Researching Human Geography

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processes of fieldwork in the past (W. F. Whyte 1984). Fortunately these are being broken down through the work of theoretically engaged researchers who maintain a self-critical edge in their research. Methods, philosophy, theory and data are intimately interlinked and form part of the whole research process. As Sharon Macdonald (1997: 173) has articulated: 'By writing, we inevitably write ourselves into a particular and politicized context. The challenge is to disrupt easy positioning and to highlight the semantics and politics of representational practices themselves.' These ideas are just as pertinent for the materials examined in Chapter 7, where we explore research styles in which the researcher takes a more involved role in the everyday operations of the population under investigation.

7 Part of life: research as lived experience

An important element in making the method of ethnography less mysterious and more accessible to both practitioners and outsiders (e.g. funding agencies) is a more open and explicit discussion of how we, as ethnographers, come to know what we know.

(J. C. Johnson 1990: 15)

As part of the growing awareness that research processes do not merely describe social life but construct a particular 'reality', there has been considerable interest in the relationship between researchers and their research. In this chapter we discuss two methods that focus on this relationship – namely, participant observation and, one of its relations, Action Research. Participant observation differs from the detached observer studying a situation, because the observer is a player in the scene investigated. So far little participant observation research has been undertaken in geography, but this method has become of increasing interest. Taking a more active part in the research process, and producing effective research, requires real skill, indeed a number of important skills. In this chapter we discuss the merits of participant observation and explore the limitations of the approach.

Participant observation can be *overt*, as occurs when the observer makes known what he or she is doing, or *covert*, when the observer does not tell subjects they are being studied (see e.g. D. Rose 1987). The type of participant observation that is focused on in this chapter is *ethnography*. In this method the researcher tries to understand the world through the eyes of the participants in a social situation (organization, community and so on). To undertake this type of project, different approaches can be used, including *autoethnography*, which is an ethnography of ourselves, and *cyberethnography*, which is participant observation on the Internet, in chat rooms and so on. These approaches differ from Action Research, for which a key feature is that the researcher (and sometimes participants) seeks to alter a situation, by taking an active role. This method is action, rather than user, oriented.

The research methods discussed in this book so far have involved the researcher keeping a certain distance from the everyday lives of informants. In participant observation the researcher has personal involvement in the everyday. But the reduction in distance between researcher and researched creates challenges, alongside potentialities:

> *A methodology that offers little in the way of prescription to its practitioners and has no formula for judging the accuracy of its results is vulnerable to criticism from methodologies such as surveys, experiments and questionnaires that come equipped with a full armoury of evaluative techniques. In the face of these critiques the popularity of qualitative methodologies, including ethnography, is based on their strong appeal as ways of addressing the richness and complexity of social life. The emphasis on holism in ethnography gives it a persuasive attraction in dealing with complex and multi-faceted concepts like culture, as compared with the more reductive quantitative techniques. Ethnography is appealing for its depth of description and its lack of reliance on a priori hypotheses. It offers the promise of getting closer to understanding the ways in which people interpret the world and organize their lives. By contrast quantitative studies are deemed thin representations of isolated concepts imposed on the study by the researcher. (Hine 2000: 42).*

The reader might note slippage in the wordage used in the chapter so far. This slippage shifts from references to participant observation on to ethnography. The seeming collapsing of these terms should not be taken at face value, even though most commentators see the core element in ethnography as participant observation. As an outcome of this close association, reference to autoethnography or cyberethnography does not jar against images of the containers into which different research methods are placed. Referring to auto-participant-observation, by contrast, brings forth the kind of chill down the back that sharp nails inspire as they claw their way down a blackboard. Yet we need to be a little careful in equating the two. This is seen if we look at the roots of the word 'ethnography'. This is taken from the Greek word *ethno*, meaning people, and from *graphein*, meaning depict. As such, ethnography is in many ways as old as human curiosity. It reflects an approach to understanding behaviour that goes beyond observing while being a participant.[1] As Hammersley and Atkinson (1995: 1) remind us, ethnography is:

> *a particular method or set of methods which in its most characteristic form involves the ethnographer participating, overtly or covertly, in people's daily lives for an extended period of time, watching what happens, listening to what is said, asking questions – in fact, collecting whatever data are available to throw light on the issues that are the focus of the research.*

The emphasis on multiple methods creates a distinction between participant observation and ethnography. This is brought out in Hammersley and Atkinson's *Ethnography* (1995), in which participant observation is one of a multitude of methods reviewed under the

1 But note Jeffrey Johnson's observation (1990) that ethnography is an ambiguous term, as it is not clear whether it refers to an end product (detailed studies) or a process (method). We use it in the latter sense.

heading ethnography (see also Atkinson *et al.* 2001). In a sense, all the methods in this book can be involved in ethnography, with the caveat that participant observation should be a core feature. Critical for this chapter is grasping how the character and contribution of participant observation can deepen understanding of human societies. In seeking to provide that understanding, readers might find we are somewhat loose in the manner we glide between using 'ethnography' and 'participant observation'. The primary reason is that quality research using participant observation inevitably draws on other research methods, not simply in preparing for a period of participation but also in providing triangulation to strengthen validity claims (Chapter 2).

The tradition of participant observation research

The roots of contemporary research using participant observation are found in three veins: anthropology,[2] the Chicago School of Sociology and humanist geography. For a long period of time, participant observation was the main research method in anthropology. Undertaking studies of predominantly non-Western societies, anthropologists held that researchers had to immerse themselves in the culture or society they were studying (e.g. Barley 1983). The model was of the anthropologist going 'native'; living, eating and sleeping with the investigated population. Famous early monographs that used this method were Malinowski's *Argonauts of the Western Pacific* (1922) and *Coral gardens and their magic* (1935/1978) and Evans-Pritchard's studies of the Azande (1937) and the Nuer (1940). These studies were based on the researcher spending long periods of time living with subjects. According to Kuper (1983), the 'Malinowskian revolution'[3] united the fieldworker and the theorist, for the fieldworker travelled, looked and reported as well as analysed and interpreted. Travel often still plays an important part in distinguishing ethnographic approaches, even if many more investigations are of 'home' locations (e.g. D. Rose 1987; Rapport 1993).[4] Yet, as van Maanen (1988: 3) noted: 'Whether or not the field worker ever really does "get away" in a conceptual sense is becoming increasingly problematic.'

Participant observation was long sacrosanct in anthropology. Yet the power relations that underscored one portion of humanity selecting, valuing and collecting the 'pure' products and artefacts of others were not deconstructed, criticized nor transformed until relatively recently (D. Rose 1990: 38). Traditional approaches to anthropology were characterized by critical distance and emotional detachment from research outputs, the

2 Franz Boas's American 'salvage anthropology', with its emphasis on culture-history and the collection of texts and text-analogues, was a great influence on Carl Sauer and the Berkeley School of Landscape Morphology (cultural geography). This later impacted on the 'new' cultural geography (Skeels 1993). Boas's analysis of historical texts, folklore and oral history, alongside interviews and observations (sometimes with film), can be seen in the ethnographic practices of many cultural geographers today.

3 Rob Burgess (1984) notes that Malinowski first advocated direct observation using participant observation. Van Maanen (1988: 10) holds that *Argonauts of the Western Pacific* is the Genesis of the fieldworker's Bible.

4 Although early ethnographies did involve some studies of 'home' communities in the UK (e.g. A. D. Rees 1950; W. M. Williams 1956; Littlejohn 1963) and the USA (e.g. Park 1925; Lynd and Lynd 1929/1956; W. F. Whyte 1943).

research process, and those researched. Barley (1988: 9) offers a somewhat exaggerated account, but does bring out that investigators commonly failed to appreciate bias in their own 'authoritative' accounts:

> *Traditionally, anthropologists have written about other peoples in the form of academic monographs. The authors of these somewhat sere and austere volumes are omniscient and Olympian in their vision. Not only do they have a faculty of shrewd cultural insight superior to that of the 'natives' themselves, but they never make mistakes and they are never deceived by themselves or others. On the maps of alien culture they offer there are no dead ends. They have no emotional existence. They are never excited or depressed. Above all, they never like or dislike the people they are studying.*

Early accounts commonly gave the impression of 'telling it like it is'. Overtones of positivism are allied to a view of the anthropologist (or more broadly the ethnographer) as a skilled, trained observer who produces objective reports. This form of ethnographic enquiry and writing has been much criticized.[5]

The other two roots of ethnography, the Chicago School and humanist geography, were premised on an interactionist approach. These drew on a humanist theory of knowledge, experience and reality developed in the late nineteenth- and early twentieth-century writings of Pierce, Dewey and James. The pragmatic philosophy that underpins this perspective is concerned with the social construction of meaning; that is how meaning is constructed in the practicalities of everyday life (see S. Smith 1984). Pragmatists view knowledge construction as fallible. The challenge for pragmatists is to correct, as much as possible, this fallibility. In these terms, the research process is a form of knowledge construction. As such it is a fallible process. From this perspective knowledge is a trial-and-error process in which individuals make choices based on their attitudes and beliefs as they search for 'truth' (see Jackson and Smith 1984: 71–9; S. J. Smith 1984). Both in the urban sociology of the 1920s and 1930s Chicago School and in behavioural and humanist geography in the 1960s and 1970s, pragmatism played a significant part. For pragmatists, understanding is rooted in experience; as such it has to be inferred from situation and behaviour. Research attention thereby focuses on the interaction of individuals (society) rather than individuals as such.[6] By exploring the lives of people, analysts discover beliefs and attitudes about society (and everyday lives).

5 As Bell and Newby (1971) note, there has long been recognition of investigator influence on research conclusions, as commentaries comparing work by Redfield (1930/1973) and Lewis (1951) on Tepoztlan, Mexico, portray. But the absence of a strong epistemological and/or overtly theoretical base in early ethnography was a major flaw. Awareness of such shortcomings changed the nature of ethnographic studies. As van Maanen (1988: 5) notes: 'Exotic-mongering ethnographies of a remote but romantic wind-rustling-through-the-palm-trees kind are … out of favor these disenchanted days, replaced, by and large, with more focused, technical, cold and puzzle-solving varieties.'

6 This perspective is different from later humanist approaches, where the subject/individual is the focus.

For Susan Smith (1981) the Chicago School marked the inception of a genuinely 'humanistic methodology'. The Chicago School used ethnographic methods to undertake first-hand studies of city life. One of its founders, Park (1925: 15), appreciated the merits of the patient methods of field observation undertaken by anthropologists. Park and the Chicago School undertook numerous ethnographic studies of social relations and urban structure from Chicago's Gold Coast to the city's slums. Park studied the middle classes, ghetto-dwellers and hobos. Park's work reveals the pragmatist's view of history, where change is seen to be inevitable. Thus he viewed race relations as progressive and irreversible (Jackson 1984: 171): 'Customs regulations, immigration restrictions and racial barriers may slacken the tempo of the movement; may perhaps halt it altogether for a time; but cannot change its direction; cannot at any rate, reverse it' (Park 1950: 150). Park's perspective proved highly influential, with Chicago attracting high-calibre graduate students who undertook what for the time was highly original empirical research. As an illustration, Nels Anderson, one of Park's students, undertook a landmark participant observation study. In order to write *The hobo* (1923), Anderson lived with hobos for 12 months and collected over 60 life histories. This work revealed how understanding the language hobos used was an important and strategic part of interpreting their lives. Insights such as this came in abundance from the Chicago School, which introduced a series of approaches and beliefs that still influences contemporary ethnographic research (Box 7.1).

Box 7.1 Chicago School legacies for ethnographic research

Geographical research in the early twenty-first century utilizes ethnographic approaches in which the following legacies from the Chicago School can be found:

- the situational approach;
- the use of the personal document (letters, diaries, oral histories);
- the case study method;
- content analysis;
- the belief in the socially constructed nature of phenomena;
- the belief that people can make decisions about their own lives (they are not all predetermined);
- the idea that the observer (researcher) only gains partial insight, the whole truth is never revealed to a single observer;
- the importance of language.

Second-generation Chicago sociologists, such as Suttles, have also been influential. His *The social order of the slum* (1968) revealed the complex interplay of social order, ethnicity and territoriality. This was expanded in his *The social construction of communities* (1972), in which he sought to reveal how people build collective representations with communicative value. The reader just has to look at Kay Anderson and Fay Gale's text (1999) on the 'new' cultural geography to see how collective representations have a

communicative value. This understanding has been employed in classic geographical studies that employed participant observation and other methods to construct local ethnographies. Thus Ley showed how Philadelphia street gangs used graffiti to claim territory, with graffiti more prevalent and aggressive (communicative) at boundaries between gang turfs (Ley 1974; Ley and Cybriwsky 1974). As Jackson (1984: 179) rightly states: 'Discovering the social and moral order in such communities requires unusual patience, sensitivity and skill. For undertaking this task, the ethnographic tradition of the Chicago School provides a rich heritage and a fertile source of inspiration.' This emphasis on patience is illustrated by third-generation Chicago sociologists, such as Elijah Anderson (1978), who spent three years as a participant observer studying the interactions and exchanges that produced social order at Jelly's bar and liquor store in Chicago's South Side. The depth of human insight that can be derived from such investigations offers a key advantage for ethnographic approaches.

Such work was admired 'from a distance' by human geographers, but they were slow to adopt the approach. Even when humanistic geography sought to draw closer lines of understanding with ethnographic approaches, this was a cause of disappointment, Thus, in criticizing Ley and Samuels (1978), Prince (1980) argued that the work tended to chart spaces 'populated by identical automata and sociological marionettes', whereas Prince wanted the new humanism to study 'passionate, thoughtful, active men and women' (Prince 1980: 295); he sought deeper exploration of the agency of ordinary men and women. Such criticism was not restricted to humanistic geography. By the early 1970s so-called behavioural geography was also placing greater weight on human agency, but influential research under this umbrella carried a sustained ethos of positivism (see e.g. Cox and Golledge 1969). The universalist model of science that underpinned much behavioural work was rejected by those who engaged with humanist geography. Perhaps, as Prince hinted, this work was not well developed, but early humanist geographical studies did react against limited interest in the complexities of social and cultural processes. Leading from this, humanist geographers incorporated ethnographic methods into their research repertoire (see Ley 1988).

