

**Exercise 14.1**

Below is an extract of around 300 words from my book  
*Discourses of Counselling: HIV Counselling as Social Interaction*.

- 1 Read the passage and make notes from it (no more than 100 words) appropriate to a thesis on the nature of professional-client communication.
- 2 Now repeat the process on the assumption that your thesis topic is 'effective AIDS counselling'.
- 3 What relevance, if any, does this extract have to your own research? Note that such relevance can be methodological and theoretical as well as substantive. This means that a reading can be useful even if your substantive topic is very different.

Three major points have emerged from this discussion of a small number of post-test counselling interviews. First, following Peräkylä (1995), 'cautiousness' is seen, once more, to be a major feature of HIV counselling. This is true of the activities of both counsellors and clients. Thus, these counsellors seek to align their clients to the disclosure of their test-result, while clients, to whom the character of counselling is presumably 'opaque', often demur at taking any action which might demand an immediate telling of their test-result (or indeed, many other activities, like directly demanding clarification of the validity of HIV-tests) even when, as here, given the right to decide the agenda of their counselling interview. However, these agenda-offers, unlike the alignment strategies discussed by Maynard (1991) and Bergmann (1992), are being used in an environment where the upcoming diagnosis is likely to be heard as 'good'.

Second, we have seen how, when clients respond to agenda-offers by introducing other topics than the test-result (e.g. volunteering statements about themselves or asking, usually indirectly, about the validity of the HIV-test), they seem to 'kick in' standard counselling responses (e.g. information and requests for specification). While such responses are consonant with normative standards of good counselling practice, they are, once again, produced in an environment in which their positioning (prior to the telling of the test-result) may be problematic.

Finally, we have demonstrated that, for at least one client, this delay in telling is problematic. As Ex 7 (and its continuations) showed, this client analysed the delay in the delivery of his test-result as implying that C was about to deliver a 'positive' result – by referring to 'support groups' for HIV-positive people.

<sup>38</sup>This apparent lack of fit between a delayed delivery of the test-result and its content (i.e. as HIV-negative) leads directly into some fairly clear practical implications. (Silverman, 1997a: 106)

## 15

**Relations in the Field**

Two French qualitative sociologists have recently reminded us of the close association between ethnographic research and a sense of the studied world as a 'field' with emergent properties. As they put it:

Ethnographic studies are carried out to satisfy three simultaneous requirements associated with the study of human activities:

- (i) the need for an empirical approach
- (ii) the need to remain open to elements that cannot be codified at the time of the study
- (iii) a concern for grounding the phenomena observed in the field. (Baszanger and Dodier, 1997: 8)

A whole series of practical questions about fieldwork now arise:

what is the status of this 'specific' context in which the study takes place? How is it described? How is this framework delineated, since it is not a here-and-now situation, nor a situation in which mankind as a whole is characterized through the fundamental properties of every one of its activities? (1997: 11)

However, Baszanger and Dodier recognize that this concern for explaining observed activities in terms of social context does not apply to all qualitative research:

this reference to field experience nevertheless distinguishes ethnographic studies from other observation methods that are not grounded in a specific field (analysis of conversation, situated cognition and ethnomethodology). (1997: 11)

So the meaning and significance of 'relations in the field' will vary according to the model of social research with which you are operating. For instance, as Gubrium and Holstein (1997) point out, while naturalists seek to understand social reality as 'it really is', using methods like prolonged observation and open-ended observation, this position is challenged from a range of directions by conversation analysts, feminists and postmodernists.

The obvious implication is that 'relations in the field' cannot simply be a technical issue to be resolved by technical means. Nevertheless, for ease of

presentation, I will begin with some more practical questions, returning to the crucial analytic issues at the end of this chapter.

The following five practical questions are often asked about 'field' research:

- Are certain field settings more 'closed' to observation?
- Must access always be overt?
- What ethical issues lie in wait for me?
- Is feedback to research subjects necessary and/or useful?
- Can I learn anything from relations with subjects in the field?

I discuss below each of these questions. Each question will lead on to the discussion of possible 'solutions' and several case studies will be used in illustration of these points.

