of the research and the procedures to be employed, be made with all those involved, right from the start. This is often seen as essential to the requirement of informed consent. Often, though, in ethnographic work, this is neither possible nor desirable. As a result of the way in which research problems may change over the course of fieldwork, the demands likely to be made on people in the setting and the policy implications and political consequences of the research are often a matter for little more than speculation at the outset. There is also the danger that the information provided will influence the behaviour of the people under study in such a way as to invalidate the findings. While often it may be judged that the chances of this are small, given the other pressures operating on these people, there are instances where it may be critical. Had Festinger et al. (1956) informed the apocalyptic religious group they were studying not only that the research was taking place but also about the hypothesis under investigation, this would almost certainly have undermined the validity of their research.

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

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

In colleges of high prestige, the researcher may be hampered in his negotiations because the administrators cannot imagine that anything harmful to the college could be discovered. In this case, it is up to the researcher to explain the kinds of
things that often turn up. . . . The administrator can sometimes be drawn into a scientific partnership. By treating him as a broadminded and sophisticated academic, one gradually works him around to a realization that although the study may be threatening, he and his college are big enough to take it. It may seem unnecessary to prepare administrators for the worst in this fashion, but it prepares the ground for the shock they may get when they see the manuscript at the end of a study. Administrators may attempt to prevent publication or feel that the college has been exploited and similar research should not be authorized. However, the administrator who has committed himself to a generous research bargain is more likely to be proud of the results.

(Geer 1970: 83)

Negotiating access is a balancing act, then. Gains and losses now and later, as well as ethical and strategic considerations, must be traded off against one another in whatever manner is judged at the time to be most appropriate, given the purposes of the research and the circumstances in which it is to be carried out. Moreover, changes in judgement about what is best may need to be made as the research progresses.

**Obstructive and facilitative relationships**

Seeking the permission of gatekeepers or the support of sponsors is often an unavoidable first step in gaining access to the data. Furthermore, the relationships established with such people can have important consequences for the subsequent course of the research. Berreman (1962), discussing his research on a Pahari village in the Himalayas, reports:

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

(Berreman 1962: 6)

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

"The impression I received of people's attitudes to me was that they were very curious and very friendly. As I walked along country paths I was constantly being bothered by inquisitive peasants who had no inhibitions in talking about their problems, especially in relation to the land. It took at least an hour to cross from one side of the village to the other due to the constant need to stop and converse. This contrasts markedly to reports I had received from anthropologists who have worked in Quechua-speaking areas of Peru and have found people dull and uncommunicative. I believe one reason for this is that my introductions into the area were exceptionally good. On the one hand, my official introductions through the Ministry of Agriculture had come through the one official who was not distrusted."

He was referred to as 'a good person, he didn't try to cheat us like the other officials'. On the other hand, I had introductions through, and for a time lived in the same building as, members of the progressive Catholic Church. They also happened to be Europeans. Their identification with the peasants, and people's identification of me with them, was extremely valuable.

(Rainbird 1990: 89)

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

Barrett (1974) reports that the members of his chosen village, Benabarre, were initially reserved. This was partially breached when a village baker started to take Barrett round and introduce him to others. However, the big breakthrough came when the village was visited by a Barcelona professor who was descended from a Benabarre family. The professor was interested in Barrett's work and spent a good deal of time with him:

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

(Barrett 1974: 7)

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

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

(Barrett 1974: 8)
Hansen’s (1977) experiences in rural Catalonia are equally revealing about the hierarchical assumptions of village life:

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

(Hansen 1977: 163-4)

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

The model of the ‘expert’ often seems to suggest that the social researcher is, or should be, a person who is extremely well informed as to ‘problems’ and their ‘solutions’. The expectation may be set up that the ethnographer seeking access is claiming such expertise, and is expecting to ‘sort out’ the organization or community. This view therefore leads directly to the second image, that of the ‘critic’. Gatekeepers may expect the ethnographer to try to act as an evaluator.5

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

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

From a variety of contexts researchers report hosts’ suspicions and expectations as often proving barriers to access. Such suspicions may be fuelled by the very activities of the fieldworker. Barrett (1974), for instance, remarks on how the inhabitants of his Spanish village interpreted his actions. He was not sensitive to the possibility that villagers might be frightened by someone making notes, when they did not know what was being written down. Rumours about him included beliefs that he was a communist spy, a CIA agent, a Protestant missionary or a government tax agent. Relatedly, in her fieldwork in Brazil in the late 1930s, Landes (1986) was accused of seeking ‘vigorous’ men to do more than carry her luggage. She was labelled a prostitute during her research because she inadvertently broke the local rules about the proper behaviour of a woman (Landes 1986: 137). As might be expected, this created problems for her research and for her personal relationships in the field. Scourfield and Coffey (2006) report a similar experience in which a male researcher was negotiating access to a UK local authority social services department: questions were raised as to whether he might be a paedophile wanting to make contact with others of a similar persuasion. Suspicions may also sometimes arise as a result of events in the society being studied. Owens (2003) reports how a political crisis in Zanzibar led to him being identified as a spy and ordered out of the country without warning.

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

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

(Shaffir 1991: 76)

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

As we noted early on in this chapter, the problem of access is not resolved once one has gained entry to a setting, since this by no means guarantees access to all the relevant data available within it. Not all parts of the setting will be equally open to observation, and not everyone may be willing to talk. Moreover, even the most willing informant will not be prepared, or perhaps even able, to divulge all the information available to him or her. If the data required are to be obtained, negotiation of access is therefore likely to be a recurrent preoccupation for the ethnographer. Negotiation here takes two different but by no means unrelated forms. On the one hand, explicit discussion with those whose activities one wishes to study may take place, much along

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5 Sometimes, of course, the ethnographer may be officially engaged in evaluation: see Fetterman (1984); Fetterman and Pittman (1986); Shaw (1990); McKee (2002). However, even in this situation, it may still be advisable to try to distance oneself from the roles of both expert and critic.
the lines of that with sponsors and gatekeepers. But the term 'negotiation' also refers to the much more wide-ranging and subtle process of manoeuvring oneself into a position from which the necessary data can be collected. Patience and diplomacy are often at a premium here, though sometimes boldness is also required. The ethnographer's negotiation of a role in the setting, and the implications of different roles for the nature of the data collected, will be examined in the next chapter.

