11

Developing Data Analysis

Chapter 10 stressed the importance of early data analysis and showed how to kick-start such analysis. In this chapter, we will examine how you can develop your research after these beginnings. Although we will focus here just on observational and tape-recorded data, many of the suggestions equally apply to other kinds of qualitative data.

However, a checklist of 'suggestions' can appear somewhat anaemic and without substance. This chapter begins, therefore, with an account of how data analysis developed in one qualitative study.

A CASE STUDY: OBSERVING HEART CLINICS

In the early 1980s (see Silverman, 1987: Chs 1–6) I was directing a group of researchers studying a paediatric cardiology (child heart) unit. Much of our data derived from tape-recordings of an outpatient clinic that was held every Wednesday.

It was not a coincidence that we decided to focus on this clinic rather than upon, say, interaction on the wards. Pragmatically, we knew that the clinic, as a scheduled and focused event, lasting between two and four hours and tied to particular outcomes, would be likely to give us a body of good quality data. By contrast, on the ward, tape-recording would be much more intrusive and produce tapes of poorer quality because of multiple conversations and background noise. Even if these technical problems could be overcome, the (apparently) unfocused character of ward life meant that it would be far harder to see order than in the outpatient clinic. For instance, unlike the latter, there would be no obvious repetitive structures like scheduled meetings by appointment, physical examinations and announcements of diagnosis and prognosis.

Of course, this does not mean that a researcher should never study apparently unfocused encounters – from the hospital ward to the street corner. But it does mean that, if you do, you must be prepared for long vigils and apparently unpromising data before researchable ideas start to gel.

At our hospital clinic, we became interested in how decisions (or 'disposals') were organized and announced. It seemed likely that the doctor's way of announcing decisions was systematically related not only to clinical factors (like the child's heart condition) but to social factors (such as what parents would be told at various stages of treatment). For instance, at a first outpatients' consultation, doctors would not normally announce to parents the discovery of a major heart abnormality and the necessity for life-threatening surgery. Instead, they would suggest the need for more tests and only hint that major surgery might be needed. They would also collaborate with parents who produced examples of their child's apparent 'wellness'. This step-by-step method of information giving was avoided in only two cases. If a child was diagnosed as 'healthy' by the cardiologist, the doctor would give all the information in one go and would engage in what we called a 'search and destroy' operation, based on eliciting any remaining worries of the parent(s) and proving that they were mistaken.

In the case of a group of children with the additional handicap of Down's syndrome, as well as suspected cardiac disease, the doctor would present all the clinical information at one sitting, avoiding a step-by-step method. Moreover, atypically, the doctor would allow parents to make the choice about further treatment, while encouraging them to dwell on non-clinical matters like their child's 'enjoyment of life' or friendly personality.

We then narrowed our focus to examine how doctors talked to parents about the decision to have a small diagnostic test on their children. In most cases, the doctor would say something like:

> What we propose to do, if you agree, is a small test.

No parent disagreed with an offer which appeared to be purely formal – like the formal right (never exercised) of the Queen not to sign legislation passed by the British Parliament. For Down's syndrome children, however, the parents' right to choose was far from formal. The doctor would say things to them like the following:

> I think what we would do now depends a little bit on parents' feelings.
>
> Now it depends a little bit on what you think.
>
> It depends very much on your own personal views as to whether we should proceed.

Moreover, these consultations were longer and apparently more democratic than elsewhere. A view of the patient in a family context was encouraged and parents were given every opportunity to voice their concerns and to participate in decision-making.

In this subsample, unlike the larger sample, when given a real choice, parents refused the test – with only one exception. Yet this served to reinforce rather than to challenge the medical policy in the unit concerned. This policy was to discourage surgery, all things being equal, on such children. So the democratic form coexisted with (and was indeed sustained by) the maintenance of an autocratic policy.

TABLE 11.1   Four ways to develop data analysis

- Focus on data which are of high quality and are easiest to collect (tape-recordings of clinics)
- Focus on one process within those data (how medical 'disposals' are organized)
- Narrow down to one part of that process (announcing a small diagnostic test)
- Compare different subsamples of the population (Down's syndrome children and the rest)

The research thus discovered the mechanics whereby a particular medical policy was enacted. The availability of tape-recordings of large numbers of consultations, together with a research method that sought to develop hypotheses inductively, meant that we were able to develop our data analysis by discovering a phenomenon for which we had not originally been looking.