### SETTINGS AND ACCESS

Textbooks (e.g. Hornsby-Smith, 1993: 53; Walsh, 1998: 224–5) usually distinguish two kinds of research setting:

- 'closed' or 'private' settings (organizations, deviant groups) where access is controlled by gatekeepers
- 'open' or 'public' settings (e.g. vulnerable minorities, public records or settings) where access is freely available but not always without difficulty, either practical (e.g. finding a role for the researcher in a public setting) or ethical (e.g. should we be intruding upon vulnerable minorities?).

Depending on the contingencies of the setting (and the research problem chosen) two kinds of research access may be obtained:

- 'covert' access without subjects' knowledge
- 'overt' access based on informing subjects and getting their agreement, often through 'gatekeepers'.

The impression you give may be very important in deciding whether you get overt access:

Whether or not people have knowledge of social research, they are often more concerned with what kind of *person* the researcher is than with the research itself. They will try to gauge how far he or she can be trusted, what he or she might be able to offer as an acquaintance or a friend, and perhaps also how easily he or she could be manipulated or exploited. (Hammersley and Atkinson, 1983: 78)

Five ways of securing and maintaining overt access have been noted.

#### IMPRESSION MANAGEMENT

Impression management is to do with the 'fronts' that we present to others (see Goffman, 1959). It involves avoiding giving an impression that might

pose an obstacle to access, while more positively conveying an impression appropriate to the situation (see Hammersley and Atkinson, 1983: 78–88). For instance, I have failed to gain access, despite initial expressions of interest, in two settings. In a paediatric clinic in the early 1980s, a very conservatively dressed physician, spotting my leather jacket, said I was being 'disrespectful of his patients' and threw me out! Fifteen years before that, as a novice researcher, I let slip over lunch that I was thinking of moving from the UK to North America when I had completed my PhD. This attitude was apparently viewed as improperly 'instrumental' by my host organization and the promised access was subsequently refused. The implication of this latter incident is that there is no 'time out' in field relations and that the most apparently informal occasions are times when you will often be judged.

#### OBTAINING 'BOTTOM-UP' ACCESS

This can sometimes be forgotten at great cost. For instance, in the early 1970s, the access granted by the head of personnel at a large organization was put in danger by the fact that I had not explained my aims properly to his subordinates. This underlines the point that access should not be regarded as a once and for all situation.

#### BEING NON-JUDGEMENTAL

Being non-judgemental is often a key to acceptance in many settings, including informal subcultures and practitioners of a particular trade or profession. While the relativist tendencies of many social sciences may allow the researcher sincerely to profess non-judgementality on particular groups' values and practices, this is not always the case when you are studying certain forms of professional practice. Indeed to the researcher who thinks they know something about 'professional dominance' or even just basic communication skills, it is very easy to appear judgemental. However, this not only endangers field relations but also espouses a dangerous orthodoxy.

The 'divine orthodoxy' is that people are 'dopes' (see Silverman, 1997b: 23–6). Interview respondents' knowledge is assumed to be imperfect: indeed they may even lie to us. In the same way, practitioners (like doctors or counsellors) are assumed always to depart from normative standards of good practice.

Under the remit of the divine orthodoxy, the social scientist is transformed into a philosopher-king (or queen) who can always see through people's claims and know better than they do. Of course, this assumption of superiority to others usually guarantees that access will not be obtained or, if obtained, will be unsuccessful!

#### OFFERING FEEDBACK

Some research subjects will actually want your judgements – providing they are of an 'acceptable' kind. For instance, business organizations will expect

some 'pay-off' from giving you access. I discuss what this might involve shortly.

#### ESTABLISHING A CONTRACT

Establishing a contract with the people researched may vary from an information sheet, read and agreed by an individual, to a full-blown contract (but see Punch, 1986, on post-contract problems).

I have so far avoided discussing 'covert' access, i.e. access obtained without subjects' knowledge. We should not assume that 'covert' access always involves possible offence. For instance, on a course I am currently teaching, students have been asked to engage in a small exercise where they observe people exchanging glances in an everyday setting (see Sacks, 1992, Vol. 1: 81-94). Providing the students are reasonably sensitive about this and refrain from staring at others, I do not envisage any problems arising.