4 Field relations

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, ships, 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 category 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

Where the research is overt, as with gatekeepers and sponsors, people in the field will seek to place or locate the ethnographer within the social landscape defined by their experience. Some individuals and groups have little or no knowledge of social research; and, as we saw in the previous chapter, field researchers are frequently suspected, initially at least, of being spies, tax inspectors, missionaries, or of belonging to some other group that may be perceived as undesirable. Thus, Kaplan reports that the New England fishermen she studied initially 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 Federal Bureau of Investigation (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 on those subject to the evaluation. Despite this, Hunt (1984) 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 (1967) 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 social 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; and, here again, a balancing act may need to be achieved between these roles and that of someone who is unacceptably ignorant or naive. Thus, Atkinson found it necessary to manage ... contrasting impressions of expertise and ignorance' (Atkinson 1997: 65) during the course of his fieldwork in a medical school. The students tended to assume his ignorance, but there were also some who came to recognize that he had picked up quite a bit of medical knowledge in the course of the research and resented 'my ability to gain some passing acquaintance with their subject, without the background training in the basic and medical sciences' (1997: 62).

Occasionally, participants may be, or consider themselves to be, very sophisticated in their knowledge of research methodology; and/or they may have a negative attitude towards research. Anderson, for example, found that his whole way of investigating environmental activism was challenged by those he studied, leading him radically to restructure his approach (Anderson 2002).

This problem of resistance may be especially acute, of course, where the people being studied are academics, or even sociologists, themselves (Platt 1981). Scott (1984) provides an example from research on the experience of postgraduate students in British universities. Along with her co-researcher, she 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)

Resistance, or at least reluctance to participate, may also arise because of fears of retaliation by others. Baez (2002) reports how, in his study of minority ethnic group staff in a private US university, one tenured African-American professor within the sociology department refused to allow his interview to be audio-recorded, 'suggesting that to do so would place him at risk of retaliation from his colleagues' (Baez 2002: 39).

Outside academia there may be less knowledge but equal or greater hostility. The comment of a constable in the Royal Ulster Constabulary (RUC), 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 shit, 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'll 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. The management of "personal front" (Goffman 1955) is important here. As in other situations where identities have to be created or established, much thought must be given to the ethnographer to 'impression management'. Impressions that pose an obstacle to access must be avoided or countered as far as possible, while those which facilitate it must be encouraged, within the limits set by ethical considerations.

1 For a striking analysis of this process, see Edgerton (1965).
Field relations

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 (1973) 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 – ‘in gallous wae’.

(Patrick 1973: 15)

Much the same sort of attention to dress is often required in research that is destined to be overt, especially where an initial period of gaining trust is necessary. In the case of Wolf’s (1991) 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 (1991) 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 (1990) research on homeless people. 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 homeless people whom he wished to interview. He solved this problem by carrying an old briefcase that was cheap-looking and whose stitching had unraveled 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 (1984) 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 (1967) 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 [his research site] in a suit and tie. My loss of case 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, Sudarkasa (1986) 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 jeopardise the entire enterprise. Having gained access to the Edinburgh Medical School, for instance, Atkinson (1976, 1981a) went to see one of the influential gatekeepers for an ‘informal’ chat about the actual fieldwork. He was dressed extremely casually (as well as having very long hair). He had absolutely no intention of going on to the hospital wards looking like this. 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.

At the same time, there may be personal limits for the researcher in how far the strategic use of dress and other aspects of identity can and should be manipulated in order to establish good field relations. While Blackwood (1995) hid her lesbianism from her Indonesian hosts, she did not feel able to conform completely to the expectations made about her as an unmarried, and presumably heterosexual, woman. She writes:

where the dress code was at odds with my lesbian self... I developed the most resistance in reconstructing my identity. I could not force myself to wear skirts as any proper Indonesian woman does, except very occasionally. My host sometimes remarked on this lapse because it raised deeper questions for her about my womanhood.

(Blackwood 1995: 58; quoted in Coffey 1999: 26–7)

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 generally, 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 (1974) 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)

With covert research, of course, having relevant kinds of knowledge and skill will often be essential to playing particular roles. Calvey’s employment as a bouncer depended to a large extent on his having studied martial arts for many years, and his taking a course in ‘door supervision’ (Calvey 2000: 44). Holdaway’s (1982) covert study of the police required not just that he was already formally a member of the police force, but also that he had the knowledge, skills and experience to carry out the job and to protect his research identity. Graham’s (1995) experience in previous factory work was probably essential for her to survive ‘on the line’ at Subaru-Isuzu Automotive. Expertise and knowledge may also be of value in the field as a basis for establishing reciprocity with participants. Thus, in some contexts, anthropologists often find themselves trading on their 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 giving 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):

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

Both Hudson and Beynon note that the employment of such strategies in establishing ‘mutuality’ was more than simply establishing field relations. Not only did such exchanges facilitate the collection of data, but also they were data in their own right. At the same time, ethnographers often experience some feelings of personal disquiet, wondering whether they are unduly exploitative in offering ‘friendship’ in return for data. There are no easy answers to such questions, they always depend upon the particular circumstances and personal judgement.

A problem that the ethnographer often faces in the course of fieldwork 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, in particular, have stressed the importance of this from an ethical point of view also (see, for example, Oakley 1981; see also Tang 2002). At the same time, just as in many everyday situations, as a researcher one often has to suppress or play down 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). However, 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). Indeed, it may be necessary to tolerate situations, actions, and people of which one disapproves, or that one finds distasteful or shocking (Hammersley 2005a).

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 (1988) 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 himself or herself being ‘tested’ and pushed towards disclosure, particularly when the group or culture in question is founded upon strong 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.

It is worth emphasizing the contingencies involved in these processes, however. For instance, Abell et al. (2006) report the divergent results on different occasions, and in different contexts, of their efforts to build relationships with young people through disclosing shared experience. What is expected to be recognized as commonality can sometimes be interpreted as marking significant difference. And this may, sometimes, damage the nature of the data that become available.

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 determined or fixed, such characteristics as gender, age, ‘race’, and ethnic identification may shape relationships with gatekeepers, sponsors, and people under study in important ways. In the case of covert participant observation, these characteristics may of course be a barrier to doing the research that is difficult if not impossible to overcome. For example, going undercover as a ‘bouncer’ on a club or pub door (Calvey 2000) involves certain requirements, at the very least in terms of age and physique, that not everyone would meet.