Box 7.2 Four types of 'participation' and 'observation'

Junker (1960) outlines four combinations of 'participant' and 'observer' for empirical investigations. These all involve participation and observation, but only participant as observer and complete participant are generally accepted to come under the umbrella of the participant observation method. The combinations Junker identified are:

- *The complete participant.* This is often associated with covert research. Some commentators see in this strategy the dual risks of 'going native' and of data collection being restricted owing to expectations associated with the role the researcher plays. In addition, for a full participant, writing up conclusions can create pressures not to reveal events or practices that cast the group or organization in a light others might see negatively. After all, as complete participant, the writer often continues to be involved in a group or organization after a write-up is in the public realm. Even publication after retirement (or death) can cause anguish for ongoing participants, as seen for noteworthy early publications in this genre – for example, Richard Crossman's Cabinet diaries (1975). Studies by geographers who were ongoing participants in processes written about include Blowers (1980) and Gilg and Kelly (1997).
- *The complete observer.* Here observation is prioritized, with social interaction avoided (for example, recording data from behind a one-way mirror). This method does not equate with participant observation, quite the reverse. Its strengths (and weaknesses) and its varied forms are outlined in Webb and associates (1966/2000), who provide a sweeping overview of non-reactive measures in social research. As one illustration, to assess the popularity of radio stations while avoiding the status biases of social surveys, record the stations that car radios are set to when they are brought in for servicing. Such information can aid participant observation by checking potential biases from uneven engagement with social groups. In its own right this strategy raises difficulties in 'interpreting' the significance of data, which can lead to problems of ethnocentrism (albeit even complete participation cannot remove this potentiality, as the actions of others still have to be interpreted).
- *The participant as observer.* This emphasizes participation and social interaction over observing. This is what commentators most commonly mean by 'participant observation'. Priority is given to participating in the everyday life of a group, event or organization, with data recording commonly undertaken 'under cover' (that is, not in front of other participants). In some cases the recording is deliberately in secret, for fear discovery will threaten the ability to continue participating in group activities or even result in violence towards the researcher (see e.g. Friedland and Nelkin 1971; Wilkinson 1981). In other cases, while informants might be aware of the researcher's purpose, recording observations when away from informants is seen to mitigate the intrusive influence of the researcher on 'normal' social activities (C. Bell 1977; M. Evans 1988; W. F. Whyte 1994).
- *The observer as participant.* Here the balance favours observation over participation. This can be associated with the utilization of a variety of data collection mechanisms (for example, in-depth interviews or focus groups). However, it is not unusual to find references in the literature to this kind of work as 'participant observation', when it is really observation with some participation. This sense is conveyed in Bernard's observation (1994: 140) that: 'At the extreme low end it is possible to do useful participant observation in just a few days.' With information collection commonly taking substantial allocations of time if the researcher adopts a participant-as-observer stance, many researchers are driven by preference or lack of time to sample events they observe (or engage with via interview and so on) or to focus on interpreting activities going on around them rather than in which they are active agents.

However, even today, there is criticism that attention to human agency has been inadequate in human geography. As one illustration, Jackson and Thrift (1995: 210) assert that the 'residual influence of Marxian political economy' in the study of shopping malls has led to a rather one-sided view of the mall as 'an essentially threatening presence, able to bend consumers to its will'. They argue research should focus on the active role of consumers in negotiating messages, meanings and lines of action in the mall, instead of seeing them more passively. After using ethnographic approaches to investigate shoppers, they found consumers were not passive agents, duped by advertising and commodities. As Jackson (1999: 402) put it: 'Landscapes of consumption are revealed as peopled by skilled and purposeful human beings whose behaviour is governed by a variety of moral concerns rather than solely by the cold economic logic of the marketplace.' That this point has broader resonance is exemplified by Thrift's answer (1997a) to the question 'what is place?' As Thrift notes, too commonly cultural geographers have looked at place as something 'to be animated by culture', as if place is made before it is lived in. By contrast, Thrift conceives of place as 'a part of us', as 'something that we constantly produce, with others as we go along' (Thrift 1997a: 196–7). These arguments embody a deep criticism of that band of cultural geography that complacently restricts itself to representation and signification. As Goss (1999: 48) argues concerning geographical studies of shopping malls, 'the ethnographic approach risks banality by reproducing the obvious finding that consumers make their own meanings, without engaging in positive or negative critique of the politics of meaning'. Lees (2001) takes up Thrift's call by driving this point further, noting that geographers should concern ourselves with the inhabitation of space as much as its signification. The implication is that the situated and everyday practices through which built environments are used needs to be better understood. Participant observation provides one way to explore how built environments produce *and* are produced by social practices within them, yet human geographers have been slow to recognize this. Despite regular appearances of the word 'ethnography' in the geographical literature, much under this heading is rather superficial. Especially when analysts limit investigation to issues of representation and signification, the sense is that ethnography is not seen as 'participant as observer' but as 'observer who happens to be participant' (see Box 7.2 on this distinction). Offering a stronger sense of omission, Thrift (2000a) calls for researchers to be *observant participants* rather than *participant observers*. In effect he calls for recognizing that representation and interpretation are insufficient, that greater emphasis on the materiality of human action is needed to move us towards theories of practice. This call has clear links with ideas on Action Research, yet it is grounded in an appreciation of the limitations of earlier participant observation research. In this Thrift and others are pointing toward a new critical ethnography.

Summary: the tradition of participant observation

- Participant observation is not the same as ethnography but is a core element of it. Ethnography includes multiple data sources.
- In anthropology the 'Malinowskian Revolution' united the fieldworker and theorist in one body, with a critical distance often maintained between observed and observer, and much work in 'foreign' lands.
- Work within 'home' environments was associated with the Chicago School of Sociology and humanist geography. Such work was slow to take off in geography, with contemporary criticism still levelled at undue emphasis on observing rather than participating.

Towards a critical ethnography

The roots of participant observation lay in white ethnographers entering 'foreign' cultures. From post-colonial and anti-racist viewpoints, this approach was problematical. It came to be challenged in the fall-out from decolonization (Katz 1992: 497) and growing recognition of the one-sided representations of former colonial societies (see e.g. Chakravarty 1989; Banthiya 1994). But in a global political economy with vast social inequalities it is not surprising that commentators are still concerned about bias in contemporary reports. Thus hooks (1991) has noted a continuing link between traditional ethnography and the 'new' ethnography when discussing the image on the front cover of Clifford and Marcus's book (1986). This shows a white male doing fieldwork in India, sitting at a distance from darker-skinned people 'located behind him'. For hooks (1991: 126–7):

> *As a script, this cover does not present any radical challenge to past constructions. It blatantly calls attention to two ideas that are quite fresh in the racist imagination: the notion of the white male as writer/authority, presented in the photograph actively producing, and the idea of the passive brown/black man who is doing nothing, merely looking on.*[7]

What hooks (1991: 124) draws attention to is the 'cultural context of white supremacy' that she sees as embedded in writings by white scholars about non-white people. She urges white scholars to interrogate critically their own positions, to realize the difficulties

[7] We have mixed views on this commentary. It embodies a powerful critique of ethnographic bias, but to extend this critique because of a book cover is less convincing. No evidence is provided to link the cover with the editors' views. Authors might be given little or no choice on book covers by publishers. If Clifford and Marcus provided the picture, hooks's criticism is valid. If they did not, the hooks critique might fit 'popular' (or publisher) images of anthropology but not the editors'. How important, then, is one book cover? Or one vision? (Examine S. Daniels 1992, and Kinsman 1995.) The front of Sanjek (1990) illustrates nothing except a male and a female (white) anthropologist writing at a desk 'in the field'. Does this signify anthropologists never see the people they write about – contriving a magical fiction from an enclosed verandah?

of translating ethnographies of the 'other'.[8] She illustrates these difficulties by outlining her grandmother's 'talk story' or philosophy of being and living:

> *One of her favourite sayings was 'play with a puppy he'll lick you in the mouth' ... These lectures were intended to emphasize the importance of distance, of not allowing folks to get close enough 'to get up in your face'. It was also about the danger of falsely assuming familiarity, about presuming to have knowledge of matters that had not been revealed. (hooks 1991: 23).*

An 'outsider' would probably have difficulty translating this. The point is that we are not always able to understand other 'languages', even if this involves the language we speak. In any translation, meaning is lost and invented. Despite the intensity of participant observation as a method, it cannot be assumed it reveals 'reality' as experienced by studied populations.

With such insights located at a time of significant challenge to dominant modes of thought in the social sciences, especially regarding claims that knowledge is independent of practices of knowing (see Chapter 1), the basis on which ethnography offers insight into human societies has been increasingly questioned:

> *Rather than being the records of objectively observed and pre-existing cultural objects, ethnographies have been reconceived as written and unavoidably constructed accounts of objects created through disciplinary practices and the ethnographer's embodied and reflexive engagement. (Hine 2000: 42; see also Dorst 1989)*

Denzin (1997: 3) has called this the 'triple crisis of representation, legitimation and praxis'. This triple crisis threatens ethnographers claims to authentic knowledge. As Marcus (1997: 399) noted:

> *Under the label first of 'postmodernism' and then of 'cultural studies', many scholars in the social sciences and humanities subjected themselves to a bracing critical self-examination of their habits of thought and work. This involved reconsiderations of the nature of representation, description, subjectivity, objectivity, even of the notions of 'society' and 'culture' themselves, as well as how scholars materialized objects of study and data about them to constitute the 'real' to which their work had been addressed.*

8 The implication is that white people should study white people, black people black people, and so on. Yet it is a long time since Dollard (1937/1957) found that trying to study the black population in a southern US town could not be separated from appreciating white actions and concepts of whiteness, both of which were critical to black people's self-imagery and behaviour. Multi-member research groups might help get around this problem, although this does not circumvent other weaknesses in the hooks argument, as discussed in Chapter 6 (and, for warnings and advice on team ethnography, see Erickson and Stull 1997).

The possibilities within the constraints/limitations of the ethnographic method are well demonstrated by van Maanen (1995), who offers a paper that students and researchers should benefit from reading. The message is that, although participant observation offers a prospect of deeper insight into the social fabric of a group or organization, the researcher should not forget he or she is at best a *marginal native* (Walsh 1998: 223).

hooks's critique should not be restricted to white researchers and non-white study populations, for the problematic applies as much when 'First World' investigators undertake research in the 'Third World' (Sidaway 1992), as it does when middle-class investigators study 'subordinate' cultures in their own country (Jackson 1989: 73). As explored in Chapter 6, the issue is not that 'others' are studied, for this is the heart of most research (if 'others' means people who are different from the researcher). The critical issue owes more to the embedded power relations that lie in representing the lives of others. This issue is raised by all research. Rapport (1993: 57) captures the mood, for an Englishman amongst the English:

> *as I unwittingly barged my way into the local social milieux and, in my eagerness to learn more names and news, exposed my ignorance of the manners of the people, I became more suspect. I was a 'mystery man', I had come from nowhere, without a family or a name, and yet I was English, or claimed to be, so why should I be so naive and odd? It was a challenge to their Englishness.*

As many postmodernists note, there is a bias in the background of researchers, who are commonly white, male and middle class, with disproportionately low ratios between researcher and researched for non-whites, the poor, women and so on. Research often involves those with more economic and cultural capital studying those with less (see Haraway 1988, 1989).

Recognition of this led critical researchers to engage more openly with their own positionality, while emphasizing the necessity for more reflexivity over the research process (see e.g. Box 7.3). This recognition has become embedded in more profound ways in researcher psyche than earlier calls, like that of Myrdal (1969), for the researcher to declare his or her values. Within a more reflexive research process, issues of researcher impact on study conclusions are now conceptualized and appreciated in more profound ways. Thus, while studies examining interviewer impacts on questionnaire responses have been conducted within a positivist framework for a long time (see e.g. Ferber and Wells 1952; or, more recently, Leal and Hess 1999), the postmodern challenge offers a deeper critique than technical deficiencies. In this regard, Clifford and Marcus's *Writing culture* (1986) was a significant marker in its attempt to redefine ethnography, to produce a more reflexive research style. *Writing culture* is a foundation text on the representational crisis in ethnography. Clifford and Marcus argued that ethnographies were not true or transparent representations of culture. They

Box 7.3 Positionality

As participant observers at a feminist geography workshop in New Zealand, Lees and Longhurst (1995) demonstrate the positions of Longhurst the insider (which is reworked as inside/embodied) and of Lees the outsider:

Insider (Inside/embodied)

'I want to discuss my inside-embodied-experiences rather than my "insider" experiences by way of my institutional positioning within the academy in Aotearoa/New Zealand … The brief round of introductions that evening did little to reduce the "knots" in my stomach as I realized that the participants had come to the workshop excited and with high expectations. I could not quite shake the feeling that I was somehow solely responsible for the success or otherwise of the unfolding events …'

Outsider

'a number of issues around the New Zealand feminist geography scene, and my own positionality, emerged as the conference opened with a greeting in Maori from Robin Peace … Robin suggested that her welcome in Maori might itself be a topic for later discussion since few Maori people were present at the workshop and the implications of her greeting Maori/Pakeha academics in Maori seemed complex. For me identity politics reared its head and I was unsure with what/whom I was identifying … So I decided to identify, and indeed position myself as the "other", the "outsider", non-Pakeha and non-Maori.'

critiqued and deconstructed the concept of culture, alongside the fact the anthropological project was/is built on implicit cultural comparison (see Marcus and Fisher 1986). Ethnographies to these authors were accounts, or what van Maanen (1995) calls stories, that rely on the experiences of researchers, as well as on how engaging or authoritative an author's writing is. Central to this view is the understanding that ethnographic stories are selective; or, as Paul Atkinson (1990) puts it, textual constructions of reality. In this regard, Geertz (1973/1975, 1983, 1988) questioned the *author*-ity of ethnographic accounts, whereby participant observer narratives laid claim to special insights that were inaccessible to others. Offering a thought-provoking pointer, Clifford (1986) charged that ethnography is not about representing cultures but inventing them. As Wolcott (1994: 15) expressed it:

> *Never forget that in your reporting, regardless of how faithful you attempt to be in describing what you observed, you are creating something that has never existed before. At best it can only be similar never exactly the same as what you observe.*

This point is now recognized to hold even when investigators focus on social groups who are 'similar' to themselves. In any translation from the observed to the written,

meaning is both lost and invented (Ely *et al.* 1997), which, as Keith (1992: 554) implied, reminds us of the responsibilities of the reporter and the ethics of the reporting process:

> *in the architecture of the academy there is an internal relation between researcher and researched in the production of power-knowledge that makes all ethnographic writing, in part, an act of betrayal … it is in the very nature of representation that misrepresentation occurs.*

This idea, alongside the term 'inventing' (or 'imagining'), is now common parlance in geography. Take, for example, Anderson and Gale's *Inventing places* (1991), Gregory's *Geographical imaginations* (1994) and Westwood and Williams's *Imagining cities* (1997).

The contentious nature of representing others, which such book titles imply, prompt the deeper question of who has the right to write about whom? Additionally, who are we writing for? These are fundamentally questions of *positionality* and *situatedness*. For geographers these questions are not new. They certainly extend beyond participant observation. Most evident in this regard, these issues were explored in radical geography in the early 1970s (Dickenson and Clarke 1972; D. W. Harvey 1973). One geographer who became aware of the need to reposition himself away from books and onto the street, to become more personally involved in the research process, was Bunge (1971; see also Merrifield 1995). But Bunge found it difficult to make the change, and questioned the ability of researchers to empathize with disadvantaged 'others'.[9] In particular he was concerned he could not locate himself outside his own past, his own upbringing. For Bunge, part of the research process was learning to cope with this.