However, in other cases, covert observation can lead to severe ethical problems as well as physical danger to the researcher. For instance, Fielding (1982) obtained permission to research a far right British political party but still felt it necessary to supplement official access with covert observation.

In this new situation, he put himself at some potential risk as well as creating ethical dilemmas relating to how much he revealed to his subjects and to outside authorities. It is such ethical issues which I will discuss in the next section.

### ETHICS IN QUALITATIVE RESEARCH

As the German sociologist Max Weber (1946) pointed out in the early years of this century, all research is contaminated to some extent by the values of the researcher. Only through those values do certain problems get identified and studied in particular ways. Even the commitment to scientific (or rigorous) method is itself, as Weber emphasizes, a value. Finally, the conclusions and implications to be drawn from a study are, Weber stresses, largely grounded in the moral and political beliefs of the researcher.

From an ethical point of view, Weber was fortunate in that much of his empirical research was based on documents and texts that were already in the public sphere. In many other kinds of social science research, ethical issues are much more to the fore. For instance, both qualitative and quantitative researchers studying human subjects ponder over the dilemma of wanting to give full information to subjects but not 'contaminating' their research by informing subjects too specifically about the research question to be studied.

Moreover, when you are studying people's behaviour or asking them questions, not only the values of the researcher but the researcher's responsibilities to those studied have to be faced.

Jennifer Mason (1996: 166-7) discusses two ways in which such ethical issues impinge upon the qualitative researcher:

- 1 The rich and detailed character of much qualitative research can mean intimate engagement with the public and private lives of individuals.
- 2 The changing directions of interest and access during a qualitative study mean that new and unexpected ethical dilemmas are likely to arise during the course of your research.

Mason suggests that one way to confront these problems is to try to clarify your intentions while you are formulating your research problem. Three ways of doing this are to:

- 1 Decide what is the purpose(s) of your research, e.g. self-advancement, political advocacy etc.
- 2 Examine which individuals or groups might be interested or affected by your research topic.
- 3 Consider what are the implications for these parties of framing your research topic in the way you have done (1996: 29-30).

Ethical procedures can also be clarified by consulting the ethical guidelines of one's professional association. All such guidelines stress the importance of 'informed consent' where possible (see Punch, 1994: 88-94). The nature of 'informed consent' is set out in Table 15.1.

However, initial consent may not be enough, particularly where you are making a recording. In such cases, it often is proper to obtain further consent to how the data may be used (see Table 15.2).

I have now responded to three of the five questions with which I began, namely:

- Are certain field settings more 'closed' to observation?
- Must access always be overt?
- What ethical issues lie in wait for me?

However, so far, I have provided fairly general answers to these questions. I now want to slow the pace down and give you an example of a case study. I hope this will 'flesh out the bare bones' of these important issues.

TABLE 15.1 What is informed consent?

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- Giving information about the research which is relevant to subjects' decisions about whether to participate
  - Making sure that subjects understand that information (e.g. by providing information sheets written in subjects' language)
  - Ensuring that participation is voluntary (e.g. by requiring written consent)
  - Where subjects are not competent to agree (e.g. small children) obtaining consent by proxy (e.g. from their parents)
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Source: adapted from Kent, 1996: 19-20

TABLE 15.2 A sample consent form for studies of language use

As part of this project, we have made a photographic, audio and/or video recording of you. We would like you to indicate below what uses of these records you are willing to consent to. This is completely up to you. We will only use the records in ways that you agree to. In any use of these records, names will not be identified.

- 1 The records can be studied by the research team for use in the research project
- 2 The records can be used for scientific publications and/or meetings
- 3 The written transcript and/or records can be used by other researchers
- 4 The records can be shown in public presentations to non-scientific groups
- 5 The records can be used on television or radio

Source: adapted from ten Have, 1998: Appendix C, based on a form developed by Susan Ervin-Tripp, Psychology Department, University of California at Berkeley

### A CASE STUDY: OPEN ACCESS TO A PUBLIC SETTING

This case study is drawn from my work on HIV counselling (Silverman, 1997b: 226–8) and has already been referred to in Chapter 7. It illustrates the changing trajectory of one qualitative research project according to the nature and kind of access and funding and my relations with people 'in the field'.