Similarly, 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 (Roberts 1981; Goode 1980; Whitehead and Conaway 1986; Warren 1985; Westmarland 2000). 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, elderly people, and so on. In Golde’s (1986) 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.
Field relations

. . . Being an unmarried girl meant that I should not drink, smoke, go out 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 settings that are reserved for women, especially in cultures where there is a strong division between the sexes; and even in public settings there may be rules about what they can and cannot do.

However, often, 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 (1990) 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. Esterday 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 – which may, or may not, be a problem in research terms. Not only may the female researcher sometimes find it difficult to be taken seriously by male hosts, but also other females may display suspicion and hostility in the face of her intrusions. Of course, female researchers may also find advantageous trade-offs. The ‘hustling’ informant who is trying to impress the researcher may prove particularly forthcoming to her; 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 (1988) 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 perceived 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, ‘ahh, what harm can she do, she’s only a broad’. Upstairs I went.

(Warren 1988: 18)

There is often some scope, then, both for capitalizing on gender roles and for renegotiating some aspects of them for the purposes of the fieldwork. This is part of the more general process of impression management. Thus, in her study of the police, Westmarland had to contend with ‘protectiveness’ on the part of male police officers, but this related to her outsider status as well as to her gender. Moreover, by passing various tests and ‘showing bottle’, good field relations were established (Westmarland 2000). Of course, gaining respect and trust may take considerable time for any ethnographer, and there will always be limits to how far this is achieved.

‘Race’, ethnicity, and religious affiliation, like gender, can also set limits and pose problems. ‘Race’ is, of course, not merely a matter of physical appearance, but is also defined in terms of culture, power, and personal style. Keiser (1970), reflecting on his work with the ‘Vice Lords’, a Chicago street gang, notes that it was difficult for him, as a white man, to establish relationships with black informants. While some were willing to accept him as a ‘white nigger’, others displayed strong antagonisms. 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)

Peshkin’s (1985) experience researching a fundamentalist Protestant school shows also that the ethnicity and religious affiliation of the ethnographer can be an important factor in the establishment of field relations:
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. Bethamites let me define my research self, but could never rest easy with my unwaved 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 Fein’s Republican News. (Brewer 1991: 21)

Sometimes, belonging to a different ethnic or national group can even have distinct advantages. Hanmer (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 usefully 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; 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.

Age can also have an effect on the researcher’s modus operandi, as Henslin (1990) 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 engaging 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.

Being the ‘wrong’ age, or generation, can be a particular problem in covert research, in that it may lead to the researcher engaging in ‘inappropriate’ behaviour. In the covert phase of his research on college students, Moffatt (1989: 7?) found that he had to control both his tendency towards ‘high’ or academic talk and the use of ‘low’ talk about sex.

Much the same sort of problem can arise, though, in overt research, where behaving in what are judged to be inappropriate ways may damage relations with some participants (though it may improve relations with others!).

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)
Very often researchers working with children have sought to adopt the ‘least adult’ role in this manner. This can/may work well, but it can create its own limitations (Fine and Sandstrom 1988; Mandell 1988; Epstein 1998).

We have restricted our 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 (1986) 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)

This example also illustrates the fact that it is not just ethnographers’ own personal or social characteristics that can be crucial: so may be those of partners who accompany them into the field. And, occasionally, research may be done in a field in which a partner already has an established role. Hudson (2000) studied a school in which her husband was employed, and in the course of the fieldwork he moved from being a teacher to being assistant principal in charge of discipline. Not surprisingly, this had an influence on how the students perceived her, but it did not have the damaging results that might be anticipated. At the outset, she decided ‘not to introduce the subject of being married to a teacher’ but at the same time that ‘if the young people raised the subject, then I would answer their questions openly’. She reports that ‘it was not long before the subject arose’. Initially, they confused her husband with another teacher.

Girls crowded round in a flurry of sympathy. Anna exclaimed: ‘What not that tall bloke! Poor you! What’s he like at home?’ (Research Log, 24.1.97). Her remark suggests some sympathy for my perceived position, rather than constructing Richard and myself as a couple, separate from the young people. Indeed, over fieldwork, the young people’s comments tended to suggest they had a much stronger sense of my relationship with them, rather than any relationship I might have with my husband. For instance, later in the fieldwork, when Sara was reading the school newsletter in December 1997, she turned to me and exclaimed: ‘Miss! Did you know your husband’s been made Assistant Principal?’ (Research Log, 12.12.97). When Sara telephoned me at school during the summer holidays, my husband answered the phone. . . . Despite the explicit reminder that I was married to someone official, when I asked how she was, [she] immediately launched into a diatribe about a boy she had ‘shagged’ and described the prospect of returning to school as ‘crap’.

(Hudson 2004: 264)

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 other aspects of appearance and manner, and relationships. 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, not least 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. But, in an important sense, the novice is also acting like a social scientist: making observations and inferences, asking informants, constructing hypotheses, and acting on them.

When studying an unfamiliar setting, the ethnographer is necessarily a novice. Moreover, wherever possible they must put themselves 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 a good sense of the social structure of the setting and begin to understand the culture(s) of participants.

Styles (1979) 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 field notes, 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 be 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 customers, 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 discovers that what he or she knows about the new country will not suffice for survival in it. Laura Bohannon (under the pen name Doreen Bowon) 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 household 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 (1997) 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 Rousseau-like expectations about 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, he expected to encounter social facts running about altruistically calling each other kinship terms... each waiting and anxious to have me collect [their genealogies]. 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 1997: 11–12)

It is worth noting in passing that Chagnon’s account shows not only the ‘culture clash’ of the Westerner encountering an ‘exotic’ culture, but also the problem of the social scientist who expects to uncover social facts, rules, institutions, organizations, and so on by direct observation of the social world. This is perhaps one of the hardest lessons to learn at the outset. One does not ‘see’ everyday life laid out like a sociology or anthropology textbook, and one cannot read off analytic concepts directly from the phenomena 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, it may be necessary to ‘fight familiarity’ (Delamont and Atkinson 1995). Becker (1971) provides a classic statement of the problem, in the context of research on educational institutions:
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 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 it may not be possible to take on a novice role. We noted in Chapter 3 how researchers are sometimes cast into the role of expert or critic. Moreover, ascribed characteristics, notably age, and latent identities, may reinforce this. In studying such settings the ethnographer is faced with the difficult task of rapidly acquiring the ability to act competently, which is not always easy even within familiar settings, while simultaneously privately struggling to suspend for analytic purposes precisely those assumptions that must be taken for granted in relations with participants.