The lessons to be drawn from this study are summarized in Table 11.1.

In the second half of this chapter, I discuss the more general research strategies available to you when your data, as here, are in the form of tape-recordings of naturally occurring encounters. But perhaps you do not possess your data on tape. Does this mean that everything is lost?

In the next section, I attempt to show how you can shore up the quality of your fieldnotes. Even if, in the final analysis, fieldnotes can never rival the reliability of a good quality tape and transcript, thoughtfully constructed fieldnotes can provide the impetus for advanced data analysis.

FIELDNOTES AND DATA ANALYSIS

Two ways of systematizing fieldnotes

Where you do not have access to naturally occurring data – such as tape-recordings, texts or documents – you must attempt to transcribe as much as possible of what is said and done, and the settings in which it is said and done. Two practical rules have been suggested for making fieldnotes:

- Record what we can see as well as what we hear.
- Expand fieldnotes beyond immediate observations.

USING YOUR EYES

In a study of the social organization of a restaurant, W.F. Whyte (1949) reaped rich rewards by using his eyes to observe the spatial organization of activities. More recently, in a study of interaction in hospital wards, Anssi Peräkylä (personal correspondence) notes how spatial arrangements differentiate groups of people. There are the wards and patient rooms, which staff may enter anytime they need to. Then there are patient lounges and the like, which are a kind of public space. Both areas are quite different from areas like the nurses' room and doctors' offices where patients enter only by invitation. Finally, if there is a staff coffee room, you never see a patient there.

As Peräkylä points out, one way to produce different categories of human beings in a hospital is the allocation of space according to categories. At the same time, this allocation is reproduced in the activities of the participants. For instance, the perceptive observer might note the demeanour of patients as they approach the nurses' room. Even if the door is open, they may stand outside and just put their heads round the door. In doing so, they mark out that they are encroaching on foreign territory.

Unfortunately, we have all become a little reluctant to use our eyes as well as our ears when doing observational work. However, there are exceptions. Stimson has noted how 'photographs and diagrams are virtually absent from sociological journals, and rare in sociological books' (1986: 641). He then discusses a room set out for hearings of a disciplinary organization responsible for British doctors. The Professional Conduct Committee of the General Medical Council sits in a high-ceilinged, oak-panelled room reached by an imposing staircase. There are stained-glass windows, picturing sixteen crests and a woman in a classical Greek pose. As Stimson comments:

> This is a room in which serious matters are discussed: the room has a presence that is forced on our consciousness . . . speech is formal, carefully spoken and a matter for the public record. Visitors in the gallery speak only, if at all, in hushed whispers, for their speech is not part of the proceedings. (1986: 643–4)

In such a room, as Stimson suggests, without anything needing to be said, we know that what goes on must be taken seriously. Stimson aptly contrasts this room with a McDonald's hamburger restaurant:

> Consider the decorations and materials – plastic, paper, vinyl and polystyrene, and the bright primary colours. [Everything] signifies transience. This temporary character is further articulated in the casual dress of customers, the institutionally casualised dress of staff and the seating that is constructed to make lengthy stays uncomfortable. (1986: 649–50)

Stimson and Peräkylä show that ethnographers who fail to use their eyes as well as their ears are neglecting a crucial source of data. This lesson is most readily learnt if you imagine a sighted person being forced to make sense of the world while blindfolded!

EXPANDED FIELDNOTES

> Fieldwork is so fascinating and coding usually so energy-absorbing, that you can get preoccupied and overwhelmed with the flood of particulars – the poignant quote, the appealing personality of a key informant. You forget to *think*, to make deeper and more general sense of what is happening, to begin to explain it in a conceptually coherent way. (Miles and Huberman, 1984: 69)

In order to make 'deeper and more general sense of what is happening', Spradley (1979) suggests that observers keep four separate sets of notes:

1 short notes made at the time
2 expanded notes made as soon as possible after each field session
3 a fieldwork journal to record problems and ideas that arise during each stage of fieldwork
4 a provisional running record of analysis and interpretation (discussed by Kirk and Miller, 1986: 53).

Spradley's suggestions help to systematize fieldnotes and thus improve their reliability (see Chapter 13).

Like Spradley, Miles and Huberman offer systematic ways of expanding what gets recorded in fieldnotes. They suggest writing 'contact summary sheets' or extended memos after each observation (1984: 50–1, 69–71). An example of how to use a contact summary sheet to encourage analytic thinking is set out in Table 11.2.