In 1987, I was given permission to sit in at a weekly clinic held at the genitourinary department of an English inner-city hospital (Silverman, 1989a). The clinic's purpose was to monitor the progress of HIV-positive patients who were taking the drug AZT (Retrovir). AZT, which seems able to slow down the rate at which the virus reproduces itself, was then at an experimental stage of its development.

Like any observational study, the aim was to gather first-hand information about social processes in a 'naturally occurring' context. No attempt was made to interview the individuals concerned because the focus was upon what they actually did in the clinic rather than upon what they thought about what they did. The researcher was present in the consulting room at a side angle to both doctors and patient.

I set out below some of the things that happened during this research using indicative headings.

#### MAKING CONCESSIONS

Patients' consent for the researcher's presence was obtained by the senior doctor who preferred to do it this way (this was effective but was it ethical?). Given the presumed sensitivity of the occasion, tape-recording was not attempted. Instead, detailed handwritten notes were kept, using a separate sheet for each consultation. Because observational methods were rare in this area, the study was essentially exploratory.

#### GIVING FEEDBACK

Along the way, I also discovered how an ethos of 'positive thinking' was central to many patients' accounts and how doctors systematically concentrated on the 'bodies' rather than the 'minds' of their patients. This led on to

some practical questions about the division of labour between doctors and counsellors.

#### GOOD LUCK

About the time I was writing up this research, Kaye Wellings, who then was working for the publicly funded Health Education Authority (HEA), approached me about the possibility of extending my research to HIV counselling. Until that time, the HEA had been funding research on the effectiveness of 'safer sex' messages carried in the mass media. In the light of the explosion in the number of HIV tests in the UK in the late 1980s, Kaye thought it might be useful to take a longer look at the effectiveness of the health promotion messages being delivered in counselling people around the HIV antibody test.

I was interested in such a study for two reasons. First, it was the logical development of my study of medical interviews with AIDS patients. Second, it offered the opportunity to pursue my interest in looking at how communication between professionals and their clients worked out in practice – as opposed to the injunctions of textbooks and training manuals. Consequently, I submitted a research proposal and received funding from the HEA for 30 months beginning in late 1988.

#### TROUBLES WITH ACCESS

As it turned out, receiving the funding was only the first part of what became a battle to recruit HIV-testing centres for the research. It must be remembered that the late 1980s was a time when AIDS health workers were being flooded by patients and by requests from researchers anxious to study AIDS care. Apart from such overload, two other factors complicated access. First, obviously, were the multiple ethical issues involved in studying consultations where patients were asked to reveal the most intimate aspects of their behaviour. Second, extra patients and government worries about the AIDS 'pandemic' had brought sudden huge increases in resources to the previously 'Cinderella' branch of medicine treating patients with sexually transmitted diseases. Following the usual pattern, these resource changes produced 'turf' battles between different professions and different centres involved in the AIDS field (see Silverman, 1990).

All this meant that many months were taken in obtaining research access. One leading British centre turned me down, offering the understandable reason that they were already overloaded with researchers. At another such centre, a doctor gave me access but the counsellors subsequently proved very resistant to me observing or tape-recording their HIV consultations. Eventually, a compromise was reached whereby I myself was required to request patients to agree to participate in the research. Predictably, very few agreed in these circumstances.

#### MORE LUCK

Just as I thought that I had been funded for a study that I could never carry out, my luck began to turn. Riva Miller and Robert Bor agreed to offer me

access to their counselling work with respectively haemophiliacs and the general population at the Royal Free Hospital (RFH) in London. This was a major breakthrough in two respects. First, Miller and Bor had just produced a major book (Miller and Bor, 1988) on using 'systemic' method in AIDS counselling. Second, Miller and Bor had a video archive of the work of their clinics going back to the early 1980s.