Bunge's ideas did not capture the imagination of a broad spectrum of geographers, whose work continued to be grounded in then-dominant research philosophies. It required a Kuhnian (1970) type transformation for self-reflexive approaches to gain currency. This entailed acknowledging the limited capacity of researchers to empathize with 'others' and recognition that accounts were partial, not simply because informants reveal only some of themselves but because investigators' cultural baggage conditions interpretations and values allocated to them. As Katz (1994: 68) reminds us 'only in "The Wizard of Oz" do women descend on other lands without obvious cultural baggage' (see also Coffey 1999). This ethnographic recognition has drawn attention to the way Western, male, heterosexual, adult and able-bodied biases pervade studies of 'others'

9 More broadly Foddy (1993: 16) exposes why Geertz (1973/1975: 1988) is critical of the *verstehen* approach, which calls for investigators to 'empathize' with those they study: 'even if qualitative researchers honestly strive to "absorb" the culture (i.e. the organizing concepts and rules which govern perceptions and behaviour) of the member of a social group, in the final analysis they have either to infer or to guess the nature of the cultural elements.'

(on postmodern ethnography, see Denzin 1997).[10] This has led to a questioning of researchers' motives, as regards whether the benefits of investigations are primarily for themselves or for the people investigated. In this regard, most social scientists concur that research is not a non-exploitative process. Action Research and, more recently, the critical geography movement (see Blomley 1994; Castree 1999; Lees 1999), have sought to tackle these issues. In social science more generally, suggestions have grown on how to be more sensitive to power relations in research. Some suggestions include:

- framing research questions according to the desires of the oppressed group, doing work that others want and need (see Mascia-Lees *et al.* 1989). Overt efforts to move in this direction have come through participatory rural appraisal (Chambers 1994a, b, c);
- exposing 'colonizers', or more broadly those with power, who mistreat 'others' (J. Thomas 1993);
- studying our own cultures instead of other cultures (Bourdieu 1990);
- analysing our own practices, rather than those of others (Katz 1994);
- looking at global relations between peoples (Marcus and Fisher 1986);
- playing with the language (textuality) we use (Clifford and Marcus 1986).

As Katz (1992) argued, to counter oppression in the research process researchers must renegotiate and engage with the situatedness of their knowledge and positionality. This involves continual questioning of a researcher's social location (gender, class, ethnicity, age, abilities and so on) and of the physical location of the field area. For Katz (1992: 495):

> *As intellectuals operating in a postcolonial world, we must take seriously Spivak's admonition about representations as a staging of the world in a political context and begin to connect the 'micrological textures of power' with larger political-economic relations. In this expanded field, we can no longer valorize the concrete experience of oppressed peoples while remaining uncritical of our role as intellectuals. Neither can we presume to speak for or about peoples and nations as if they were outside of the contemporary world system, refusing to recognize that our ability to construct them as such is rooted in a larger system of domination.*

Going further, 'the ethnographic encounter is premised largely on the intent of the ethnographer to represent some constituted "other" to a particular audience' (Katz 1992: 496). Katz's solution is the excavation of 'a space of betweenness' (see also Anzaldua 1987). She explores this by trying to find connections between the lives of young people in rural Sudan and Harlem (her two field areas). From this work, she found 'striking parallels' between the two groups. Children in both experienced inadequate preparation for adulthood owing to socio-economic change. Through making this comparison, Katz offers an understanding of how capitalism affects social reproduction.

One solution that has been advanced to avoid the 'othering' of the subject of enquiry is autoethnography (see e.g. Anzaldua 1990). There are few examples of autoethnography in geography but Lakhani's Ph.D. research (2000) offers one example (for anthropology, see Okely and Callaway 1992). This study explored social constructions of identity amongst young British (South) Asians using participant observation. This involved engaging with young Asian people in a youth club, in employment contexts, on college courses, as well as in home, street and community locales. At a surface level the approach Lakhani used was not so different from those used in more traditional participant observation studies. A key difference that underscored both her research approach and the presentation of her conclusions was that Lakhani reflexively engaged with her own experiences as a young South Asian in Britain to inform and to underscore the interpretations presented.

A further mechanism through which researchers have sought to mitigate influences of uneven power relations arises in strategies for writing up research. Whatever the approach used, researchers inevitably employ textual strategies, and/or textual experimentation, to make their research appear plausible. When quantitative methods are employed, we see this in the utilization of statistical significance indicators, which give an air of authority to conclusions. Similarly, when Eagleton (1990: 384) discussed Foucault's use of a detached prose style, juxtaposed with the gross treatment of the body in *Discipline and punish*, he argued that this textual strategy (bordering on pornography) is used to convince readers of the validity of the arguments. Recognizing the links between authority claims and presentation styles, some self-reflexive researchers are experimenting with different textual strategies. The aim is to show that ethnographic texts are structured multi-vocal exchanges in politically charged situations. According to Katz (1992: 498), they have entered 'the hermeneutic or postmodern ethnographic project'. Some of these textual experiments are viewed as 'poetry'. As Tyler, (1986: 125) put it:

> *A post-modern ethnography is a cooperatively written text consisting of fragments of discourse intended to evoke in the minds of both reader and writer an emergent fantasy of a possible world of commonsense reality, and thus provoke an aesthetic integration that will have a therapeutic effect. It is, in a word, poetry.*

As Keith (1992) indicates, the nature of this reflexive poetry means the audience it is written for is of particular importance (see Fig. 1.2). Thus Keith has sought to write

10 Feminists point out that new ethnographers, such as Clifford, Marcus and Geertz, have been silent on women as 'other' (Strathern 1987). The postmodern turn in anthropology, it seems, has provided limited insight on gender or sexuality, as indicated by Clifford and Marcus (1986) including no feminist authors in their collection.

differently to avoid reinscribing racist representations in his work on race and racism. He has argued that

> *the texts through which cultural accounts present themselves are important and too often a neglected element of the research method ... An academic tradition of putative critical distance tied to methodological objectivity obscures, through mystification and naturalisation, the process of research itself. (Keith 1992: 556)*

We find this perspective worth exploring, but acknowledge there is no agreement on the advantages of such textual strategies. Thus, while some have praised the 'poetry' of textual experiments for their reflexivity, others critique them for being self-indulgent (Thrift 1996). But, whatever the perspective on how we *write* about our research, this should not stop us thinking about how we *do* that research.

The practical activity of *doing* research, and looking at how people *do* things, is the concern of geographers like Thrift (2000a, b) who are interested in moving researchers towards 'non-representational styles' of working (see Chapter 1) – that is, away from the obsession with representing representations. This movement in geography has entailed revisiting ideas on pragmatism, the idea that we only know through practice. Rooted in human subjectivity this movement is tied to the development of psychoanalytic geography, which focuses on the inaccessibility of the human unconscious (see Pile 1996). As a result of this 'non-representational turn', some ethnographers (although few as yet in geography) have adopted performances such as storytelling, dance, music and drama, by either themselves or those they are studying, to counter criticisms of ethnographic practices. Here the ethnographer concerns him or herself with practices that are not necessarily articulated linguistically. The ethnographer is not a participant observer but an observant participant.

Summary: toward a critical ethnography

- There has been critical self-evaluation of the products of participant observation studies. A 'triple crisis of representation, legitimation and praxis' has been linked to recognition that ethnography does not reveal 'reality' as experienced by studied populations.
- Strategies for the researcher to be situated in 'spaces of in-betweenness' have been encouraged, with autoethnography, textual strategies and 'non-representational styles' of working of note.

Approaching 'the field'

We are ready to enter the 'field'. Preparing to enter the field can be a big challenge for research that seeks to be sensitive to power relations and ethical issues. Yet fieldwork

training in ethnography has been and still is largely absent in the social sciences. Punch (1986) offers a variety of indications of this. These include the renowned Chicago anthropologist Everett Hughes telling fieldworkers to go and 'fly on their own', and the widely quoted 'training' offered to Evans-Pritchard, which was little more than to 'play it by ear' and be cautious about taking quinine and sexual relations (see also Bernard 1994). Perhaps of more help as a guide, even if suggesting somewhat Herculean a task, Malinowski is reported to have argued that everything seen or heard should be written down, as it is impossible at the start of a project to know what may be significant (D. Rose 1990), while Park is said to have told Nels Anderson to write things down like a newspaper reporter (van Maanen 1988). Today more advice can be given, but it is difficult to imagine training that is not deeply penetrated by a multitude of cautions and calls for flexibility, rather than precise instructions. The reality of participant observation is that the nature of the experience is difficult to predict (Hammersley and Atkinson 1995: 23). We noted this in Chapter 6 with regard to the unpredictable consequences of gaining access to social groups through 'gatekeepers' (J. C. Johnson 1990) and how personal attributes have uneven consequences for the research process (see e.g. Lewin and Leap 1996). As Rob Burgess (1984: 31) summarizes the situation: 'Accounts by researchers have revealed that social research is not just a question of neat procedures but a social process whereby interaction between researcher and researched will directly influence the course which the research programme takes.' In this context one has sympathy with van Maanen's view (1988: 139) that: 'On advice to students of fieldwork, my feelings are traditional. There is, alas, no better training than going out and trying one's hand at realist tales.' But if being flexible and sensitive means most researchers learn most through trial and error, this does not mean guidance cannot ease the learning process.

In this context, Cook and Crang (1995: 13–14) emphasize the importance of groundwork before data collection begins:

> *As a first step in any ethnography, it is important to develop early contacts in the organisation/industry/community in which you are interested, to find out what research may be possible within the constraints of access, time, mobility and money available for 'fieldwork', and to undertake methodological, theoretical and linguistic preparations accordingly.*

These considerations have commonly led researchers to 'familiar territory'. For example, the decision of one of the authors to investigate the meaning of urban community in Brooklyn Heights, New York,[11] was eased because the area is adjacent to a neighbourhood in which research had been conducted before. With contacts already there, a base existed for developing a network of new contacts.

11 As Katz (1994: 70) rightly notes, the act of 'choosing', deciding and travelling in such contexts overflows with the arrogance of research.

But personal knowledge of research sites is not always available. In such cases we can learn from the implied messages in Cook and Crang (1995: 15), who note that: 'Ethnographic projects do not emerge in the form of pristine hypotheses to be tested later "in the field" but require a fusion of knowing what is interesting and what is accessible.' Implicit in this message is recognition that there is a politics to gaining access to informants. This is why Cook and Crang (1995) suggest 'a combination of reading *and* doing' in preparation for fieldwork.[12] As Keith (1992: 553) reminds us: 'the context in which the research takes place is structured by moral, political and ethical considerations.' Such considerations require judgement prior to entry into the field, as well as when there. This is signified in his discussion on the problematic of access in research on racism in the police:

> *Several attempts on my part failed and I eventually succeeded because the family of a partner at that time came from a small mining town in Wales … and, it transpired that the partner was a cousin of a Deputy Assistant Commissioner of the Metropolitan Police. The officer concerned could not have been more helpful when I wrote to him. Yet the presentation of myself, the researcher, was mediated by a double link; quasi-family and an academic reference. My 'critical distance' and 'academic objectivity' were staked out from the outset, but the disclosure of 'self' was selective; several facets of my personal history were obscured and not even mentioned. Deceitful? Yes, certainly at one level. Defensible? I'm not sure, though neither am I sure what the disclosure of myself without dissembling would have looked like, short of an imperfect biography … the trust that was extended to me was considerable and the fact that the authorisation for fieldwork came from on high [made] access that much easier. In the hierarchical world of policing, my senior officer endorsement on several occasions conferred on a doctoral research project the appearance of an official investigation, at times radically altering some of the most finely balanced research relations. Clearly there were both gains and losses from this, but then it is hard to pretend that such status was not in and of itself deceitful. (Keith 1992: 554)*

As Newby (1977) asked, how many projects have been 'won' or 'lost' by luck in being associated with a key informant? How many researchers, as Howard (1994) reports for Nicaragua, need to make extra efforts to gain information from certain population segments because they were seen to be closely associated with another segment? How many, as Bell (1977) reports for a multi-investigator study of social life in Banbury, fail in this quest? There are no easy answers to such questions (for valuable insights, see J. C. Johnson 1990). But access will be influenced by the nature of a research project. Researchers who seek access to physical sites like a prison, a youth club or a library are likely to experience different degrees of difficulty. The same can be said for access to specific activities, such as a football match, movie watching or prostitution. Often unevenness is found in the same project, for the social milieu of an investigation can be a multitude of sites, like home, the street, a youth club, workplaces and so on (see e.g. Lakhani 2000). Each of these sites poses different access issues, which are informed by the personal attributes (and personality) of the researcher and the researched. In any event, making contact with people is inevitable. It is at this point that fieldwork 'formally' begins (albeit much preparatory work should have taken place previously to prepare for initial contact).

Recognize the need to be reflexive about 'doing fieldwork'. As one consideration, recognize that fieldwork has attained a status of its own historically. As James Duncan (1993: 42) put it, empirical investigation came to be called 'fieldwork … in order to professionalize it and thereby elevate its products above the representations of amateurs such as colonial administrators and travellers'. This point is taken further by Driver (2001: 12–13), who notes the significance of 'field' and 'fieldwork' in the development of geography as a discipline:

> *If we think of geographical knowledge as constituted through a range of embodied practices – travelling, seeing, collecting, recording, mapping, and narrating – the subject of fieldwork becomes difficult to escape. The field in this sense is not just 'there'; it is always in the process of being constructed, through both physical movement – passage through a country – and other sorts of cultural work in other places. It is produced locally by the spatial practices of fieldwork, and discursively through texts and images …*

Doing fieldwork encompasses a complex politics. This is discussed in an excellent 1994 special issue of the *Professional Geographer* on women in the field. In her opening remarks in this volume, Nast (1994: 54) argues that little attention has been given to how a 'field' is defined, nor to the politics of choosing and working in a particular 'field'. The authors in this volume are concerned that the 'field' is not naturalized as 'place' or 'people', but is located and defined in terms of political objectives, which cut across time and place. The feminist objectives of these authors embodied working towards critical, liberatory ends (not saving the exotic 'other') and overcoming shared experiences of oppression through racism, patriarchy and capitalism:

12 In preparatory work, practicalities should not be forgotten. For overseas research, a visa, immunization, foreign exchange and a local bank account are often essential (noting Barley's caution (1983) that local arrangements may not work). In addition there are personal concerns about health: 'I had water purifying tablets, remedies against two sorts of malaria, athlete's foot, suppurating ulcers and eyelids, amoebic dysentery, hay fever, sunburn, infestation by lice and ticks, seasickness and compulsive vomiting. Only, much, much later would I realize that I had forgotten the aspirins' (Barley 1988: 15). Do not let the amusing endnote distract here from the seriousness of health dangers. Even a glimpse at standard works on ethnography reveal regular references to Howell's survey (1990) of US fieldwork experiences, with frightening figures like 70% of fieldworkers in the South-East Asia region reporting liver disease and about 40% malaria (see e.g. Bernard 1994: 157). To this add that a quarter were accused of being a spy. Death after being attacked by someone from a study population is not unknown (Lee 1995: 3, 31).