The 'acceptable incompetent' is not, then, the only role that ethnographers may take on in the field; and, indeed, even where it is adopted it is often abandoned, to one degree or another, as the fieldwork progresses. There have been several attempts to map out the various roles that ethnographers may adopt in settings (Adler and Adler 1991, 1994). In their classic accounts, Junker (1960) and Gold (1955), for example, distinguish between the 'complete participant', 'participant-as-observer', 'observer-as-participant', and 'complete observer', these representing points on a dimension from 'external' to 'internal'.

In the 'complete participant' role, the ethnographer's activities are wholly concealed. Here the researcher may try to 'pass' as an ordinary participant in a scene (Karp 1980; Pettinger 2000) or will join covertly an organization or group - Alcoholics Anonymous (Loft and Lejeune 1960), Pentecostalists (Homan 1980), an army unit (Sullivan et al. 1958), a mental hospital (Rosenhahn 1973), bouncers on a club door (Calvey 2000), the staff of a shop (Pettinger 2005). Complete participation is also involved 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.

What 'complete participation' means will vary, of course; but it is approximated in some circumstances. Some commentators have suggested that it is the ideal to which researchers should aim (Jules-Rosette 1978a, 1978b; Ferrell and Hamm 1998). Jules-Rosette, for instance, argued for the necessity of 'total immersion' in a native culture: in other words, not simply pretending to be a member but actually committing oneself, body and soul. In her case this was accompanied by conversion to the Apostolic Church of John Maranke, an indigenous African movement. This individual 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 perhaps seem very attractive. Such identification and immersion in the setting 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 covert participation may be the only strategy by which the data required can be obtained. However, when carried out over a protracted period it usually places great strain on the fieldworker's dramatical capacities. Moreover, however successfully the research is pursued in these terms, various contingencies may lead to one's cover being blown. Calvey's (2000) covert research on bouncers was endangered by the following item in the local student newspaper:

Conscientious as ever, members of staff at the Sociology Department have been doing undercover research into the life of those much-maligned guardians of the door. Bouncers. At least, Biteback presumes that's why some tutors have been working on the door at Pub X in town. They wouldn't be moonlighting would they?

(Manchester issue no. 13, 29 January 1996; quoted in Calvey 2000)

While in this case no serious consequences followed, the effect can 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 Armat and, 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 about their backgound? 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'.

Field relations
Field relations

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,' Lure 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 therefore normally prove hard for the fieldworker to arrange his or her actions in order to optimize data collection possibilities. Some potentially fruitful lines of inquiry may be rendered practically impossible, in so far as the complete participant has to act in accordance with existing role expectations. At the same time, of course, others may be opened up that might not have been available to someone researching overtly.

There are also issues to do with the sheer time and energy that can be taken up with participation. 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 of public behaviour in the street from a window (Loфland 1973) also falls into this category, and perhaps also research like that by Karp (1980) on the ‘public sexual scene’ in Times Square or Pettinger’s (2005) adoption of the role of shopper in department stores.

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 the people being studied. On the other hand, there may be severe limits on what can and cannot be observed, and questioning of participants may be impossible. Adopting either of these roles alone would usually 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 the only options possible.

Most field research involves roles somewhere between these two poles. And Junker (1960) and Gold (1958) distinguish two midway positions: participant-as-observer and observer-as-participant. Whether this distinction is of any value is a moot point. Indeed, 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. Another important issue concerns how central to proceedings within the setting is any established role taken on by the ethnographer.

Of course, in secret research one has little option but to adopt an existing role, though it may be possible to extend and modify it somewhat to facilitate the research. And sometimes even in open research there may be no choice but to adopt an established role, as Frelich (1970a, 1970b) 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.

(Frelich 1970b: 193)

Curer (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’ (Curer 1992: 17-18).
Field relations

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 Ghail 1991), but sometimes they have not (Brown 1967; 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 (Moffatt 1989; Tobias 1990).

Reflecting on her research into how families with school-age children use television and other media in their homes, Jordan (2006) explores the limitations on the role of researcher in this context, and how these are co-constructed. The fact that the setting was very much a private one made the task of negotiating access and creating a path that was both ethically defensible and ethnographically productive a delicate matter. She draws attention to the subtle changes in ascription and adoption of roles that the people she studied engaged in, and the implications of this for her own behaviour. She traces her movement among the roles of ‘researcher as student’, ‘researcher as person’, ‘researcher as guest’, and ‘researcher as negative agent’. The last of these ascribed and felt roles arose where the researcher’s presence seemed to exacerbate tensions within families (Jordan 2006: 179-80).

Decisions about the sort of role to try 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, Sevigny (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

Another dimension of variation associated with the typology of research roles developed by Junker (1966) and Gold (1958) concerns the perspective adopted. In crude terms, this can range from the ‘external’ view of an observer to an ‘internal’ view from the position of one or more participants. However, this dimension is surrounded by what Styles (1979) 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)

Indeed, the very distinction between outsider and insider is problematic (Merton 1972; Labarre 2002: Kusow 2003). In her study of African-American communities on the Sea Islands of South Carolina, Beoku-Bets was able to draw on her racial background to build rapport with the Gullah women, but as an educated professional and university academic she was also sometimes positioned as an outsider (Beoku-Bets 1994).

But the same time, the insider/outside distinction does capture something important about the different sorts of roles that ethnographers can play in the field, and the perspectives associated with them. Broadly speaking, those defined as outsiders or 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 orientations of participants. Where this strategy is used alone, participant 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 would 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. Indeed, Labarre (2002) suggests that even in research where an insider role is taken on, there is a danger of ‘going observationalist’.

A more common danger in ethnographic research, one that is associated with the other three roles in Junker’s and Gold’s typology, is ‘going native’. Not only may the task of analysis be abandoned in favour of the joys of participation, but also, even where it is retained, bias may arise from ‘over-rapport’. In a classic discussion, Miller (1952) 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 more 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 contact 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 (1952), 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
personally identifying with such members’ perspectives, and hence of failing to treat these as problematic.

One very 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 displayed ‘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 also have no desire for them, enthusiastically seeking working-class employment. Willis argues that their school counterculture 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 these did not conflict with his own. Hence, the book becomes, in many ways, a celebration of their lifestyle: 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 affected by this.