On the basis of my access at the RFH, a major pharmaceuticals company, Glaxo Holdings PLC (now Glaxo Wellcome), agreed to fund a two-year study (subsequently increased to three years) of the video archive. I was then lucky enough to recruit Anssi Peräkylä from Finland as Glaxo Research Fellow to work on this archive. Anssi had already conducted distinguished ethnographic work in hospital settings. Following his appointment, he more or less taught himself conversation analysis (CA) and had finished his PhD on the RFH data in three years, as well as publishing many articles both jointly with me and/or Bor and on his own. Gradually, other centres joined the project and data were also obtained directly from centres in the USA and Trinidad, as well as from Douglas Maynard's US HIV counselling materials.

#### ETHICAL ISSUES AGAIN

As the research started to take off, great attention had to be paid to the ethical issues involved. We ended up with a method of recruitment whereby counsellors themselves explained the research to patients (often with the aid of written materials) and invited them to participate. Consent was sought on the understanding that the anonymity of all patients would be strictly protected by concealing their names (and other identifying information) in reports or publications. In addition, only Peräkylä, myself and a limited number of trained researchers and transcribers would have access to the audiotapes. The RFH videotapes were given additional protection: Peräkylä himself transcribed them, so access to them was limited to the two of us and the videos were never to be publicly shown or indeed to leave the premises of the RFH.

#### THE CONTINGENCY OF METHODOLOGY

In a multiple centre study, I could not, as in my earlier work, be physically present as all the data were gathered. Instead, the audiotapes were simply sent to me by each of the centres for analysis. Soon we were inundated by data to be passed on to our main transcriber, Dr David Greatbatch, himself a distinguished CA researcher.

However, given the high quality of transcription required and our limited resources, it became totally impractical to transcribe all the tapes. Instead, a few interviews were transcribed from each centre. On this basis, what I can best call 'candidate hypotheses' were developed about particular features in the talk, for instance how health advice was delivered and received. Peräkylä and I would then transcribe multiple instances from many more interviews where relevant phenomena seemed to occur.

In this way, the initial hypotheses were refined and subject to the test of

'deviant cases' which we actively sought out in our data. Overall, our method had much in common with the traditional method of 'analytic induction' commonly used by anthropologists and ethnographers (see Silverman, 1993: Ch. 7).

Let us now return to the two remaining questions:

- Is feedback to research subjects necessary and/or useful?
- Can I learn anything from relations with subjects in the field?

As we shall see, my case study bears on both these questions.

#### FEEDBACK: CASE STUDIES

The bottom line for practitioners is always, 'So what?' A qualitative researcher's efforts to convey nonjudgmental objectivity is likely to be perceived instead as a typical academic cop-out. (Wolcott, 1990: 59)

In order to address practitioners' 'So what?' question, during and after the research described above, I held many workshops for AIDS counsellors – including many who had not participated in the study. To give some idea of the extent of this 'feedback', between 1989 and 1994 I ran four workshops on the research for counsellors in London (two at hospitals, one at Goldsmiths' College and one at The Royal Society of Medicine), as well as three workshops in Australian centres, three in Trinidad and Tobago, and one each in the USA, Finland and Sweden. In addition, each participating centre was given a detailed report of our findings.

At these workshops, we did not shield behind a posture of scientific neutrality. But neither did we seek to instruct counsellors about their presumed 'failings'. Instead, we spoke about the ways in which our data showed that all communication formats and techniques had mixed consequences. We then invited our audience to discuss, in the light of their own priorities and resources, the implications for their practice. Moreover, when asked, we were not afraid to suggest possible practical options.

In my judgement, these meetings were successful, not least because our detailed transcripts showed features of counselling of which the practitioners themselves were often unaware. Often such features revealed how these people were cleverer than they had realized in following their own theoretical precepts and achieving their desired goals.

However, less experienced researchers may be more hesitant to offer feedback to practitioners and organizations. In this case, Wolcott (1990) offers the three ideas set out in Table 15.3.

Of course, not all qualitative research is concerned with organizations or professional practitioners. What kind of feedback is possible when you are studying non-work-related activities?