> *This is not to say that we can 'move unheralded into just any field situation and become an effective part of its struggle for change just because we believe in its political ends' … There also needs to be a recognition that some historical and material realities are beyond our personal and social reach. Such reflexive analysis requires that we link larger-scale political objectives to smaller-scale methodological strategies which break down hierarchical objectivistic ways of knowing in the field. (Nast 1994: 58)*

The reflexivity such ethnographers seek involves 'processes of locating, reaching out to, and working within a particular field' (Nast 1994: 59). The field for these authors is a social terrain through which researchers can forge a bond between the academy (also a field) and the world at large. 'Thus fieldwork allows "fields" of everyday bodies and problems "out there" … to be incorporated into and thereby to subvert what has traditionally been the preserve of the white, the masculine, and the abstract – the ivory tower' (Nast 1994: 57). As Katz (1994: 72) argued: 'I am always, everywhere, in the "field".' Put another way, the participant observer is always positioned simultaneously in a number of fields, in a constant state of 'betweenness' (see e.g. Lewin and Leap 1996). In this context, in 'the field' the researcher is neither an insider nor an outsider in an absolute sense, but an interlocutor. As such, 'the field' is a place 'in between'. It is politically situated, contextualized and defined. Its social, political and spatial boundaries shift with changing social circumstances and in different political contexts.

Observer positioning in the research process

A decision ethnographers have to make about their role in the research process is whether to be overt, by explaining their research role to subjects, or to undertake research in a covert manner, by concealing their purpose (see Fyfe 1992: 131). Both covert and overt research are fraught with political, moral and ethical dilemmas. But covert research raises the potential for danger and violence (in both the metaphorical and actual sense of the word), as well as personal heartache. While totally covert research is rare in ethnography, the words of those who have undertaken such projects are poignant. One of the most vivid expositions is provided by Dan Rose (1987). Living undercover for two years as a car mechanic, Rose provides heart-felt diary entries at the end of his book. He also indicates the emotional pain of the negative experiences of his fieldwork:

> *The fact that I did not say explicitly that I was conducting anthropological fieldwork led rapidly to a disintegration of any assumption about what information I could gather. I feared that what was occurring was a complete lack of match up between what I had read in graduate school and entries in my field notes. At the time I could think of no greater anxiety. (D. Rose 1987: 13)*

Less traumatically, discomfort and loss of rapport resulted when Bax (1976) revealed he had used a hidden recorder to tape conversations during his fieldwork. But, despite some classic covert studies (e.g. Griffin 1962; Friedland and Nelkin 1971; Wilkinson 1981), it is more usual for researchers to tell at least some informants about their research. Many researchers argue that ethnographers should always be open about their research role, explaining their project to the people who are to be researched. Yet other ethnographers accept that it is not always possible, desirable or safe to outline one's position to everyone involved in the project. For one thing, as Hammersley and Atkinson (1995) argue, relaying too much information before the ethnography begins can introduce unwanted potential bias into a social situation, by influencing the behaviour of the people towards you (potentially by misinterpreting intentions). In addition, the nature of the 'lived experience' of ethnography means that many people on the margins of a project will be encountered during its passage (marginal in the sense of not being primary informants). If we stick to the stipulation that everyone has to be informed, this will require that every time researchers meet new people in study sites they tell them they are researching a particular topic (presumably this ethical stance would demand this be done before the person has a chance to speak, so they do not talk before being informed). One can imagine what effect this would have on primary informants. If they do not think you have lost a few screws, they will certainly become bored or embarrassed by the format spiel, with the potential result that they will avoid you or seek not to introduce you to new people.

There are no easy answers to these issues, for the context of the research can be critical. Thus Lee (1995) notes that many researchers see deception as permissible, if not laudable, in highly stratified, repressive and unequal social contexts. But, as Jackson (1983: 43) observed, whether circumstances favour overt or covert research is a question 'every social scientist must face personally'. In this regard we should not automatically class overt research as good (or better) and covert as bad (or worse):

> *Both research methods involve complex personal positionings (when 'doing research' and 'writing research'), differing 'explanations' of the self, immersion in local geographies, and observation and recording of ethnographic materials … Overt research is not assumed always to be free of covert practices, since much consensual participant observation involves the recording of social moments and processes, of which the research 'subjects' are often unaware. (Parr 1998: 29)*

In this regard Parr (1998) offers an interesting discussion of her covert research on mental-health problems. In this discussion she seeks to politicize the body as an ethnographic tool, including discussing how she acted and dressed:

At first, I would attend the drop-in in appropriate dress, and then be uncomfortably aware of how my hair smelt of shampoo. The people with whom I was sitting might not have washed their hair for months, and shampoo set me apart from the other people in the centre. In later visits, I learned not to wash before I arrived, perhaps not as a means of ensuring intimacy, but certainly in order to limit my own 'otherness' in that situation. The ambiguity of the body (how we adorn ourselves with soap, scent and deodorant to disguise real bodily odour) was highlighted in this research setting, and my body, in this sense, had to become much more physically present ... It was important that I walked without purpose and authority, also that I sat slumped, often staring into space for hours at a time, as this was the norm in the drop in ... (Parr 1998: 32)

Similar accounts can be found in a variety of social contexts. Notable examples include ethnographic studies of 'down and outs' and exploitative work environments (e.g. Brody 1971; Friedland and Nelkin 1971; Wilkinson 1981). Encapsulating what this can mean for a researcher, Punch (1986: 17) advised that:

Continued involvement in the field can be likened to being consistently on stage. The role has to be played without dropping your guard, and researchers frequently comment on the strain this causes, not only on themselves, but also on their families.

Capturing this sentiment, Batterbury (1994: 63) explains how:

During my fieldwork in Mossi communities of Burkina Faso, I was constantly involved in self-preservation and my conscious and unconscious actions – greetings and departures, body positioning, clothing and appearance, as well as general social conduct and behaviour, were closely observed.

Our cautions over all this are twofold. First, we emphasize the need to be prepared for the unexpected – be adaptable and anticipate needing to accommodate to situations you have not come across before. As Lewin and Leap (1996: 17) note, on gay/lesbian acceptance in new settings: 'The question of whether being lesbian or gay spans cultural boundaries must be answered anew in each situation.' Second, be prepared for hard work. Ethnographic accounts tend not to dwell on real research problems. This gives an un-real sense to some reports: 'One often has the feeling ... that reports on fieldwork experiences gloss over the problems, or treat them in a semi-comical manner' (Punch 1986: 14). The work of Barley (1983) comes to mind, although the forthright way he specifies research problems is commendable. Also of help, and an essential read for students who wish to

prepare for the emotional and physical discomfort of fieldwork, are writings on the difficulties of undertaking fieldwork (e.g. Howell 1990; J. C. Johnson 1990; Lee 1995), compilations of actual fieldwork experiences (e.g. Bell and Newby 1977; Panini 1991; Robson and Willis 1994; Lewin and Leap 1996) and autobiographical accounts of fieldwork living and learning (e.g. Malinowski 1967; Mead 1977; W. F. Whyte 1994).

A final thought on entering the field is to think through the implications of your research strategy and topic for the ethics of project implementation and exposure to potential

Box 7.4 A typology of ethnographic dangers

- *Legal danger.* This might be when a researcher enters a graffiti underground or marijuana-growing subculture. A dilemma is that deep involvement is vital for detailed understanding, yet an observational perspective is necessary to analyse and critique – to do critical ethnography. The solution is inevitably a personal one (with potential legal consequences likely to vary across countries). There are also differences across issues. Thus, the researcher of urban graffiti might choose to participate as fully as possible, as he or she sees any risk to be minor – perhaps with a short jail stay, fine or probation as the legal punishment. Participating fully would be more risky for the marijuana researcher, who might face a long prison sentence, as well as loss of employment and personal property.
- *Stigma danger.* This occurs when the researcher is the brunt of prejudice and discrimination owing to his or her identity or group affiliation. Critical feminist research on the sex industry is illustrative. Using participant observation, a female researcher of female prostitution will inevitably suffer the indignities of the whore stigma and the male gaze. This could even cross over from the world of participant observation into academia – where colleagues might react inappropriately or with disgust at the nature of the research. To overcome this the researcher must draw on emotional power, sense of humour and adventure (on issues a male researcher faces when researching female sex work, see Hubbard 1999).
- *Ethical danger.* Hamm and Ferrell (1998) cite the words of Ned Polsky, a criminological ethnographer: 'If one is effectively to study adult criminals in their natural settings, he [*sic*] must make the moral decision that in some ways he will break the law himself. He need not be a "participant" observer and commit the criminal acts under study, yet he has to witness such acts or be taken into confidence about them and not blow the whistle. That is the investigator has to decide that when necessary he will "obstruct justice" or have "guilty knowledge" or be an "accessory" before or after the fact, in the full legal sense of those terms.' This raises pointed ethical issues, such as maintaining the confidentiality of informants. For one thing, if the police request your field notes and interview tapes, what is your primary duty? As Lee (1993) outlines, there are documented cases of a subpoena being issued for researchers to hand over field notes. Responses to such requests (or demands) will no doubt be impacted on by the researcher's initial decision on whether to tell subjects that he or she was undertaking the study, as well as by the nature of the incident that provokes the call for releasing records and by personal values. Such potentialities bring into the open the myth of value-free social science.

Box 7.4

- *Emotional danger.* Developing emotional attachment to one's subject during the research process can be dangerous, as it can lead to emotional pain. If the reader wishes to grasp the potential for emotional disruption resulting from the research process, then Dan Rose's account (1987) of his participant observation study in Philadelphia is a good place to start.
- *Physical danger.* Take the researcher who immersed himself in a motorcycle gang and paid a heavy price. He immersed himself, not just in mind-altering substances with the gang, but in their survival instincts (this tactic was also used in research by Brody 1971). We join him as he zooms down the highway at over 100 miles an hour, the passengers close enough to touch one another across the roaring windblast: 'the experiential equivalent of a researcher shooting heroin in order to describe yet another ineffable state'. His risk paid off and he was accepted into the gang. But this acceptance came at great sacrifice: 'And with blood filling his punctured lungs, slipping into the darkness with broken bones protruding from his leather jacket', he brings us fully into the 'sheer terror of the ethnographic moment'. Included amongst those who undertook research in the anticipation that violence against themselves might accompany their work, see Friedland and Nelkin (1971) and Wilkinson (1981).

Source: We have drawn the topics and some of the examples here from Hamm and Ferrell (1998: 256–66). The reader is also recommended to look at Lee (1995) for a broader commentary on danger in fieldwork.

dangers. The decisions ethnographers make about participation are important in this regard (for examples, see I. Cook 1997). Researchers who engage in higher levels of participation run more risk of placing themselves in ethically difficult, if not potentially dangerous, situations (Box 7.4). This raises the question of how far researchers should involve themselves in actions that violate the law or take risks that lead to injury. There are no easy answers to such questions, but some social scientists do believe such steps can be necessary in order to understand groups whose behaviour has important public policy implications. For example, some researchers who study crime and deviance argue that we cannot understand the taste for danger, pleasure and excitement that is a part of crime and deviance without experiencing this ourselves. As a complementary thought, it can be argued that to understand cults we need to participate fully in their activities. Joining an urban street gang, an all night graffiti crew or the Ku Klux Klan might similarly expose the researcher to 'dangerous' situations, just as potential health risks should be acknowledged if you live as a 'down and out' (e.g. Brody 1971; Wilkinson 1981). Such research steps move the researcher's personal experience from the periphery to the centre of the research process. Moreover, these experiences can give rise to 'intellectual insights that blossom from the application of particular human survival skills in times of personal and methodological crisis' (Hamm and Ferrell 1998: 256). That most academics reveal their dual-identity as participant and observer in such situations is commonly exposed when those who have 'gone native' 'save themselves' by 'going academic' (see G. Armstrong 1993, as a football hooligan).

Summary: approaching the field

- Approaching the field is structured by moral, political, ethical and safety considerations, which need serious self-reflexive evaluation.
- You are always 'in the field', in a constant state of betweenness.
- Preparation for participant observation research requires serious attention to the practicalities of entering and undertaking research.

Data collection

Participant observation is a peculiar research approach, in that the researcher is the primary tool for collecting data. It is an approach to learning about the social and cultural life of communities, institutions and other social settings. It is social scientific and investigative and can be inductive. It emphasizes and builds on the perspectives of people in a research setting (both the researched and the researcher). When undertaken well, it employs rigorous methods of data collection, which try to avoid personal bias by questioning researcher values as interpretations are formulated and assessed, and as far as possible seeks to ensure the accuracy of information used (namely, cross-checking and questioning validity claims).[13] In the past, participant observation was seen as a tool that permitted researchers to learn about new situations from the perspective of 'insiders'. Some still see this as a key feature, given that researchers become involved in social settings and acquire knowledge through hands-on experience (LeCompte and Schensul 1999: p. xv). More recent accounts conceptualize the approach in a more refined manner. As Madge (1993) has put it, fieldwork is an interactive text that is created from dialogues, in which researcher and researched not only learn from one another but potentially change one another as the process evolves. As such, while perhaps starting as an 'outsider', as fieldwork progresses there is a transcending of 'insider'–'outsider' status. Unless subject to extraordinary bad luck or ineptness, the interpenetration of positions associated with being a participant who observes (not an observer who participates) should set the researcher apart from both 'insiders' and 'outsiders' – albeit the balance requires sensitive handling (Madge 1994). For Duncan Fuller (1999: 221), 'mixing and manipulating the researcher's various identities, and the transparent and overt recognition and awareness of multiple positionality (as person, academic, as activist) can … benefit geographical ethnographic research (and researchers) in three main ways'.

1. Transparency of thought and reflection, which improve the design, implementation and documentation of ethnographic research, should be promoted.

13 For Sanjek (1990: 395): 'Validity lies at the core of evaluating ethnography.' He lists the canons of validity as: (1) theoretical candour; (2) the ethnographer's path – a detailed portrait of what was done and how it was done; and (3) presenting field-note evidence.