TABLE 11.2 Questions for contact summary sheets

- What people, events or situations were involved?
- What were the main themes or issues in the contact?
- Which research questions did the contact bear most centrally on?
- What new hypotheses, speculations or guesses about the field situations were suggested by the contact?
- Where should the fieldworker place most energy during the next contact, and what sorts of information should be sought?

Source: Miles and Huberman, 1984: 50

Miles and Huberman suggest five reasons why such contact sheets are valuable:

1 to guide planning for the next contact
2 to suggest new or revised codes
3 to co-ordinate several fieldworkers' work
4 to serve as a reminder of the contact at a later stage
5 to serve as the basis for data analysis (adapted from Miles and Huberman, 1984: 51).

How we record data is important because it is directly linked to the quality of data analysis. In this sense, fieldnotes and contact sheets are, of course, only means to an end – developing the analysis.

Developing analysis of field data

> The move from coding to interpretation is a crucial one . . . Interpretation involves the transcendence of 'factual' data and cautious analysis of what is to be made of them. (Coffey and Atkinson, 1996: 46)

As Miles and Huberman (1984) point out, qualitative data come in the form of words rather than in numbers. The issue, then, is how we move from these words to data analysis. They suggest that data analysis consists of 'three concurrent flows of activity: data reduction, data display and conclusion drawing/verification' (1984: 21):

- *Data reduction* 'refers to the process of selecting, focusing, simplifying, abstracting, and transforming . . . "raw" data' (1984: 21). Data reduction involves making decisions about which data chunks will provide your initial focus.
- *Data display* is 'an organized assembly of information that permits conclusion drawing and action taking' (1984: 21). It involves assembling your data into displays such as matrices, graphs, networks and charts which clarify the main direction (and missing links) of your analysis.
- *Conclusion drawing* means 'beginning to decide what things mean, noting regularities, patterns, explanations, possible configurations, causal flows and propositions' (1984: 22).
- *Verification* means testing our provisional conclusions for 'their plausibility, their sturdiness, their "confirmability" – that is, their validity' (1984: 22).

Miles and Huberman demonstrate that in field studies, unlike much quantitative research, we are not satisfied with a simple coding of data. As I argued in Chapter 3, this means that qualitative researchers have to show how the (theoretically defined) elements that they have identified are assembled or mutually laminated. The distinctive contribution qualitative research can make is by utilizing its theoretical resources in the deep analysis of small bodies of publicly shareable data.

This means that coding your data according to some theoretical scheme should only be the first stage of your data analysis. You will then need to go on to examine how these elements are linked together. At this second stage, lateral thinking can help. For instance, you can attempt to give your chosen concept or issue a new twist, perhaps by pursuing a counter-intuitive idea or by noting an additional feature little addressed in the literature. In any event, as I show below, one way of achieving better data analysis is by a steadily more narrow focus.

Progressive focusing in fieldwork

We only come to look at things in certain ways because we have adopted, either tacitly or explicitly, certain ways of seeing. This means that, in observational research, data collection, hypothesis construction and theory building are not three separate things but are interwoven with one another.

This process is well described by using an analogy with a funnel:

> Ethnographic research has a characteristic 'funnel' structure, being progressively focused over its course. Progressive focusing has two analytically distinct components. First, over time the research problem is developed or transformed, and

eventually its scope is clarified and delimited and its internal structure explored. In this sense, it is frequently only over the course of the research that one discovers what the research is really 'about', and it is not uncommon for it to turn out to be about something quite remote from the initially foreshadowed problems. (Hammersley and Atkinson, 1983: 175)

Atkinson (1992) gives an example of such a redefinition of a research problem. Many years after completing his PhD, Atkinson returned to his original fieldnotes on medical education. He shows how the original data can be reread in a quite different way. Atkinson's earlier method had been to fragment his fieldnotes into relatively small segments, each with its own category. For instance, a surgeon's description of post-operative complications to a surgical team was originally categorized under such headings as 'unpredictability', 'uncertainty', 'patient career' and 'trajectory'. When Atkinson returns to it, it becomes an overall narrative which sets up an enigma ('unexpected complications') which is resolved in the form of a 'moral tale' ('beware, unexpected things can always happen'). Viewed in this way, the surgeon's story becomes a text with many resemblances to a fairytale!