It is important that you try to offer feedback to all parties that are under

TABLE 15.3 Giving appropriate feedback to practitioners

- 1 Ask for the kind of additional information required for you to make a recommendation (e.g. what exactly the organization is trying to accomplish)
- 2 Identify seeming paradoxes in the pursuit of goals (e.g. doctors who encourage their patients to communicate and to make choices may be the most autocratic)
- 3 Identify alternatives to current practices and offer to assess these

Source: Wolcott, 1990: 60

study. So, if your target is, say, the activities of counsellors or doctors, then you have not finished your task without offering some degree of feedback to their clients or patients. One way to do this is to utilize already existing networks, for example patients' or community groups. So during my work on paediatric clinics in the early 1980s, I spoke to parents' groups at heart and diabetic clinics. For instance, I used my clinic data to show mothers of diabetic adolescents that their feelings of inadequacy were common and probably inevitable given the guilt-provoking character of diabetic control and the usual rebelliousness of teenagers.

Where it is difficult to find such community groups, you may well find that participants in a study welcome receiving their own transcript of relevant data. For instance, a transcript of your own medical interview may work as a useful reminder of what the doctor said. And a transcript of a life-history interview may give a respondent a tangible autobiographical record.

I now want to move from ethical and practical matters to the methodological issue suggested by my final question:

- Can I learn anything from relations with subjects in the field?

One way of answering this question is to think through how your own identity was viewed by the participants. As my case study showed, my identity and aims as a researcher were viewed differently, in various contexts, by different professionals such as counsellors and doctors and by research funding bodies. However, this was viewed not just as a 'trouble' for the smooth running of the research but also as a source of data about how organizations worked (see also Peräkylä, 1989).

The second case study below shows how another identity, that of gender, can be relevant to field research.

### A CASE STUDY: GENDER IN THE FIELD

Almost all the 'classics' of the Chicago School were written by men, and those researchers who rose up the academic hierarchy to become full professors were also mostly men (see Warren, 1988: 11). Increasingly, the gender of fieldworkers themselves was seen to play a crucial factor in observational research. Informants were shown to say different things to male and female researchers. For instance, in a study of a nude beach, when approached by

someone of a different gender, people emphasized their interest in 'freedom and naturalism'. Conversely, where the researcher was the same gender as the informant, people were far more likely to discuss their sexual interests (Warren and Rasmussen, 1977, reported by Warren, 1988).

In studies which involved extended stays in 'the field', people have also been shown to make assumptions based upon the gender of the researcher. For instance, particularly in rural communities, young, single women may be precluded from participating in many activities or asking many questions. Conversely, female gender may sometimes accord privileged access. For instance, Oboler (1986) reports that her pregnancy increased her rapport with her Kenyan informants, while Warren (1988: 18) suggests that women fieldworkers can make use of the sexist assumption that only men engage in 'important business' by treating their 'invisibility' as a resource. Equally, male fieldworkers may be excluded or exclude themselves from contact with female respondents in certain kinds of situation (see McKeganey and Bloor, 1991).

One danger in all this, particularly in the past, was that fieldworkers failed to report or reflect upon the influence of gender in their fieldwork. For instance, in a study of a large local government organization, we discussed but did not report the different kinds of situations to which the male and female researchers gained easy access (Silverman and Jones, 1976). Moreover, even as the role of doing fieldwork as a woman has become more addressed, hardly any attention has been paid by researchers to questions of male gender (McKeganey and Bloor, 1991: 198).

Nonetheless, as fashions change, it is possible to swing too far and accord gender issues too much importance. As McKeganey and Bloor (1991: 195-6) argue, there are two important issues relevant to the significance of gender in fieldwork. First, the influence of gender may be negotiable with respondents and not simply ascribed. Second, we should resist 'the tendency to employ gender as an explanatory catch-all' (1991: 196).

For instance, McKeganey and Bloor suggest variables other than gender, like age and social class, may also be important in fieldwork. Equally, I would argue, following Schegloff (1991), that we need to demonstrate that participants are actually attending to gender in what they are doing, rather than just work with our intuitions or even with statistical correlations. None of this should imply that it would be correct to swing full circle and, like an earlier generation, ignore gender issues in research. It is incumbent upon fieldworkers to reflect upon the basis and status of their observations. Clearly, how the researcher and the community studied respond to their gender can provide crucial insights into field realities. Indeed, we would do well to become conscious that even taken-for-granted assumptions may be culturally and historically specific. For instance, Carol Warren suggests that:

The focal gender *myth* of field research is the greater communicative skills and less threatening nature of the female fieldworker. (1988: 64, my emphasis)

As Warren notes, the important thing is to treat such assumptions not as 'revealed truths' but as 'accounts' which are historically situated.