2. Owing to this reflexive practice, the potential for a further layer of professional accountability should be added as the researcher reflects on his or her positioning and roles.
3. Critical engagement means researchers can play a greater role in social change, which links critical engagement to the potential for conducting Action Research.

Set in a context of critical self-reflection, learning and a 'fusing' of researcher and researched social environments, the investigator should continuously ask a series of principal questions. What is happening? When is it happening? Where is it happening? Who is (and is not) engaging in what kind of activities? How are people responding to what is happening? And so on. These questions are used as tools to obtain basic information about social events, social structure, cultural patterns and the meanings people give to these patterns. But to be effective participant observation has to go beyond being part of and observing social action. The dictates of conceptual validity point to the need for a triangulation of data sources, in order to challenge emergent interpretations and support claims that are made. As Susan Smith (1988: 22) reminds us, the intention 'is not, however, simply to discover the "rules" of the social game, but to become conversant with them to the extent that one feels safe angling for advantage and seeking privileged access to information'. For many this means drawing on extensive research methods in order that the researcher is confident when entering the more socially and culturally exacting environment of intensive research methods. In this combination of research techniques we find the conflation of 'participant observation' and 'ethnography'; good participant observation almost inevitably requires engaging with a bundle of other research methods. For one thing, it has long been established that, if researchers restrict themselves to participating in social activities without calling on other data forms, then the information that is gathered will be selective. As research for *Small town in mass society* revealed, despite efforts to be inclusive, participant observation gathers uneven information across social groups (Becker and Geer 1957; Vidich and Bensman 1968). Issues such as caste, ethnicity and sexuality, which were not part of the *Small town in mass society* investigation, can be expected to add to this potentiality (see e.g. Panini 1991; Mohammad 2001).

Prompts for participant observation

But, as study populations are often socially diverse, between groups, communities and organizations, let alone within them, and as efforts by researchers to be angelic pegs in local social scenes depends a great deal on the researcher him or herself, there can be no concrete step-by-step guide to participant observation or supporting data collection procedures. If the reader will indulge us to restate implied or actual messages in this chapter, participant observation is a method that necessitates flexibility, patience, a willingness to bite your tongue, a need to think before you speak, a potentially burdensome necessity to be charming (or at least to try to be) and a willingness to take an active part in activities you loath (a bit

Box 7.5 Bias, 'objectivity' and ethics in ethnography

The observant (or irritated) reader might have noted that we have referred to the need for unbiased data collection at several points in this book. We anticipate this might be provoking comments of the form, 'Hold on, what about the argument that "objectivity" is impossible, that we inevitably provide interpretations that draw on our own past experiences and by positionality?' This is a fair point, so we need to clarify what we mean. Our meaning is not that we believe that data can be collected in a value-free way, being capable of bad interpretation but incapable of inaccurate reading if 'approached' correctly. But this should not be taken to mean that researchers should wallow in their own value dispositions, seeking out and selectively reporting 'evidence' that 'conveniently' reinforces their existing prejudices. Look back at the introduction to Chapter 4, when we related the outcome of the David Irving libel trial, if you want a reminder of the slippery slope that this approach can lead you down; and remind yourself of your ethical duty to fellow researchers and the damage revelations about poor research can do for public confidence in the research community. What we mean by researchers seeking to be unbiased is embodied in investigators being conscious of their own value dispositions and engaging in a self-conscious process of seeking countervailing evidence to challenge the interpretations they are developing, in order to reassure themselves and their audience that what they report is not just pre-existing prejudices promoted under a halo of in-depth research (for example, calling on politically correct arguments, of whatever ilk, to gain reader support rather than convincing through offering argument and evidence). One aspect of this is to strive not to close down data collection avenues because researchers do not like the values or personalities of potential 'gatekeepers'. You might think the vicar is pompous, but, if your body language and manner of speaking to him make this clear, then you are likely to reduce his willingness to offer you insight into your research problem, as well as possibly ensuring that doors he could open for you will stay shut. We are not saying that this is easy, but no research is, if it is done properly. What you must never forget is that you are beholden to the people you collect information from. Most evidently, if you are living in a community, conducting interviews or in some other way imposing yourself on the time of others for data collection purposes, you are already beholden to them. For us, the ethics of this situation are both that you seek not to cause distress amongst those who are providing you with information and that you strive to the best of your ability to provide a high-quality research project, which does not simply trundle out the prejudices you came into the field with. In this regard, readers are well advised to read materials that draw out the ease with which ethnographic investigators reveal their existing prejudices and so close down potential insights. Accounts like those of Rapport (1993) and Michael Bell (1994), or many of the chapters in the Robson and Willis collection (1994), provide us with honest assessments of the problems pre-existing ideas and values have for fieldwork. You will not be able to eradicate these, but you can strive to minimize their effects. Quality research calls for approaching the field with a preparedness to change and challenge your existing ideas, not to act like a supercharged zealot determined to find class conflict, difference, distinction or discrimination in every social setting.

Our obvious caveat to all this will become clearer later in the chapter, for in Action Research deliberately calling the vicar pompous might be part of a coherent research strategy that is driven by a desire to bring about change as well as developing theorizations.

like eating over-cooked brussels sprouts with a smile on your face). This is not a research method everyone feels comfortable with (Box 7.5). We are not going to ease this situation by providing the equivalent of a Lonely Planet guide so readers can rush to do the same things. We are not going to try to cram the previous chapters of this book into the next few pages. We expect the reader to have grasped messages about the potential for participant observation to focus the researcher's attention on specific sub-populations. On this basis, we emphasize that participant observation studies can be enhanced by less intensive methods that cover a broader slice of a population. But rather than examining items like questionnaires, our intention below is to offer prompts on how to prepare for participant observation, alongside techniques we have not explored so far that might unlock insight. There are two essential elements in preparing to use these and other data collection methods. The first is timetabling (and the personnel who will execute them), for if more overt methods are employed early in a project this diminishes prospects of blending with a study population (albeit this is less likely if a researcher is not at 'home' with the study population). The second is how material will be written up. As a key thought here, we remind readers of Pollitt and associates' warning (1992: 60) of the need to convince readers of the validity of research outputs: 'it has been a weakness of some ethnographic projects that they have not been able to describe their own methods of observational enquiry in sufficient detail for internal validity to be assessed.'

Survey the community, social group or organization

It is useful to begin research by seeking basic information on the group or population to be studied. If the study is of a particular community, this could involve collecting information on age, gender, occupation, education composition and so on. These might be available in statistical publications, annual reports (of companies, local governments and so on), or official plans (such as land-use plans). Such information can be used later in your write-up as well as helping you get a feel for the community before starting your participant observing. Such written materials can be complemented with interviews with those not directly involved in the issues you study but who can offer insight on the study population. These might include planning officers, journalists, national NGOs and so on. But there is a caution to observe over such interviews. This is to treat the information collected critically. If a planning officer informs you that a locality has significant social divisions between one neighbourhood and another, this might be interesting to explore during data collection. Yet it also runs the risk of directing attention towards this issue and away from other, perhaps less overt but more fundamental social divisions. Such interviews provide prompts but you need to maintain a critical stance towards *all* the information you receive (this extends into the participant observation period). Making contact with people in key (knowledgeable) positions in an investigated community, group or organization offers an extension to these less immediate interviews. Here you are advised not to jump too soon. Preparatory work is critical before you make formal contact with someone who might prove to be a critical informant; indeed, it might be

that a formal interview is inappropriate with such people, depending on the nature of the research problem you are investigating.

Collect relevant documentary material

Relevant documentary material might be in the form of newspaper articles, minutes of local government meetings, and so on, but could include letters, diaries, films, photographs or audio tapes (for example, as found in the UK's National Museum of Photography, Film and Television at Bradford; see http://www.nmpft.org.uk/). Familiarity with such sources can give insight on previous practices, issues or events in an organization, group or community, as well as highlighting sensitive topics, potential research problems or 'local' peculiarities. Gaining information of this sort often involves visiting local (or national) archives. As one example, the London Metropolitan Archive offers access to films and tapes of radio programmes on London, with many taken from private companies, so films of working practices can be obtained. As part of the European Visual Archive, which is funded by DG-XIII of the European Commission, the Metropolitan Archive also collects and digitizes photographs of London from 1900 onwards (with 20 000 images to be displayed eventually on the World Wide Web). As the Metropolitan *Archive News Release* (4 (Summer 1999)) states:

> *The dream would be for a researcher interested in a particular subject and time span to have ready access via the Internet to images held in many different archives across Europe. Thus someone researching Docklands pre-1914 could locate images in Antwerp and London and compare installations regardless of language or national barriers*

Photocopies or notes from preparatory work should be retained, as their pertinence can be difficult to assess until you are writing up. The importance of documentary evidence in ethnographic research should be recognized, for they provide key insights in many substantial contributions (e.g. Geertz 1963; Douglass 1984).

Use a field notebook or diary

When you start to engage with your study population, the best way to record your ethnography is in a *field notebook or diary*, recording what you have observed, heard, smelt or physically felt. In doing so your observations will need to be shaped by theory, which, even if undeveloped initially, directs you towards social events or situations you need to record. Your notes will be of greater value if they are maintained with meticulous attention to detail. This requirement is especially pronounced at the outset, when interpretations of events and processes are more tentative. At this time the significance of particular observations can be lost on you. If you do not record them accurately in detail, your chances of picking up sequences, periodicities or co-variations

is diminished. Whether you employ an inductive research strategy or not, if you wish to make full use of the primary advantage of participant observation – detailed contextualization for interpreting human action – then consistently question the theoretical ideas that underpin your interpretations and guide the way you conceptualize what you observe. As Feldman (1995) observed, ethnographic interpretation tends to yield clusters of data that stick together, with uncertainty over how far these clusters are determined by researcher preconceptions or emanate from how investigated populations organize their culture. Researchers need to challenge their interpretations constantly, to test their interpretations by seeking events or processes that test the understanding they are developing (Glaser and Strauss 1967; Strauss and Corbin 1990). In doing this, observing settings and tracking events or sequences are two key forms of observation. The challenge for the ethnographer lies in the transformation of such observations into field notes. The more complete and accurate your field notes, the easier it is to catalogue and code your data. Field notes are best written on a regular basis and in a detailed fashion. You should begin to analyse your data early, in order to develop procedures for identifying patterns and connections over time and space, as well as across social contexts. Early analysis is also important to identify if there is bias in the social contexts you observe, which is a worrying potential distortion in participant observation studies (Vidich and Shapiro 1955; Becker and Geer 1957). Early analysis of your data should also be undertaken so you can engage in an iterative process of refining emergent interpretations against actual behaviour in appropriate 'test' situations.

On a more practical note, the suggestions of Schensul and associates (1999: 115) on making good field notes are appealing:

- Behaviour should be defined behaviourally, rather than in terms of what actions mean for the observer. For example, fidgeting with a pencil and keeping eyes downcast in a meeting may mean several things: boredom, disagreement, lack of understanding, anger, frustration, or preoccupation with another matter. Researchers should describe behaviour and avoid attributing meaning to it in field notes. Discovering what a particular mode of behaviour communicates to others in one setting might not be discernible until the same behaviour has been observed in a variety of other settings (and/or by many other people).
- Descriptions of a person should include details of appearance – clothing, shoes, carriage, items carried, and indicators of the status of material items. For example, rather than describing a person as 'poor and dishevelled', it would be more accurate to describe the person as 'dressed in blue jeans with shredded edges, an army jacket with dirty spots on the back and a torn collar, no belt, a white T-shirt with red smudges around the collar, and shoes with ripped edges, carrying a bulging backpack and a paper bag full of newspapers'. People at the farewell party for the local newspaper editor might be elegantly dressed, but unless the ethnographer described what they were wearing, we would have no idea of the meaning of elegance in that environment.

- The physical state of the environment should be described as if through the lens of a camera. A classroom might be described as bright and warm with a lot of visual stimulation, but readers would understand the description better if it is noted that:

> *... the walls are painted in warm shades of yellow and orange. Three of the four walls have collections of between 10 and 20 photographs, posters, children's drawings, and writing samples. Some of the writing samples are on coloured construction paper. Others are accompanied by colourful outlines, frames, or drawings. (Schensul et al. 1999: 115)*

As Schensul and associates (1999: 115–16) go on to suggest: 'Inferences and personal observations, reflections, hunches, and emotional reactions of the field researcher should be recorded separately from the stream of field notes that describes the event or situation'. In this context, the researcher might adjust to his or her particular needs a format for recording field notes that looks something like that in Box 7.6:

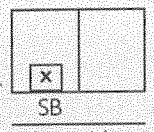
Box 7.6 A basic framework for recording field notes

Date/time:	
Location:	
Field researcher's observations:	Field researcher's comments:

A snippet from the research undertaken in the summer of 1998 by Lees on youth in public space in Portland, Maine, is provided in Box 7.7 (another illustration of field notes can be found in Cook and Crang 1995). Most ethnographers do not make notes when they are with informants. There might be opportunities to make some abbreviated notes (for example, during 'toilet breaks' or other short times on your own), but many researchers use a notebook or a small tape recorder to record material as soon as possible, when on their own (such as at their place of accommodation). It is important to transform abbreviated notes into lengthy descriptions as quickly as possible, as

Box 7.7 An entry from Lees's ethnographic notebook, Summer 1998

Date: August 6th 1998
Location: Tommy's Park
Notes:
There's at least three sets of kids hanging out in T's Pk today – one of these is a group of 3 homeless kids sitting on the wall opposite Starbucks under the tree. They look tired and withdrawn. I approached them to start a conversation but felt nervous – what on earth are they going to make of me. I plucked up the courage however and asked them if I could buy them coffee and donuts – and asked if they'd be prepared to talk to me. The middle guy (looked by age) said OK – he was wearing a black Metallica T-shirt and really baggy jeans with chains and a noticeable hoop nose ring. I went to



Comments:
Why did I pick the obviously homeless kids to talk to first? *Ethics* – is coffee and donuts enough recompense for their time – I'm annoyed there isn't much I can do for these guys.
Taping – they were not into being tape recorded – so I had to write down what they said afterward – have I got it all I wonder. I suppose

memory soon fades. If notes are word processed at this time, this should make them more amenable to coding and analysis.

What researchers should bear in mind when they write or type up notes is that field notes are the primary mechanism through which others can evaluate your research. Sanjek (1990: 400–1) is forthright in his criticism of past practice in this regard:

> *One who questions the validity of the historian's conclusions knows what places he or she decided to visit and which documents were used. The sceptic may then examine the historian's sources. And here the anthropology/history parallel ends (in most cases). Headnote evidence is manifested in the ethnography, but rarely are field notes open to anyone's inspection.*

For Sanjek, showing the relationship between field notes and the ethnography based on them is crucial for convincing accounts. In this regard, making field notes available to other researchers is critical for rigour in the research process.