Two studies of British medical clinics that I carried out in the 1980s also nicely illustrate Hammersley and Atkinson's funnel. As I showed above, my observation of a paediatric cardiology unit moved unpredictably in the direction of an analysis of disposal decisions with a small group of Down's syndrome children. Similarly, my research on cancer clinics, discussed in Chapter 10, unexpectedly led into a comparison of fee-for-service and state-provided medicine (Silverman, 1981; 1987).

These two cases had three features in common:

1 The switch of focus – through the 'funnel' – as a more defined topic arose.
2 The use of the comparative method as an invaluable tool of theory building and testing.
3 The generation of topics with a scope outside the substantive area of the research. Thus the 'ceremonial orders' found in the cancer clinics are not confined to medicine, while the 'democratic' decision-making found with the Down's syndrome children had unexpected effects of power with a significance far beyond medical encounters.

As I have noted elsewhere (Silverman, 1993), working this way parallels Glaser and Strauss's (1967) famous account of grounded theory. A simplified model of this involves these stages:

• an initial attempt to develop categories which illuminate the data
• an attempt to 'saturate' these categories with many appropriate cases in order to demonstrate their relevance
• developing these categories into more general analytic frameworks with relevance outside the setting.

Glaser and Strauss use their research on death and dying as an example. They show how they developed the category of 'awareness contexts' to refer to the kinds of situations in which people were informed of their likely fate. The category was then saturated and finally related to non-medical settings where people learn about how others define them (e.g. schools).

'Grounded theory' has been criticized for its failure to acknowledge implicit theories which guide work at an early stage. It is also clearer about the generation of theories than about their test. Used unintelligently, it can also degenerate into a fairly empty building of categories or into a mere smokescreen used to legitimize purely empiricist research (see my critique of four qualitative studies in Chapter 26; and see Bryman, 1988: 83–7). At best, 'grounded theory' offers an approximation of the creative activity of theory building found in good observational work, compared with the dire abstracted empiricism present in the most wooden statistical studies.

However, quantification should not be seen as the enemy of good field research. In the next section, I discuss one example of how simple tabulations were used to test an emergent hypothesis in the study of cancer clinics.

Using tabulations in testing fieldwork hypotheses

In the cancer study, I used a coding form which enabled me to collate a number of crude measures of doctor and patient interactions (Silverman, 1984). The aim was to demonstrate that the qualitative analysis was reasonably representative of the data as a whole. Occasionally, the figures revealed that the reality was not in line with my overall impressions. Consequently, the analysis was tightened and the characterizations of clinic behaviour were specified more carefully.

The crude quantitative data I had recorded did not allow any real test of the major thrust of this argument. Nonetheless, they did offer a summary measure of the characteristics of the total sample which allowed closer specification of features of private and NHS clinics. In order to illustrate this, let me briefly show you the kind of quantitative data I gathered on topics like consultation length, patient participation and the scope of the consultation.

My overall impression was that private consultations lasted considerably longer than those held in the NHS clinics. When examined, the data indeed did show that the former were almost twice as long as the latter (20 minutes as against 11 minutes) and that the difference was statistically highly significant. However, I recalled that for special reasons, one of the NHS clinics had abnormally short consultations. I felt a fairer comparison of consultations in the two sectors should exclude this clinic and should only compare consultations taken by a single doctor in both sectors. This subsample of cases revealed that the difference in length between NHS and private consultations was now reduced to an average of under 3 minutes. This was still statistically significant, although the significance was reduced. Finally, however, if I compared only *new* patients seen by the same doctor, NHS patients got 4 minutes more on average – 34 minutes as against 30 minutes in the private

clinic. This last finding was not suspected and had interesting implications for the overall assessment of the individual's costs and benefits from 'going private'. It is possible, for instance, that the tighter scheduling of appointments at the private clinic may limit the amount of time that can be given to new patients.

As a further aid to comparative analysis, I measured patient participation in the form of questions and unelicited statements. Once again, a highly significant difference was found: on this measure, private patients participated much more in the consultation. However, once more taking only patients seen by the same doctor, the difference between the clinics became very small and was *not* significant. Finally, no significant difference was found in the degree to which non-medical matters (e.g. patient's work or home circumstances) were discussed in the clinics.