In a striking parallel, from some years earlier, Stein (1964) provides a reflexive account of his own identification with one set of workers, the miners in the gypsum plant he studied with Goudner (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 loyalty 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 being 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 (Powell et al. 1966; Everhart 1977). As the title of the collection edited by Frew and Frewell (1978) 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. And the consequences of being discovered to be a researcher could in some cases be dire, as for example in Sheperd-Hughes’s (2004) covert investigation of illegal trade in body organs. In overt participant observation there is the strain of living with the ambiguity and uncertainty of one’s social position on the margin, and the stress will be particularly great where one is researching a setting from which one cannot escape at the end of each day, in which one must remain for days at a time; as, for example, in ethnographic research carried out on board ship (Sampson 2004).

Johnson (1975) 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 Winthrop’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, along in the field: it is based on the experiences of a number of graduate students, along in the field.
Field relations

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 retire 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.

(Winstroh 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' (Wax 1971: 70).

These problems are, of course, exacerbated where the fieldwork is carried out in a setting that involves dangerous activities or unusual risks. Here, sensible judgements have to be made about what to do and what not to do; and about what precautions to take. The problem is that these will both shape others' perceptions of the researcher, and the extent to which it is possible to gain both 'inside' and 'outside' perspectives (Lee 1995; Lee-Trewick and Linkogle 2000).

We do not wish to convey the impression that the experience of fieldwork is one of unrestrained stress and 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. Loftland (1971: 108–9) draws attention to the 'poignancy' of this experience. There is a sense of split personality 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: tough decisions concerning fieldwork strategy have to be made; working relationships may have to be established quickly; and social embarrassment, or worse, is a real possibility. On the other hand, it would be dangerous to assume that this is just a temporary 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. And 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 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.

As we have already indicated, marginality is not the only source of strain and stress in fieldwork. Another is finding oneself in physical and social situations that one might not otherwise encounter and would normally avoid. Henslin (1990) provides an example from his participant observation research on homeless people:

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 blanket. 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 the performances made by one of the directors of the shelter. Earlier that day, as I was interviewing him . . . he mentioned homosexual rapes 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 went 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, particularly to 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; Kulick and Wilson 1995; Coffey 1999; Goode 1999, 2002). Nevertheless, sexual harassment, at the very least, can be a problem. Warren (1988) reports the research of one of her students Liz Brunner among homeless people:

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 (1991) reports from her research on lawyers:

One clear-cut example of a problem related to my gender was an instance of sexual hounding 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 analyse 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 (1991) 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 bravado nor excessive cruelty, but as a reasoned proposal. We all faced the 'queer' problem didn't we? And the community will need 'cleaning' 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.

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 (1992) 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 (Delamont 2004). 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) 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 Rocketts were eliminated in the third round of the playoffs and the season was suddenly over. In just a few days the majority of the Rocketts left Summit City.

(Gallmeier 1991: 226)
the experience may sometimes be traumatic. An extreme case is that of Young (1991), 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, and some sadness, but often with not a little relief.

Conclusion

In Chapter 1 we emphasized the importance of recognizing the role of the researcher in generating ethnographic data. Rather than seeking, by one means or another, to eliminate reactivity, its effects should be monitored and, as far as possible, brought under analytic control. As we have seen, there are a variety of strategies and 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, field roles are not entirely under the control of the ethnographer; establishing and maintaining field relations can be a stressful as well as an exciting experience; and leaving the field may involve some difficulties. Furthermore, ethnographers must learn to cope with their own feelings if they are to sustain their position as marginal natives and complete their 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, and of their own experience of participation in settings. Equally important, though, are the accounts that people in the setting provide, while being observed or in interviews. In the next chapter we consider the role of such accounts in ethnographic research.

5 Oral accounts and the role of interviewing

It is a distinctive feature of social research that the ‘objects’ studied are in fact ‘subjects’, in the sense that they have consciousness and agency. Moreover, unlike physical objects or animals, they produce accounts of themselves and their worlds. Recognition of the significance of this has always been central to ethnographic thinking, though it has been interpreted in somewhat different ways over time and across different fields. Generally speaking, there has been a tension between treating the accounts of the people being studied as sources of information about themselves and the world in which they live, and treating those accounts as social products whose analysis can tell us something about the socio-cultural processes that generated them.

Differences in view about the methodological function, and importance, of participants’ accounts have been generated by divergent methodological philosophies. From the point of view of positivism, common-sense accounts are subjective and must be replaced by scientific ones. On the strictest interpretation this leads to a behaviourist stance, to the effect that people’s own accounts are simply epiphenomena that have no causal significance, and therefore little relevance in explaining their behaviour. By contrast, naturalism treats common-sense knowledge as constitutive of social reality; and, therefore, requires that it be appreciated and described, not ignored or explained away. More recent ethnographic critics of naturalism retain an interest in insider accounts, but they adopt a variety of attitudes towards them. Some regard the role of the ethnographer as to amplify the voices of those on the social margins; these being treated as having epistemological or ethical privilege. They therefore seek ways of representing insider accounts in ways that preserve their authenticity. Here, often, the ethnographer’s role approaches advocacy. Others see the task as to deconstruct accounts in order to understand how they were produced and the presuppositions on which they are based. Here the ethnographer’s role comes close to ideology critique. Slightly different again is the stance of those ethnographers, for example influenced by discourse analysis, for whom accounting practices, in interviews or in naturally occurring talk, are an important topic for investigation rather than the accounts being usable as sources of information. Associated with some of these latter views is a tendency to reject any concept of validity which implies even a potential correspondence between informants’ accounts and the world (Murphy et al. 1998; Atkinson and Coffey 2002).

Our own position is a catholic one, fitting neatly into none of these categories. For us, there are two legitimate and complementary ways in which participants’ accounts can be used by ethnographers. First, they can be read for what they tell us about the phenomena to which they refer. Second, we can analyse them in terms of the perspectives they imply, the discursive strategies they employ, and even the psychosocial dynamics they suggest.
Epilogue

A distinctive analytic mentality

As we have tried to make clear in this book, there is an important sense in which ethnography is not just a set of methods but rather a particular mode of looking, listening, and thinking about social phenomena. In short, it displays a distinctive analytic mentality. This does not imply prior commitment to any single set of very specific ideas about the nature of the social world and how it can be understood. Rather, what is involved in this mentality is more along the following lines. A commitment:

1. not to jump to quick conclusions, even though this will delay the forming of hypotheses about what is going on, and even though the aim is eventually to reach some sort of conclusion;
2. pay detailed attention to appearances, while not taking them at face value;
3. seek to understand other people’s views without treating what they say as either obviously true or obviously false;
4. examine the circumstances in which people act, including much that they may not be aware of themselves, yet without losing sight of what they do attend to.