Mapping socially significant patterns

As a cartographic exercise, mapping socially significant patterns is an underutilized technique, although it occurs regularly in Participatory Rural Appraisal (see e.g. Box 2.1). Another example of its use is Ley's mapping (1974) of graffiti and territoriality in a black inner city neighbourhood of Philadelphia, which illustrates an effective way of recording and (re)presenting your observations on a significant social marker. As Webb and

associates' commentary (1966/2000) on innovative ways to conduct unobtrusive data collection signifies, a vast array of markers can be mapped (or recorded) to demonstrate socially significant events, whether these be where empty heroin syringes are found or where particular people congregate at specific times of the day. Calling on documentary sources, mapping also illuminates social patterns, as with the home locations of couples married in local churches or the home locations of children attending particular schools. The researcher needs to write notes associated with a map, in order to explain conventions, ways of handling uncertainties and so on. Depending on their length, such notes might be better placed on the map itself or in the researcher's fieldbook.

Mapping images

Mapping images can be employed to get a handle on how informants (differentially) conceive of their social world. It is rare to find this technique employed in ethnographic studies. Yet the technique can confirm interpretations a researcher develops from verbal reports. In this regard, this technique might not be employed until the end of a project and might need careful explanation (even if informants were aware the researcher was investigating their community, group or organization). Perhaps the most well-known publication in this genre is Lynch's assessment (1960) of key landscape features in images of US cities. Another example is Eyles's maps (1968) of how residents demarcated the boundaries of Highgate village, with resulting drawings revealing ellipse-like shapes that (from whatever direction) generally encompassed their own home and the village core but failed to extend the boundary to a similar distance on the opposite side of the core from their home. More recently, Johnston and Valentine (1995) asked lesbians to draw their homes in their research on the performance and surveillance of lesbian identities in domestic environments. Although this mapping procedure has not been used extensively in geographical research, its potential is well recognized by Cook and Crang (1995: 73–5), who provide a useful summary of how to construct information from visual data:

- concentrate on the content of the image(s);
- treat the image(s) as a form of language so they can be interpreted according to the codes of representation they embody; and,
- identify patterns and regularities that provide insight into social processes.

Photography

As a record of life, photography has a long history in the social sciences (see Emmison and Smith 2000: 22–45). As one example, photography was used to explore the inner city in Riis's work (1971) on the Lower East Side. Photographs are not just useful for recording the present, nor should their use be thought of as limited to explorations of the past, for they can be valuable indicators of how landscapes have changed (presuming comparable photographs at different time-points exist). In drawing on photographs to

support participant observation work (or indeed for other research approaches), researchers can: (1) collect photographs from the locality that is being researched (on the analysis of existing images, see Emmison and Smith 2000: 46–50); (2) take photographs as part of their field notes; and/or (3) encourage those they are observing to take photographs – sometimes known as autophotography, which is often used in Action Research (see Schartz 1989). Issues of authorship and positionality will be different for these options. In terms of researcher assurance about the context and content of photographs, the most comforting option is (2), photographic ethnography. If this is used, the researcher needs to note the setting in which the photograph was taken, what interaction took place between those involved and so on. As Cook and Crang (1995: 68) point out:

> *Each photo is an act of (self)-presentation that involves the photographer, the photographed, and the expected audience. Both the people taking the photographs and those in them are already aware of the social contexts that will determine the meaning of their actions.*

This reminds us that the meaning of a photograph changes with viewing context. Thus, while photographs can be used in ethnography to find out more about a community, culture, situation and so on, they can also be used as part of a critical text. Baudrillard's *America* (1986) provides a good illustration. Baudrillard uses photographs, like Ronald Reagan's face and smile, juxtaposed with a chapter title to establish an ironic dialogue between text and image. What Baudrillard is seeking is the use of image to elaborate on the ideas in his book. But photographs are always embedded in a narrative context. For instance, if you take photos from your family album, you are likely to find that one photo can trigger discussion about family life at different times and places. Prosser and Schwartz (1998: 119–28) offer further discussion on photographs as part of data collection, as well as commentary on the analysis of photographs. More recently, Gillian Rose's *Visual methodologies* (2001) explains the methods available for reading visual culture, for interpreting visual objects.

Audio-visual techniques

Audio-visual techniques include filming, audio taping and video recording,[14] all of which provide an alternative to the written or drawn record. Audio-visual aids have the notable advantage of taking both visual and vocal acts seriously. Neither filming nor audio taping is new to ethnography, with traditional anthropologists like Franz Boas using them in work on native Canadians (his recordings can be seen in the Museum of Anthropology at the University of British Columbia). However, this tradition of 'film as science' has

14 For a practical handbook for researchers who wish to use text, image and sound in their investigations, see Bauer and Gaskell (2000).

given way to the idea of 'film as experience', which is valued for giving insight into the experience of being a participant in another culture. This audio-visual based ethnomethodology* has increased in sociology but has not gained the same popularity in geography. Given the focus of most ethnographers on 'interaction', the video recorder is particularly useful – in that both verbal and corporeal communication can be recorded. Audio-visual techniques are especially useful for the observation part of participant observation, and, given the prevalence of video recording, can often be employed without sending overt messages that the taping is for research. This does not mean the technique is not subject to 'distortion' from people knowing they are being filmed (would people who pull ridiculous faces at the camera at football games do the same if there was no camera?). Moreover, as the researcher (usually) operates the video camera, this can narrow his or her field of vision, with research events perhaps missed because they occurred out of the line of vision. The fact you record moving images and sounds does not mean your evidence has captured the 'reality' of social context, although it can offer a record of observed events that can be used to reveal your method of interpreting events (for example, for a Ph.D. thesis it could be seen by examiners). We should guard against a temptation to see film as an infallible notebook, so privileging the empirical and visual (Slater 1995). As Banks (1995) reminds us: 'while film, video and photography do stand in an indexical relationship to that which they represent, they are still representations of reality, not a direct encoding of it' (cited in Henley 1998: 42). This caution is especially noteworthy in the context of Crang's warning (1997: 368–9) that: 'Given the pressures to produce "scientific" material it is easy to see how the camera became invested with the weight of making the human, fallible process of observation "scientific".' This must be avoided, even if visual methods open up new windows for analysis.[15]

Audio-visual techniques introduce new analytical and ethical problems. We will not repeat the ethical issues raised in Chapter 6 here (see Box 6.7), but focus on the capacity of audio-visual taping to empower informants. This can be seen when researchers have given video cameras to individuals who do the filming. A good example occurred in Vancouver, where journalists working for the Canadian Broadcasting Corporation gave two street kids – Spice and Shorty – a video camera to make a diary of life on the streets (see Lees 1998). Titled *Home street home*, this video: (1) enabled these children to provide a personal representation of street life; (2) alleviated problems of voyeurism camera crews have often been criticized for; and (3) empowered (albeit temporarily) the

15 This point can be made generally for image-based research using CD-ROM technology and hypermedia software. These offer a practical way to explore relationships between text and images, for the researcher can quickly move between media and cover a large volume of information. However, care is needed over the tendency to see CD-ROMs as 'authorless aggregates of objective information which the user can wander over at will constructing his or her own narrative threads. In this sense, these CDs are more akin to an encyclopedia' than they are to an authored research text (Henley 1998: 55). Authorship and narrative need to be clearly established. Good use of multimedia in 'writing up' can be found in Walker and Lewis (1998) and on their website: http://www2.deakin.edu.au/hathaway/.

subjects under investigation. Readers who wish to consider the possibilities of using film might look to research papers based on this medium (e.g. G. Rose 1994; Lawrence 1998) or explore Cook and Crang (1995: 67–75) for a useful espousal and critique of filmic approaches. More broadly, Prosser (1998) offers a series of useful reflections on image-based research, Fiske and Hartley (1978) offer insights on 'reading' television and Gillian Rose (2001) discusses visual research methodologies.

Writing up

It might seem strange to include an entry on writing up in a list of ethnographic research methods. However, it is especially important that researchers self-critically examine their strategies for producing reports based on so intensive a research design. If we envisage writing up as a distinct stage in the research process, this implies a spatial separation between 'field' and 'home'. The first is the place where data were collected and the second where analysis is conducted and research written up. This framing of research seems to be associated with two types of writing (Gupta and Ferguson 1997: 12). In the 'field', there are field notes, with recordings as close to the research experience as possible, including raw documentation of surveys, questionnaires, interviews and observations. At 'home' the writing is polished, reflective, theoretically tied, intertextual, dare we say 'cooked' into journal articles, masters'/Ph.D. theses and research monographs. As Gupta and Ferguson (1997: 12; see also Clifford 1990) illustrate with reference to anthropology:

> *The former is done in isolation, sometimes on primitive equipment, in difficult conditions, with people talking or peering over one's shoulder; writing at home is done in the academy, in libraries or studies, surrounded by other texts, in the midst of theoretical conversation with others of one's kind. Moreover, the two forms of activity are not only distinct, but sequential: one commonly 'writes up' after coming back from 'the field'. Temporal succession therefore traces the natural sequence of sites that completes a spatial journey into Otherness.*

The spatial distanciation associated with such distinctions is linked to entry and exit narratives that have been seen to authenticate and authorize written material (M. L. Pratt 1986). This strategy was traditionally linked to an emphasis in ethnographic writing as objective, coming from a distanced observer. As Katz (1994: 67) explains, such artificial boundaries are commonly taken further:

> *between 'the research' and everyday life; between 'the fieldwork' and doing fieldwork; between 'the field' and not; between 'the scholar' and the subject … between recognition of the artificiality of the distinctions drawn between research and politics, the operations of research and the research … the researcher and the participant …*

Katz warns against constructing such synthetic separations, seeing a (field) site as a vessel holding certain attributes. 'Field' and 'home' are shifting locations rather than bounded spaces. The research process builds what Haraway (1988) calls 'web-like interconnections' between social and cultural locations (see also Gupta and Ferguson 1997: 39). Most evidently, if we undertake research in a self-reflective manner, in which our own positionalities, prejudices and comfortable associations are laid bare, then any distanciation from the rawness (realness) of 'the research process' introduces false distinctions. One reason for making this point is that, even in a post-'crisis of representation' time, when geographers are supposed to be more sensitive to the subjective nature of the research process, we find criticism of the clinical separation of 'field' and 'home' to be too silent. Researchers need to ask how they might engage with writing up 'in the field' (which is not always straightforward), how they can transpose the 'rawness' of what they encounter into the written word, and 'whether seeing fieldwork' as an integral part of everyday life changes their approach toward it, in ethically and theoretically positive ways.

Particularities of poorer countries

We do not wish to add to the list of considerations above, but it is pertinent to draw attention to a companion list associated with research in Third World settings. One reason for particular attention to these settings is because research by those from advanced economies heightens concerns about power inequities. Much of the commentary in this book focuses on settings in Europe and North America, with references to poorer nations providing a sense of the difficulties of generalizing about research methods, as well as drawing out similarities in dissimilar contexts. But only a short library search reveals that the research process is seen in a different light in countries with many poor people. One look at research methods books that focus on Third World settings confirms this impression (e.g. Bulmer and Warwick 1983; Dixon and Leach 1984; Casley and Lury 1987; Finsterbusch *et al.* 1990; Nichols 1991; Devereaux and Hoddinott 1992; Mikkelsen 1995). Much as early participant observation studies came to be associated with studies of poorer communities, so calls for research to empower local communities, to merge theory and practice, found early expression in less developed countries. Here, however, in a reversal of the previously common practice of engaging in lengthy, participant observation studies, analysts have sought quicker multi-faceted data collection methods (see Box 2.1). In doing so, researchers seek to be transparently honest about their intentions, involving the community as much as possible in data collection, while 'handing over the stick' to the community itself, so data can be employed for community benefit (Chambers 1994b). Initially instigated under the heading 'Rapid Rural Appraisal', a later change in title (and approach) to 'Participatory Rural Appraisal' reflects greater emphasis on placing people first.[16] Readers

16 Although now more common in academic research, it is poignant to note that Rapid Rural Appraisal came from the university world, whereas Participatory Rural Appraisal traces its roots to the NGO sector (Chambers 1994a).

might catch some similarities here with community profiling, which is commonly associated with poorer areas in advanced economies (Hawtin *et al.* 1994). Critical to such approaches is recognition of power relations in data collection, with weight placed on triangulation of research methods, to provide more valid information than formal surveys or participant observation alone (e.g., Buzzard 1990; Derman 1990). A key consideration is that the information gathered should be of value to the people in the research site; the researcher, as outsider, adopts more of a facilitator's than an investigator's role (Gow 1990; Honadle and Cooper 1990; Chambers 1994a). Through this researchers seek a more positive research contribution. Although based on different principles, precisely this idea underscores investigations adopting an Action Research framework.

Summary: data collection

- For an effective participant observation study, data collection needs to start before the researcher enters 'the field'.
- Multiple data sources have the particular advantages of tapping different perspectives on a study population, as well as encompassing different segments of that population.
- Data collection and analysis should always be undertaken with an eye on strategies for writing up.

Action Research

In Action Research it is the responsibility of the researcher to get involved in research that matters to the people affected by it. Action Research is often thought of as an effort to democratize research processes. The proclaimed aim is to see that those who are part of the research process benefit in some way from it, for Action Research is intended to be participatory and cooperative: 'Action research provides broad participation in the research process and supports action leading to a more just and satisfying situation for the stakeholders' (Greenwood and Levin 1998: 4). This kind of research practice is associated with a social change agenda. This can be tied to Marxist or radical geography (see *Antipode*), to feminist geography (see *Gender, Place and Culture*) and more recently to the critical geography movement, as allied to critical theory (see *ACME* – the new e-journal for the critical geography movement: http://www.acme-journal.org/). Viewed in this way, Action Research refers to the conjunction of three elements – research, participation and action (Fig. 7.1). As Action Research is proclaimed to take its lead position from the aim of democratization, the researcher must decide what is meant by democracy. If Action Research seeks to change society through the research process, this could be undertaken from a variety of political standpoints. One has only to think with horror of vile research practices perpetrated in Nazi Germany under the guise of betterment for society to realize the potential abuses of such a vision. In this context it is appropriate to be clear about the underlying vision of Action Research. Here the

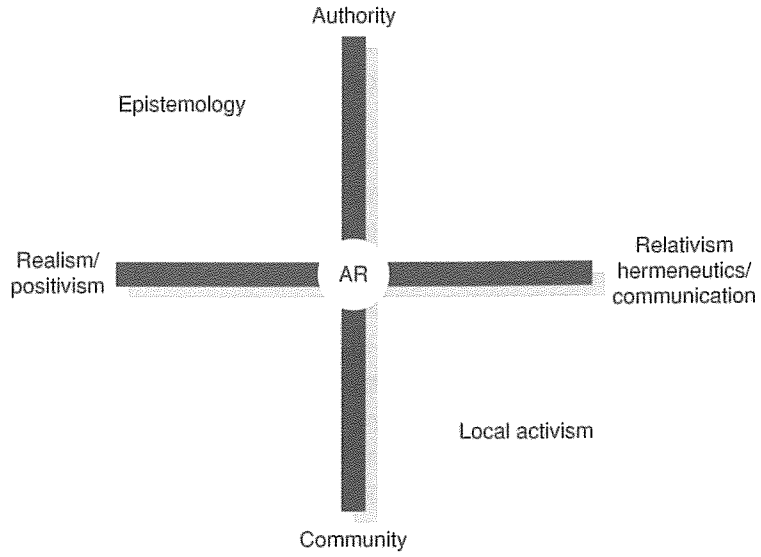


Figure 7.1 *A map of Action Research*
Source: Greenwood and Levin (1998: 90)

standpoint on democracy has been neatly captured by Iris Young (1990), who sees it as being associated with respect for diversity and the power of the disenfranchised to act on their own behalf.