This second case study shows how gender may help you learn something from relations with subjects in the field. Another way in which researchers have attempted to use field relations as data is by seeking and responding to comments made by participants about research conducted upon them. Returning to an issue I first raised in Chapter 13, can responses to feedback be used as a means of validating your research findings?

### FEEDBACK AS A VALIDATION EXERCISE?

Reason and Rowan (1981) criticize researchers who are fearful of 'contaminating their data with the experience of the subject'. On the contrary, they argue, good research goes back to the subjects with the tentative results, and refines them in the light of the subjects' reactions.

This is just what Michael Bloor (1978; 1983) attempted in his research on doctors' decision-making. Bloor (1978) discusses three procedures which attempt respondent validation:

- 1 The researcher seeks to predict members' classifications in actual situations of their use (see Frake, 1964).
- 2 The researcher prepares hypothetical cases and predicts respondents' responses to them (see also Frake, 1964).
- 3 The researcher provides respondents with a research report and records their reactions to it.

In his study of doctors' decision-making in adeno-tonsillectomy cases, Bloor used the third method, hoping for 'a sort of self-recognition effect' (1978: 549). Although Bloor reports that he was able to make some useful modifications as a result of the surgeons' comments, he reports many reservations. These centre around whether respondents are able to follow a report written for a sociological audience and, even if it is presented intelligibly, whether they will (or should) have any interest in it (1978: 550). A further problem, noted by Abrams, is that 'overt respondent validation is only possible if the results on the analysis are compatible with the self-image of the respondents' (1984: 8).

Bloor's point has been very effectively taken up by Fielding and Fielding (1986) (respondent validation is also criticized by Bryman, 1988: 78–9). The Fieldings concede that subjects being studied may have additional knowledge, especially about the context of their actions. However:

there is no reason to assume that members have privileged status as commentators on their actions . . . such feedback cannot be taken as direct validation or refutation of the observer's inferences. Rather such processes of so-called 'validation' should be treated as yet another source of data and insight. (1986: 43)

I can only add that, if feedback is a highly problematic part of validating research, this does *not* mean that it should be ignored as a way of maintaining contact with subjects in the field. However, this issue should not be *confused* with the validation of research findings.

Moreover, as Bloor points out, the problematic research status of this activity need not mean that attempts at respondents' validation have *no* value. They do generate further data which, while not validating the research report, often suggest interesting paths for further analysis (Bloor, 1983: 172).

Of course, in referring to the function of 'accounts', we are already entering into an analytical perspective on how social reality operates. This is because, as noted at the outset of this chapter, the issue of 'relations in the field' is riddled with theoretical assumptions.

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### SUMMARY

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In this chapter, I have discussed how to respond to *five* practical questions in doing fieldwork:

- 1 Are certain field settings more 'closed' to observation?
- 2 Must access always be overt?
- 3 What ethical issues lie in wait for you?
- 4 Is feedback to research subjects necessary and/or useful?
- 5 Can you learn anything from relations with subjects in the field?

Throughout, I have emphasized that 'relations in the field' are theoretically saturated. This cannot, therefore, be simply a technical issue to be resolved by technical means.

### Further reading

Hammersley and Atkinson (1983: 54–76) provide a useful discussion of the practicalities of obtaining access to individuals, groups and organizations. A more introductory account of these issues, appropriate to the undergraduate researcher, is found in Walsh (1998). Nigel Fielding (1982) is a very interesting account of the perils of observational research. Issues of ethics in qualitative research are well discussed in Jennifer Mason's *Qualitative Researching* (Sage, 1996), Chapters 2, 4 and 8. Peräkylä (1989) and McKeganey and Bloor (1991) provide revealing accounts of the negotiation of identity in fieldwork. Before you contemplate taking your findings back to your subjects, you should read Michael Bloor (1978; 1983).