As our brief list makes clear, this orientation involves some difficult tensions. But while these are a source of recurrent troubles for ethnographers, they are also part of the dynamic of ethnographic work, they are what drives it forward in interesting directions. In this epilogue, we will look at some of the key tensions within the ethnographic mentality in more detail.

Getting the view from inside and from outside

One tension is between what we might call participant and analytic perspectives. Ethnographers typically insist on the need to understand the perspectives of the people being studied in order to explain, or even to describe accurately, the activities in which those people engage. In this respect, ethnographers are opposed to any approach which assumes that researchers can immediately know what people are doing and why, as well as those approaches which insist that we must start from some set of prior theoretical categories in describing people’s behaviour. Instead, ethnographers insist on the value, for the purposes of inquiry, of suspending their own immediate inferences, common-sense assumptions and theoretical presuppositions, as far as possible, so as to try to take full account of what people say about their world and what they do. This emphasis on understanding people’s own perspectives has a long history. The need for it is particularly obvious where one is studying a society that is very different from one’s own – since much of what is seen and heard will not be immediately intelligible. However, it is equally, if not more, important to adopt this stance in studying familiar settings, where the tendency to assume that one already knows what is going on, and why, as well as who is involved and for what purposes, is particularly strong. While our inferences about these matters may of course be sound, this cannot be assumed. Those inferences must be suspended so as to avoid the danger of serious misunderstanding of people’s intentions and motives; and, also, so as to open up ways of looking at familiar phenomena that are different from the ones that we use routinely ourselves, or those that follow from a particular theoretical perspective.

At the same time, there is also usually an emphasis within ethnographic work on developing an analytic understanding of people’s perspectives and activities, one that will usually be different from, and perhaps even in conflict with, how people studied see themselves and their world. There are several aspects to this. Ethnographers usually seek to take account of the perspectives of several categories or groups of actors involved in the situations they are studying, without treating any as automatically true, and especially without relying on conventional hierarchies of credibility (Becker 1967a). There is a recognition that there will usually be multiple perspectives, perhaps even in sharp conflict with one another on key points, and that all of these can be a source of insight.

Equally important, ethnographers will often take note of behaviour that is below people’s level of consciousness: in other words, which is routinely ignored or not even noticed for practical purposes. Ethnographers also often seek to locate what people do in a wider socio-historical context than they may be aware of themselves. And what drives these differences between participant and analytic understanding is that ethnographers are usually trying to understand people’s actions, and the social institutions in which these are implicated, in such a way as to contribute to academic knowledge about the social world, rather than to further the practical enterprises in which the people they study, or others, are involved.¹

¹ This is not to deny that ethnographic method is sometimes used in more ‘applied’ or ‘critical’ forms of research, and within practitioner inquiries, where this contrast may not be so sharp.
that, though practically effective for the most part, are false. It also stems from the
aim, already mentioned, of producing an analytic understanding of what is being studied,
rather than simply reproducing participant understandings. Sometimes, too, there is a
concern to avoid normalizing alien perspectives, to resist the tendency to assume that
other people's lives can be immediately comprehended in our terms.

Given this important tension within the ethnographic mentality, it is important to
note that, in recent years, there has been a recurrence of quite fundamental question-
ing about the very possibility of understanding other people. In the past, in the context
of historical work, this was formulated in terms of the question 'Is it necessary to be
Caesar in order to understand him?'. If this were a literal requirement, then, of course,
understanding others' actions would be impossible. While this question formulates
understanding in largely psychological terms, the same problem arises if one con-
strues it in cultural terms, asking how far it is possible genuinely to understand other
cultures. Those who assume that there is a common human nature underlying diverse
cultures can hope that this will facilitate such understanding. But many reject the idea
that there is any common human nature; and/or ask how we could ever produce an
account of its character that is not itself simply an expression of our own cultural
assumptions.

These issues take on particular significance in the context of a world where most
social scientists still belong to the West, and where Western societies are implicated
in a process of globalization that seems to entail the obliteration, or at least substantial
reconstruction, of other cultures. However, it is also an issue that arises within Western
societies, for example concerning how far men can understand women, or white middle-
class women can understand, say, poor Hispanic women, or whether heterosexuals can
understand gay, able-bodied people comprehend people with disabilities, and so on.

One, not uncommon, way of dramatizing this problem is to formulate it as the
problem of understanding 'the Other'. However, this misleadingly presents other people
as a single metaphysical entity. Furthermore, there is a sense in which any process of
understanding reduces the strange to the familiar, and we should not assume that this
necessarily involves distortion. Indeed, the very notion of 'distortion' may imply a
misleading conception of the goal of ethnography: as if its aim could be to capture
and represent cultures or other social phenomena 'in their own terms'. Yet, surely, all
knowledge, including that produced through ethnography, can only be about some
aspects of the phenomena studied: it is aimed at answering some particular set of
questions, rather than, literally, holding a mirror up to the world.

Can we ever get beyond our own cultural assumptions so as to achieve a proper
understanding of what is initially alien to us? Interestingly, this question can be extended
in an even more radical direction: Can we ever really claim to understand ourselves? After
all, even our understandings of ourselves will be constituted in and through our
particular background cultural assumptions. Others will see us differently, and who is
to say that there is any single fully comprehensive account that is true? Questions
running in the opposite direction include: do we need to get beyond all of our cultural
assumptions in order to be able to understand people belonging to other cultures; must
we assume that our background culture is a prison-house from which there is no escape?

These are difficult issues, but there seems no more reason to assume that all
understanding is impossible than there is to believe that anyone has direct, absolutely
valid knowledge about anything, even about themselves. We can recognize that social
inquiry is an uncertain business, in the sense that we can never be entirely sure about
the validity of any knowledge claims, without adopting a complete scepticism which
is, in any case, self-refuting - to say that we can know nothing is itself to make a
knowledge claim. We can recognize that, as ethnographers, we may be able to understand
others better than they can understand themselves, in certain respects, because we are
able to examine what they say and do in more detail than they do themselves, or
because we can see them in a wider context and compare them with others in ways
that, currently at least, they cannot. But, at the same time, we should not dismiss their
own understandings of themselves as fanciful or ideological, since they will have
access to some kinds of knowledge that are not available to us.