The person generally thought to have coined the term Action Research is the social psychologist Kurt Lewin. Some of his slogans are worth repeating: 'Nothing is as practical as a good theory' and 'The best way to try to understand something is to try to change it'. In the 1930s and 1940s Lewin:

> *set the stage for knowledge production based on solving real life problems. From the outset, he created a new role for researchers and redefined criteria for judging the quality of an inquiry process. Lewin shifted the researcher's role from being a distant observer to involvement in concrete problem solving. The focus was/is on the world of experience, with its complexity, historicity, frictions and assumptions. The quality criteria he developed for judging a theory to be good focused on its ability to support practical problem solving in real-life situations. (Greenwood and Levin 1998: 19).*

Lewin's ideas were taken on board, post-war, by the Tavistock Institute of Human Relations in London. The Tavistock Institute (or Clinic, as it was then known) worked through psychoanalytic thinking and an action orientation. Amongst its outputs was the now famous study by Trist and Bamforth (1951) on the social and psychological consequences of longwall coal getting. This represented a break with conventional

Tayloristic approaches to work, where research focused on finding technically efficient ways to organize workers. It was associated with the industrial democracy movement as an alternative to the conventional hierarchical structure of organizations (see Pateman 1970).

Philosophically Action Research rejects the separation of thought and action, an assumption that has underlain social science research for some time. In doing so Action Research is not prescriptive on method. It can be quantitative or qualitative, using statistical analyses, social surveys, interviews, focus groups, ethnographies and life histories. It is as a form of in-depth participatory research that it fits the remit of this chapter. The aim of action researchers to overcome the separation of thought and action is mirrored by their aim to overcome the separation of science from action. Action researchers turn their backs on 'conventional' disengagement (whether intended or not) with the phenomena studied, equating this disengagement with naive assumptions about objectivity and impartiality. Instead, action researchers seek to narrow this distance as much as possible. They pursue constant interactions between thought (science) and action. This has led some to argue that this research style resembles practices in the physical sciences more than in 'conventional' social science (Greenwood and Levin 1998: 56).

The test of theory for action researchers is the capacity of the research process to solve problems, alter situations, aid social justice and transform power relations in the direction of greater democracy. Other research methods are perhaps less testing of theories that enframe their research design. The action researcher needs to have or learn certain skills – primarily they must know how to be a friendly outsider – for this external role is vital in opening up local groups to processes of change. They must be able to reflect findings about a group back to group members in a supportive, non-critical way. They must be expert (as with interviewers, focus group coordinators and ethnographers) at opening lines of discussion. At the same time the action researcher must enable people to access resources that could be utilized to bring about change. The action researcher must be self-confident in social situations without being arrogant, for people are good at picking up a person's sincerity. Playfulness and irony are said to be useful tools for the action researcher (more broadly the role of humour is explored by Rorty 1980). For Greenwood and Levin (1998: 107):

> *Someone who is unremittingly serious and dour and carries the burdens of the world on his or her shoulders energizes no one. Humor and playfulness have an important role in social change processes. This is because AR projects attempt to suspend business as usual and try to produce unlikely but positive outcomes. In these contexts the powers of irony, absurdity, and humor are considerable precisely because they cause ordinary thought to stop momentarily, creating juxtapositions that can provoke both amusement and openness to change.*

In Chapter 1 we pinpointed the key role of feminist social scientists in outlining the crisis of representation. Feminists have developed a strong commitment to articulate 'the view from below', to hearing the voice of the silenced (women) and to bring these henceforth to the table (see e.g. Townsend 1995). This is an intention that fits well with Action Research in the sense that is seeks to halt and turn around gendered silences (similarly, Marxist, neo/post-Marxist, radical and critical social scientists focus on class silences). The critiques of positivism, essentialism, oppression and the separation of theory and practice that are central to feminist research are central to Action Research as well. Viewing oppression as the norm, research practices are directed by a belief that the status quo must be overturned in favour of more liberating social relations/conditions. Feminist social scientist Patricia Maguire (1987) goes as far as to argue that feminist research cannot but move into the realm of Action Research (see also Reinharz 1992). We have sympathy with this view, although we add a rider. This is well articulated in the work of Catherine Riessman (1993: 80), who warns:

> *Qualitative researchers often seek to depict others' experiences but act as if representation is not a problem. Feminists, for example, emphasize 'giving voice' to previously silenced groups of women by describing the diversity of their experiences ... [but Riessman is more cautious] We cannot give voice, but we do hear voices that we record and interpret. Representational decisions cannot be avoided.*

The implication, that we should not forget, is that action researchers need to do more than give voice, for the remit of this approach is to bring about change; and there is a different politics to who reads academic literature, how it gets onto policy-making agendas, and whether or not it is listened to (Cantley 1992). Moreover, we should not forget that the voice that is 'given' by researchers can be penetrated by career considerations (on this problematic, see D. Rose 1990).

This second point is implicit in the conclusion of Kitchin and Hubbard (1999: 195) about research in human geography: 'it appears that many social and cultural geographers are happy to survey (and "map") the exclusionary landscape, but rarely do much to change that landscape apart from the occasional nod to "planning and policy recommendations".' For Kitchin and Hubbard the roots of this situation lie in the separation of the pristine 'ivory tower' and the messy world of the 'streets'. This distinction 'reinforces notions of modernist, rationalist science and seeks to maintain privileges attached to certain types of (academic) knowledge production over alternative ways of gaining understanding'. They further argue that Action Research may offer 'a route for geographers to combine a role of activist with that of putative academic' (Kitchin and Hubbard 1999: 196). However, this is not unproblematic, for the researcher/activist is often placed in a complicated 'third space' (see Routledge 1996). Perhaps linked to these problems Action Research has not made regular appearances in

geographers' toolboxes. Indeed, some work that has adopted this style has not led to publications in academic venues – even if work by Bill Bunge and Bob Colenutt has informed other outputs they have produced (e.g. Bunge 1971; Ambrose and Colenutt 1975; Merrifield 1995). Yet Action Research projects are being undertaken as Ph.D. theses. A geographical example of this was Louise Ackers's investigation (1985) of racism in the Labour Party, which involved active steps to increase anti-racist awareness, alongside deliberate, pointed interventions in party meetings aimed at making policies more race aware. Potentially, research like this can make a powerful contribution to social change. For a window on Action Research in contemporary geography, read the papers in Kitchin and Hubbard's special issue of *Area* (1999). These papers provide examples of Action Research and draw out the pitfalls of the method. A wider perspective on the approach can be found in William Foote Whyte (1990).

Summary: Action Research

- Action Research is associated with learning about society through efforts to change it.
- Changing society involves more than 'giving voice', which has uncertain capacities to bring about change. Geographers have so far shown more concern with 'giving voice' than generating change, although there have been advocates of Action Research within the discipline for some decades.

New field sites for ethnographers

So far we have been dealing with participant observation as a social process in which 'face-to-face' interaction occurs between the researcher and the researched. We now want to turn to a relatively new field site for ethnographers, which is not grounded in this kind of social interaction. This new option arises from the emergence and greater availability of access to the Internet. From this the prospect of undertaking *cyberethnography* or *virtual ethnography* is feasible. While traditional ethnography is associated with travel, often to foreign places, the Internet provides opportunities for social engagement without leaving 'home'. On the Internet, interaction is commonly faceless (Chapter 5). As the ethnographer can be at his or her desk whilst exploring the Internet, the relocation is experiential rather than physical. Cyber or virtual ethnographers do not need to get their hands dirty 'in the field'. They can sit in a sterile office entering the field in a matter of seconds. But such investigators do 'enter' the field, as they have to negotiate access. That said, when we consider the importance of face-to-face interaction in ethnography, we are drawn into considering the crisis of representation in the social sciences (Chapter 1), as brought into focus by the role of 'travel' in the construction of ethnographic authority vis-à-vis fieldwork.

The idea of 'the field' in ethnography has been analysed and critiqued in both anthropology and geography – the two disciplines in which 'fieldwork' is most central to professional and intellectual identities. The field is a space in which (other) culture is

waiting to be observed and written (Gupta and Ferguson 1997: 2). One of the issues with the traditional notion of fieldwork is how it fits a post-colonial, mobile, globalized world. As Appadurai (1996: 191, 196) points out:

> *As groups migrate, regroup in new locations, reconstruct their histories, and reconfigure their ethnic 'projects', the ethno in ethnography takes on a slippery, nonlocalized quality, to which the descriptive practices of anthropology will have to respond. The landscapes of group identity – the ethnoscapes – around the world are no longer familiar anthropological objects, insofar as groups are no longer tightly territorialized, spatially bounded, historically self conscious, or culturally homogenous … The task of ethnography now becomes the unraveling of a conundrum: what is the nature of locality, as a lived experience, in a globalized, deterritorialized world?*

The Internet seems to represent this (over-travelled) new world. An ethnography of, in, and through the Internet can thus be seen as an adaptive and partial approach that 'draws on connection rather than location in defining its object' (Hine 2000: 10).

Given that communicative interaction is the root of ethnographic research, what challenges do new technologies of communication pose for ethnographic research? How does the Internet force us to rethink traditional ways of studying society and culture? For some social scientists the Internet is a logical extension of modern society's obsession with control and rationality. For others the Internet is distinctive by virtue of its uncertainty. It is seen as the embodiment of a postmodern mode of *dis*organization 'characterized by the fragmentation of concepts such as science, religion, culture, society and the self' (Hine 2000: 6). From this perspective it heralds a new information society with possibilities of a radical social change (see Castells 2000), with social restructuring across space and time (for an espousal and critique of this viewpoint, see Lees 1997). On a methodology note, the Internet and other new communications technologies are implicated in the postmodern project, for they cast doubt on authenticity, representation and reality, the unitary self and the separation of self from society. Thus, Poster (1990: 1995) has argued that the Internet enables the decentring and dispersal of the subject, the blurring of boundaries between human and machine, between real and virtual. 'In the Internet postmodernity seems to have found its object, in an "anything goes" world where people and machines, truth and fiction, self and other seem to merge in a glorious blurring of boundaries' (Hine 2000: 7). On the Internet:

> *Communication can be synchronous or asynchronous, it can consist of private messages between known individuals or discussion among large numbers in relatively public forums, and it can be textual or audio or visual. Talking about 'the Internet' encompasses electronic mail (email), the World Wide Web (www), Usenet newsgroups, bulletin boards, Internet Relay Chat (IRC), Multi-User Domains (MUDS) and many other applications. (Kollock and Smith 1999, cited in Hine 2000: 2)*

Geographers can now undertake ethnographic research online. This can be achieved through participant observation in, for example, a 'chat room' or in virtual communities.[17] Using the Internet in ethnographic research draws attention to specific research questions (Box 7.8). Yet the relative absence of ethnographic research on the Internet makes for difficulties in articulating experiences and good practice, even if some points have been rehearsed in arguments against fieldwork-based ethnographies in favour of written and visual representations (see e.g. Dorst 1989). We recommend that before embarking on Internet-based research, especially if this is seen as a step towards a virtual ethnography, be sure to read Hine (2000). Her informative ten principles of virtual ethnography, and especially her ethnographic enquiry into the Louise Woodward law case, are particularly noteworthy (Hine 2000: 63–5).

The Internet has already become a powerful research tool. Although researchers might be cautious until more evaluations of its limitations and strengths for ethnography have been undertaken, the potential for reaching new, especially geographically dispersed audiences, is appealing. However, to achieve high-quality research using the Internet, researchers will have to approach their task in a rigorous manner, critically evaluating whether this is the right, as opposed to an easy, medium through which to obtain data. As those who already receive a lot of e-mail questionnaires will be aware, there is a real possibility of informants tiring of being inundated with calls to participate in online research. Here again the responsibility of researchers towards those who follow needs stressing. As we have indicated often in this book, poorly thought-out projects have the potential to do great harm.

Box 7.8 Research questions raised by virtual ethnography

- How do the users of the Internet understand its capacities? What significance does its use have for them? How do they understand its capabilities as a medium of communication, and whom do they perceive their audience to be?
- How does the Internet affect the organization of social relationships in time and space? Is this different from the ways in which 'real life' is organized, and, if so, how do users reconcile the two?
- What are the implications of the Internet for authenticity and authority? How are identities performed and experienced, and how is authenticity judged?
- Is 'the virtual' experienced as radically different from and separate from 'the real'? Is there a boundary between online and offline?

Source: Hine (2000: 8).

17 Mann and Stewart (2000) discuss collecting data through participant observation in virtual communities (along with virtual focus groups, online interviewing, etc.).

Summary: new 'field' sites

- The Internet is providing access to geographically dispersed, specialist populations, that allow new insight into social behaviour.

Analysing data collected from participant observation

For the ethnographer primary data come in *written form* (text from a field notebook, field audio tapes or an audio-visual recording) or in *visual form* (photos, video images, film, drawings, paintings, maps). Some ethnographers may choose to include another category – namely, music (or more broadly sound). We have not dealt with the analysis of sound in this book, but for a good example refer to Susan Smith (1997) on the soundscape of brass band music (see also S. J. Smith 2000). To analyse written and visual data it has not been our experience that different approaches are necessarily needed. For many of the methods ethnographers use, it is possible to adapt principles and practices to analyse both visual and written data, provided the investigator remembers that different data types are being used.

Central to such analyses is the requirement that researchers do not fall into the 'theory' versus 'practice' binary that feminist geographers have long criticized (see Sayer 1989, 1991 on British geography). This is a binary that Bordo (1986) terms 'the Cartesian masculinization of thought' – that is, the idea that masculinity has been associated with mind, rationality and reason, and femininity with body, irrationality and unreason (see G. Rose 1993 on Anglo-American geography). Indicating the specificities of such ideas, Berg (1994) finds that, in New Zealand geography, this binary is reversed: theory is gendered feminine and empirical research masculine. But it is not the gendering of a binary that is important. What is critical is the fact that empirical investigations are already theoretical, 'just as our theories are infused with experience' (Berg 1994: 254). As Geertz (1988: 129–30) put it:

> *However far from the groves of academe anthropologists seek their subjects – a shelved beach in Polynesia, a charred plateau in Amazonia; Akabo, Meknes, Panther Burn – they write their accounts with the world of lecturns, libraries, blackboards, and seminars all about them. This is the world that produces anthropologists, that licenses them to do the kind of work they do, and within which the kind of work they do must find a place if it is to count as worth attention.*

The reader may wish to replace lectum with lecture theatre and the 'place' of academia with a degree. Whatever, the point is that you cannot falsely construct a dividing line between fieldwork and brainwork. To summarize: we should explicitly acknowledge that

the articulation between theory and empirical investigation involves flows in both directions – 'empirical investigations' are always already theoretical, just as 'theory' is always touched by our empirical experience. It is also necessary to point out, however, that this does not mean that theorizing and doing empirical research are identical activities. (Sayer 1991)

Box 7.9 Analysing ethnographic data using computer software

For field notes of say 50–100 pages, we suggest researchers code text manually, sorting it by hand. For larger studies, we suggest the researcher(s) enter the ethnographic data into a computer text management program. Most of these will read WordPerfect or MicroSoft Word. For the main part the use of computers does little more than increase the speed of retrieval of information from a large volume of transcripts. Software may, however, further assist by helping develop more refined coding schemes and counting the number of times something occurs (namely, content analysis).

It is certainly worth thinking about the rationales that geographers have put forward in urging us to analyse qualitative data using computer software packages. Crang and associates (1997: 772) point out that the academic boom in computer-aided qualitative data analysis software involves '*both* a quantitative expansion *and* a constriction in prior freedoms'. It returns us to those old 'skirmishes between more freeform "interpretation" and scientized "analysis" ' (Crang *et al.* 1997: 773). Using the computer to analyse qualitative data can be seen as one way that researchers have given otherwise subjective data an aura of objectivity, dare we say an aura of rationality, standardization and order. This is not something that we subscribe to, for using a computer software package inevitably creates distance between yourself and the data you have collected (for a sensible critique of tactile versus digital in the context of ethnographic research, see Hinchcliffe *et al.* 1997). Whether you decide to use such computer packages to analyse your data depends a lot on how close a distance you wish to have to your data and research. It also depends on the amount of time you are prepared to invest in learning to use these packages effectively. Low-tech paper-based solutions should not be discarded out of hand! One positive aspect of the digital solution, however, is the reduction of field experiences. We have all at one time or another sought to put too much material into an ethnographic account. We have all had the task of telling students that they have to be selective, that owing to dissertation word limits they must be selective in using qualitative data. Software programs can alleviate some of the pain involved in this. Another positive aspect of such programmes is that they help us avoid mining material for the most 'choice' (or 'juicy' as some would have it) quotations – a process that can introduce a dimension of bias into any analysis.

Most qualitative data analysis software packages code text by categorizing segments according to topics derived from the subjects or theoretical approach of the researcher. The main differences between software packages are in the ways they categorize material. Researchers need to be aware of these differences and of 'their specific possibilities in specific research contexts' (Crang *et al.* 1997: 772). We also need to be aware of the approach to interpretation that we wish to take (see previous section);

Crang and colleagues (1997) identify three: (1) theoretically selective analysis – illustration through 'informant quotation'; (2) analytically inductive analysis or grounded theory – involving categorizing and coding statements to demonstrate the reasoning behind actions, attitudes or beliefs; and (3) formal structural analysis – which focuses on narrative form, tropes, metaphors and metonymity in order to examine structures of a text that generate meaning.

The software packages available have been overwhelmingly focused on the written word, but this is changing fast with developments in graphics, interactivity and increased focus on the WWW. In what follows we do not offer a thorough insight into each software package; rather we outline those we have found most useful to date.

ATLAS.ti. This can be used for the visual analysis of text, audio or video qualitative data. It is especially good for content analysis. It helps the researcher to code and annotate significant elements of a text. It enables you to build networks that visually connect passages and codes to help with the exploration of the meanings embedded in your data. Here images are incorporated as primary material with codes attached to them and links to other text or images. This is an especially useful package for those researchers who have focused on visual data collection.

*QSR NUD*IST* (Non-numerical Unstructured Data Indexing Searching and Theory-building). This supports various methodologies from survey data to focus groups to Action Research. It is a good package for a research team to collate their efforts. Its main added value is that it links ideas and supposedly constructs theory (for criticism of its theory building, see Crang *et al.* 1997: 778). Basically NUD*IST allows the researcher to do the following:

- process texts such as interviews, focus groups, field notes, letter extracts, historical documents, and so on;
- code the data on screen and explore all coding and documents interactively;
- add comments or memos to documents and codes;
- search text(s) for words, phrases or patterns and code the results;
- search indexed categories so as to explore and/or identify complex relationships or patterns;
- create categories of data that are either structured (hierarchical) or unstructured;
- export results into concept mapping software, such as Inspiration or Decision Explorer.

Kitchin and Tate (2000: ch 9) provide a good step-by-step introduction to using QSR NUD*IST. They also include a practice run that can be downloaded from the Pearson Education website (http://www.awl-he.com/). Gahan and Hannibal (1997) also offer a practical guide for using QSR NUD*IST.

HyperRESEARCH enables the researcher to work with both text and multimedia data. Its features include the following:

- Autocode – assigns a code looking for multiple words or phrases.
- Hypothesis tester – presents your data in a similar way to a statistical analysis.

- Code Map – this diagram tool enables you to display your data graphically and graphically explore relationships between codes.
- The ability to use less structured notes.
- Allows the creation of diagrams and the creation of graphics.
- It is good for content analysis.

Ethnograph. This software package has not been discussed in great detail in geography methods texts. Ethnograph is a program for the analysis of textual data. Like NUD*IST, it is primarily an instrument that files information systematically, enabling the researcher to sift and manipulate without having to trawl through reams of paper. Unlike NUD*IST, which is designed for multi-researcher and larger projects, Ethnograph is designed for building complex categories and codings from a smaller set of materials. Amongst the advantages of Ethnograph, the following might be noted:

- If you type in your ethnographic field notes, Ethnograph will number every line of the transcript. You can then identify that particular lines are about a certain topic and assign code words.
- Ethnograph will allow you to search for segments of text according to whether code words overlap.
- Ethnograph will allow you to search for an interviewee by means of interviewee identifiers.
- Details about an interviewee, such as their age, gender, ethnicity, etc., can be attached to each interviewee identifier.

Read Seidel and associates (1995) for a user's guide to Ethnograph.

Textbase Alpha and *HyperQual* are slightly different software packages in that they 'have explicit functions to integrate a range of different materials; they both allow the precoding of prestructured data, such as structured interview or questionnaire responses. Hence it is possible to do preliminary sorting of answers to specific questions, as well as incorporate freeform materials such as interviews or fieldnotes' (Crang *et al.* 1997: 778).

As we have regularly stated, when undertaking ethnographic research there is a need to analyse as you gather your data, which is a practice that provides an overt integration of theory and data collection. Indeed, from our experience, from the minute a researcher collects data, analysis tends to kick into gear, even if this is unintentional. But, while this coalescing might seem to ease the task of analysis, commentators regularly make clear that the analysis of ethnographic information is not straightforward. Wolcott (1994) offers one vision of this, when holding that the real mystique of research comes not from data gathering but from its analysis. As Lee Harvey (1990) points out, one reason for this is that ethnographic research tends to yield a mammoth quantity of information that has to be sifted in analysis.

This is one reason why computer software packages are becoming more popular as data analysis aids (Box 7.9). But such aids, like the theory that has been developed or

underscores a project, simply help with the process of bringing materials together (see e.g. Fielding and Lee 1991). The issue of what is selected and what is relegated during analysis and writing-up cannot be articulated in a series of self-evident steps. That analysis should not be expected to be easy is made clear by Dan Rose (1987). Here, in vivid terms, he reveals how the intensity of real-life participant observation led him to question theories and assumptions in the academic literature. In particular, reflecting on the central analytical notion of an event or situation, Dan Rose (1987: 29) found that these commonly employed units of analysis failed to reflect the reality of the lives he studied. They distorted rather than revealed the temporal grounds upon which social life took place. Such a thought-provoking critique of the basic building blocks of so much analysis draws out real difficulties in writing up research. Yet we have sought to show in this book that analysts are not restricted in the ways they examine data. For example, in Chapter 4 we examined the relative merits and shortcomings of content analysis, semiotics and discourse analysis in investigating meaning and key messages in texts. As Box 7.9 shows, these approaches have been introduced into computer software packages for analysing ethnographic data. Similarly, in Chapter 6 we explored the capacity of conversation analysis to provide insight on human beliefs and rationalities. Such procedures are the substance of ethnographic work, which, as Geertz (1973/1975) has argued, is visibly directed toward the interpretation of culture. Yet we note that ethnographic research also focuses on the materiality of social life – not so much with meaning and culture as with the raw exertion of power and processes that create social discriminations and inequalities. Here, as our passage from William Foote Whyte (1984) in Chapter 4 revealed, attention can centre more on what happened than on what it meant. Classic studies that have adopted participant observation strategies to draw out such insights include the work of Dollard (1937/1957) and Griffin (1962) on racial discrimination or Dahl (1961) on the exercise of power in US city politics.

For all these methods the reader will find a substantial number of books and papers that offer specific advice. As various commentators make clear (e.g. Plummer 1983; Barnes and Duncan 1992; Riessman 1993; Ely *et al.* 1997), a regular failing in these accounts is the scant regard they give to writing up research. Our agreement with this has been made apparent on numerous occasions, in making clear that writing creates a 'fiction' that does not exist 'in the real world'. Our writing is a distorted abbreviation of a particular interpretation of the world. Precisely this vision has underscored the social constructivist approach that has been written about in this chapter, for, as Wolcott (1994: 16) reminds us, 'data are tainted with an analytical or interpretive cast in the very process of becoming data', with the same colouring process extending into the translation of 'data' into a research report. But in our awareness of this constructivist process we are informed by a coalescing of viewpoints. This arises from one of the valued products of engaging in multi-author work, for the three authors of this book would position themselves at somewhat different points along hypothetical scales that distinguish whether their main interests are with materiality or meaning (acknowledging

the falseness of implying that this is a binary divide) and that specify the research tools they are most comfortable using. This has prompted an interchange of ideas that has provoked learning and self-reflection. Fortunately in this regard we have shared a common sense that attention to questions of representation in human geography have often been troubled by a watering-down of materiality issues. In this regard it is worth indicating for those who feel more comfortable focusing on material concerns that this stance is not inconsistent with learning from the serious critiques social constructivism raises. The fact that writing-up creates a 'fiction' does not mean central elements to this fiction will not be accepted by most analysts – at least if they are viewing the social world you write about from the perspective you adopt. If the social constructivist argument is taken too literally, it can leave the impression that research accounts are unique. This impression is unfortunate. Social constructivism has much to offer those who are mainly concerned with materiality, not just in providing a powerful message that critical self-reflectivity is needed in data gathering, analysis and presentation, but in destabilizing notions of a single truth. Insight from another keyhole might produce a different emphasis and understanding. This should be a welcome addition to understanding – something to prompt reassessment of one's own work, not a threat to it.

But let us finish where we could have begun. For many who are reading this chapter the idea of undertaking participant observation research might seem a frightening prospect. Certainly we would encourage you to approach the method with caution, for it is not easy and needs to be eased into, so that you learn from experience. But if you have not undertaken ethnographic research before, how might you start to learn and then hone your skills? Our suggestion is that you choose different social settings, well before you wish to enter the field using this method. Undertake mini-projects in them. Why not start by having a coffee at Starbucks (or, if you are opposed to the McDonaldization of society, try a local coffee shop where people sit and read or talk for a long time). Perhaps compare this with a pub or bar. Seek to record your observations about what is happening in the settings you select. Do this with a view to trying to reconstruct the social fabric of the place after the event. Ask yourself what the people are doing, how the room is laid out, what signs there are of social relationships and of social differentiation. Hold back from trying to interpret what you 'see' when you are there – practise recording. Do this for a number of occasions in each of the place settings you select. Do not assume the social world that inhabits a location is uniform across the hours of the day or the days of a week (and so on). Think through what makes social situations different (and similar) from one another. A key factor in appreciating events might be consistency over time, but it is just as likely to be inconsistency. When you have completed each session, write up your notes as fully as possible but leave analysis to one side until you have completed all the sessions you want to observe (as you practice more, try to analyse as you go on). Now try to interpret what you saw. Seek to do this not from your own perspective but from that of the people observed. The key in approaching this as a learning exercise is to identify the difficulties you have in this

reconstruction. Are your notes inadequate – did you forget to record information about people, so you cannot be sure whether people of one ethnic background engaged equally with others or only with those of a particular class or gender (or)? Are your difficulties linked more to a sense of theoretical shortcoming? Did you record more when social activity was high, so you missed a great deal when it seemed boring? Contrast your notes and interpretations on different settings (and at different times) so you get an idea of whether your notes are too descriptive or not focused enough. Keep asking yourself why am I finding this difficult? Go back and try to observe (and record) the issues you are having difficulty with. Make a presentation to colleagues. Do you convince them? Ask them to critique your account – get them to keep asking 'but how do you come to that conclusion'. Their prompts will probably be different from yours and should help you think in new ways. Practice will not make you perfect. But done effectively it should allow you to enter the field with more confidence and should enable you to undertake higher-quality research. Confidence is important, for first impressions amongst informants can be critical for project success. This means preparatory work is critical.

Aversion to ethnography in geography, according to Herbert (2000), comes from three major criticisms; it is unscientific, too limited to enable generalization and fails to consider its inherent representational practices. In this chapter we have exposed the limitations of such critiques. We agree with Herbert (2000: 564) that 'Geography's neglect of ethnography diminishes the discipline'. It is a different method from the surveys and interviews we outlined in previous chapters because it looks at what people *do*, as well as what people *say*. It is an approach that exposes meaning through action, not just words, and as such is associated with various methods like participant observation, Action Research and more recently performativity. It is a method that is particularly self-conscious about the process of interpretation. It pushes us as researchers to be explicit about how we interpret and the politics therein. It highlights that the dilemma of interpretation is unavoidable, for interpretative practices and subjectivity are central to all social scientific methodologies. We will leave this discussion with the words of David Ley (1988: 126):

> *The geographer's charge to interpret the complex relations of people and place requires a methodology of engagement not detachment, of informal dialogue as well as formal documentation. There is both an ontological and epistemological requirement that place as a human construction be granted more respect and complexity than the profile it displays from the pages of the census.*