Equally difficult problems arise with the notion of analytic understanding. There
is sometimes a tendency within ethnography, and outside of it as well, to see any analytic
understanding as in some sense a cultural imposition, a form of symbolic violence;
and this is especially true if it conflicts with participants' own understandings. But
even if we put this aside, on the grounds that, as we have seen, 'understanding from
within' is itself problematic, we must ask: on what grounds do we choose one form
of analytic understanding over another? In the past, the answer to this would have
been: by appeal to the evidence. However, not only has there been increasing questioning
of what counts, and should count, as evidence, but it is also clear that many forms of
analytic understanding are incommensurable - not least because they are addressing
different questions and, as a result, rely on different presuppositions. This means that
evidence cannot easily be used to judge among them, since each requires evidence of
different kinds. Some commentators have concluded from this that there exist multiple
analytic perspectives that must all be equally valued, or which can be evaluated only
in terms of the particular purposes, or judged in aesthetic, political or ethical
terms rather than according to whether they represent the world accurately. Others,
however, see particular analytic approaches as providing us with important knowledge
about the world, even though no approach can tell us everything there is to know, and
even though what knowledge any study offers is always fallible. This is the position
we take.

The particular versus the general
There is a second set of important, and closely related, tensions. Ethnographers are
usually suspicious of rapid moves from statements about particular situations to broader
claims, whether in the form of generalizations about some large population or inferences
to some general theory. At the same time, while they usually study only a few cases,
and cases that are themselves relatively small in scale, they are also almost inevitably
concerned with drawing general conclusions of some kind. What is distinctive to their
orientation is that they want to do this in ways that still respect the particularity of the
cases they study.

Ethnographers differ in how they deal with this tension, and in how much thought
they give to it. Some seek, through the detailed study of particular instances, to develop
general, theoretical accounts of types of social phenomena that preserve particularity
as far as possible through the density of the analysis. An example is grounded theorizing
in which data are collected in ways that are specifically designed to generate theories
but where those theories are intended to capture all that is relevant in the particular
cases studied. Other ethnographers select cases for study in ways that they believe
enable them to draw general conclusions about some larger population, because these cases are typical or atypical in key respects, because they represent the leading edge of a changing trend, and so on. Yet others aim at producing thick descriptions whose general value is to be judged by readers when they use those descriptions to understand new situations in which they are interested or involved. At face value, ethnography is weak as a basis for producing general conclusions. As regards theoretical inferences, by contrast with experimental research it cannot physically control variables so as to reveal associations that are likely to indicate causal relations. Nor is it able to make use of the techniques for statistical control employed by survey researchers. And, in relation to empirical generalizations from a sample to an extent population, it is not usually able to employ statistical sampling theory, which often allows survey researchers to produce findings that have a high, and specifiable, probability of being representative of that population.

However, these various means employed by other forms of research to deal with the issue of generality have themselves been subjected to considerable criticism. In the case of the experimental method, there is the problem that what the subjects do is shaped by the experimental situation and by the experimenter, and therefore may not be indicative of what they would do in more normal circumstances. As regards survey research, the weakness of statistical control as against physical control is widely recognized, and there is the problem that very often the data on which the analysis is based consists of what people say in response to highly structured questions. Here, the problem arises of how reliable are inferences from such data to people's orientations and actions in ordinary contexts.

Of course, in everyday life, we all draw general conclusions from particular cases; albeit, if we are sensitive, tentatively. What ethnographers do builds on this. Moreover, it is often possible for ethnographers to engage in comparative analysis, so as to provide some evidence about what causes what. And examining the details of the processes by which things happen also provides indications about what causal mechanisms may be in play. Much the same is true of generalization from cases studied to some larger population. As Schofield (1990) has elaborated, there are means by which ethnographers can make reasonable judgements about this. Ethnography cannot provide a definitive solution to the problem of generalization, but then neither can any other form of social research.

Process versus structure

A third set of tensions arises because ethnographers tend to emphasize the processual character of human social life while yet at the same time seeking to generate theoretical ideas that, of necessity, involve ‘fixing’ the phenomena under study as belonging to particular categories. Stress on the importance of studying social life as a process arose, to a large extent, from opposition to attempts to understand it in terms of fixed, determinate relationships among variables, in the way that many quantitative researchers do. Thus, much general social science has been concerned with identifying key variables, measuring them, and then investigating what are taken to be stable associations among them, whether through experimental control or statistical analysis. And the aim has been then to produce causal models of these relationships that purport to hold true over time and across contexts. By contrast, ethnographers tend to emphasize the contingent character of human social life, the extent to which it does not take the form of repeated patterns or standard forms. They have drawn attention to how similar paths can lead to quite different destinations, and how very different paths may result in the same outcome. From their point of view, outcomes are not only never entirely predictable, but sometimes what happens is quite surprising, not being anticipated by anyone, and not even open to being captured in terms of some probability.

Emphasis on the processual character of human social life gives priority to detailed narratives, descriptions of the roles that various people play, accounts of the local contexts in which patterns of action occur, and so on. However, at the same time, ethnographers rarely wish to limit themselves to providing mere chronicles of events. Like historians, they usually want to interpret what is happening, to make sense of it and explain it. And, in order to do this, they cannot avoid employing analytic categories that presume general patterns. Indeed, the pressure to do this is even greater than it is in the case of historians, since the particular events ethnographers study do not usually carry much intrinsic interest for wider audiences, they are only of interest precisely as examples of more general categories of event. So, using such categories is essential if the research is to produce findings with any news value.

Once again, there is no simple, magic solution to this problem, for ethnographers or for anyone else. It remains the case, though, that ethnography serves as an important corrective to interpretations of social phenomena, in both everyday thinking and the social sciences, which assume that they can be understood in terms of universal or standard patterns of causal relationship, of whatever kind.

Discovery versus construction

The final tension we will discuss operates between a view of research as discovery and one which treats it as necessarily a matter of construction. On the first interpretation, ethnographers go into the field in order to find out what happens there so that they can document it. Indeed, often there is an emphasis on the value of ethnography in documenting what really goes on, behind official fronts. If, on the other hand, we interpret ethnography as a process of constructing accounts of the world, then we must acknowledge that, at least in some ways, what it produces will reflect the cultural background and the interpretative work of the ethnographer.

In the past, ethnographers generally saw their task as discovery, even while emphasizing the extent to which the people they study construct the social world through various sense-making practices and the courses of action these produce. By contrast, much ordinary social science, then as now, was seen by ethnographers as operating behind the screen of prior hypotheses and structured research instruments that prevented the researchers concerned from learning directly about the social world. It was argued that their background assumptions were being protected from the correction that would result if they were to engage in direct contact with the world. Ethnography was treated as an antidote to this: exposing researchers to the world and thereby enabling them to find out what really goes on and why.

However, in recent times some of the assumptions on which this commitment to discovery is based have come to be questioned by many ethnographers. The idea that it is possible to get into direct contact with the world has been challenged, on the grounds that any process of observation is assumption-laden: we cannot but make sense of what we experience in terms of some set of concepts, and we cannot derive those concepts simply from 'the things themselves'. Furthermore, in seeking to understand,
we are likely to reduce the strange to the familiar, tending to make sense of what we experience in terms of our existing categories, wherever possible. From this point of view, to talk of suspending one’s background assumptions, in order to allow direct contact with reality and discovery of the truth, can be seen as a form of bad faith; as falsely claiming some superior means of acquiring knowledge. It might also be argued that it ignores the extent to which what one finds, and even what counts as a ‘finding’ or a ‘discovery’, is itself determined by background cultural expectations; and is therefore to some degree an expression of the ethnographer’s individual and institutional self as much as it is a representation of the world.

More fundamentally, some of those who argue that what is involved is construction rather than discovery assume that there simply is no single reality which it is the task of researchers to represent. In other words, it is not only that we can never get access to reality, but also that the ‘reality’ that ethnographers document is no less a construction than the accounts produced by the people that they study. The very assumption that there is some single, available world in which we all live is rejected in favour of the idea that there are multiple realities. And yet, of course, to claim that there are multiple realities is itself to claim that one knows something about the multiplex character of reality.

In our view, it is impossible to do ethnography, or any other kind of research, without assuming that there is a reality that can be investigated and about which we can gain knowledge. However, ethnographers have always been acutely aware of the diverse cultural perspectives that exist within the social world, and of how these shape our understandings and actions. The principle of reflexivity, which we have emphasized throughout this book, underlines the fact that ethnographers themselves are part of the social world, and must work within whatever cultural perspectives are available to them. At the same time, this principle also insists that we can always learn to understand the world in new ways, and thereby come to better understandings of it. While we cannot prove in some absolute sense what is true and what false, and while there will sometimes be accounts amongst which it is hard to adjudicate with any confidence, this does not mean that just any account is as valid as any other.

Conclusion

We have suggested that much ethnographic research is guided by a distinctive analytic orientation; yet one which contains some important, unresolved, and perhaps irresolvable, tensions. We believe that these can be highly productive if they are held in balance. Of course, we do not wish to make exaggerated claims for the value of ethnography, as against other approaches to social research. Moreover, we acknowledge that its value is restricted to facilitating the production of knowledge: it is not an effective means for advocacy, empowerment, or bringing about socio-political change. But the value of the knowledge that ethnography can provide must not be underestimated; and there is a genuine danger of this in a world where the primary emphasis is on doing rather than knowing, and therefore on the rhetorical power of words rather than on their representational capacity. A reflexive ethnography implies a commitment to the value of understanding human social life; even while recognizing the limits to such understanding and to its power in the world.

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References


Index

Abell, J. 73

access 39–40, 41–62; approaching the field 43–9; covert research 53–8; gatekeepers 49–53, 58–60; interviewees 104, 105; negotiation of 4, 57, 61–2, 73

accountability 131, 227

accountants 129

action research 14, 32

actor-network theory 134, 167

Adler, P. 222, 224

Adler, P.A. 222, 224

advocacy 97, 229

Agar, M. 100, 194–5

age 76–8, 82

analogy 162–3

analytical induction 17, 186–8, 195, 197

analytic notes 150–1, 158, 163

analytic perspective 230, 231, 233, 236

Anderson, Elijah 44–5, 204–5

Anderson, Jon 64

anthropology 1, 2; ‘crisis of representation’ 203; ‘culture shock’ 81; grounded theorizing 166; non-literate cultures 121, 122; rhetoric 197; ritual 169; writing styles 202; written materials 122, 123

anti-realist 10–13, 14

Arensberg, C.M. 145–6

artefacts 121, 133–7

assumptions 29, 81, 231–2, 235–6

Atkinson, P., autoethnography 204–5; ‘culture of fragmentation’ 155; hermeneutologists 134, 143, 222; impression management 64, 68; informed consent 211; narratives 199; opera company ethnography 135, 169; personal feelings 151; research memorandum 27–8; spoken discourse 170

atrocity stories 164, 171, 226

audience 111, 113–14, 176–9, 201–3

audio-recording 127–4, 140, 141, 147–8, 150, 206

authenticity 49, 97, 136

authorial style 204

autobiography 124, 204–5

autoethnography 1, 204–5

‘awareness contexts’ 172–4

Back, L. 54, 220

Baez, B. 64

Bailey, F.G. 123

Ball, Mike 121

Ball, S. J. 52, 107, 127; case selection 33, 34; respondent validation 181, 182–3; temporal cycles 179

Barbera-Stein, L. 41–2

Barnes, J.A. 212

Barrett, R.A. 59, 61

Barten, A. 172, 173

 Bateson, William 186

Becker, H.S. 184, 200, 215, 219, 221; coding of data 155; familiarity 81–2; oral accounts 101, 177; ‘single study model’ 185

behaviourism 97

Bell, C. 210

Benjamin, A.F. 219

Bensman, J. 165–6, 214

Beoku-Bents, J. 87

Berlais, A.C. 35

Berremann, G. 58, 177

Bettelheim, B. 82

Beynon, J. 71–2, 217, 223

bias 98, 101, 177; documents 124, 130; social locations 181

bikers 45, 57, 66, 168, 216

biography 124, 137

biomedical research 226

block communities 50–1

Blackwood, E. 68

Bloom, B. 126–7

Bloor, M. 130, 181, 182

Blumer, H. 7, 164

Brams, F. 98

Bochner, A. 204

Bogdan, R. 51–2, 177

Bohannon, L. 80–1

Bohannon, P. 160