These quantitative data were a useful check on over-enthusiastic claims about the degree of difference between the NHS and private clinics. However, as I argued in Chapter 10, my major concern was with the 'ceremonial order' of the three clinics. I had amassed a considerable number of exchanges in which doctors and patients appeared to behave in the private clinic in a manner deviant from what we know about NHS hospital consultations. The question was: would the quantitative data offer any support to my observations?

The answer was, to some extent, positive. Two quantitative measures were helpful in relation to the ceremonial order. One dealt with the extent to which the doctor fixed treatment or attendance at the patient's convenience. The second measured whether patients or doctor engaged in polite small-talk with one another about their personal or professional lives. (I called this 'social elicitation'.) As Table 11.3 shows, both these measures revealed significant differences, in the expected direction, according to the mode of payment.

Now, of course, such data could not offer proof of my claims about the different interactional forms. However, coupled with the qualitative data, they provided strong evidence of the direction of difference, as well as giving me a simple measure of the sample as a whole which contexted the few extracts of talk I was able to use. I do not deny that counting can be as arbitrary as qualitative interpretation of a few fragments of data. However, providing researchers resist the temptation to try to count everything, and base their analysis on a sound conceptual basis linked to actors' own

TABLE 11.3   Private and NHS clinics: ceremonial orders

	Private clinic (n = 42)	NHS Clinics (n = 104)
Treatment or attendance fixed at patients' convenience	15 (36%)	10 (10%)
Social elicitation	25 (60%)	31 (30%)

Source: Silverman, 1993: 165

methods of ordering the world, then each type of data can inform the analysis of the other.

In Chapter 13, I return to the role of counting as an aid to validity in qualitative research. In the case of observational studies, such counting will often be based on the prior coding of fieldnotes. I now, therefore, turn to the issues that arise in such coding.

Limits in coding fieldnotes

The tabulations used in the cancer study derived from:

that well-established style of work whereby the data are inspected for categories and instances. It is an approach that disaggregates the text (notes or transcripts) into a series of fragments, which are then regrouped under a series of thematic headings. (Atkinson, 1992: 455)

Such coding by thematic headings has recently been aided by computer-aided qualitative data analysis systems as discussed in Chapter 12. In larger projects, the reliability of coding is also buttressed by training coders of data in procedures which aim to ensure a uniform approach.

However, there remain two problems with coding fieldnotes. The first, and more obvious, problem is that every way of seeing is also a way of not seeing. As Atkinson points out, one of the disadvantages of coding schemes is that, because they are based upon a given set of categories, they furnish 'a powerful conceptual grid' (1992: 459) from which it is difficult to escape. While this 'grid' is very helpful in organizing the data analysis, it also deflects attention away from uncategorized activities. Therefore, as Clive Seale (personal correspondence) has noted: 'A good coding scheme would reflect a search for 'uncategorized activities' so that they could be accounted for, in a manner similar to searching for deviant cases.'

The second, less obvious, problem is that, as I pointed out in Chapter 3, 'coding' is not the preserve of research scientists. All of us 'code' what we hear and see in the world around us. This is what Garfinkel (1967) and Sacks (1992) mean when they say that societal members, like social scientists, make the world observable and reportable.

Put at its simplest, this means that researchers must be very careful how they use categories. For instance, Sacks quotes from two linguists who appear to have no problem in characterizing particular (invented) utterances as 'simple', 'complex', 'casual' or 'ceremonial'. For Sacks, such rapid characterizations of data assume 'that we can know that [such categories are accurate] without an analysis of what it is [members] are doing' (1992, Vol. 1: 429).

At this point, the experienced researcher might respond that Sacks has characterized conventional research as over-naive. In particular, most researchers are aware of the danger of assuming any one-to-one correspondence between their categories and the aspects of 'reality' which they purport to describe. Instead, following Weber (1949), many researchers claim that

they are simply using hypothetical constructs (or 'ideal types') which are only to be judged in relation to whether they are *useful*, not whether they are *accurate* or *true*.

However, Sacks was aware of this argument. As he notes:

> It is a very conventional way to proceed in the social sciences to propose that the machinery you use to analyze some data you have is acceptable if it is not intendedly the analysis of real phenomena. That is, you can have machinery which is a 'valid hypothetical construct', and it can analyze something for you. (1992, Vol. 1: 315)

By contrast, the 'machinery' in which Sacks is interested is not a set of 'hypothetical constructs'. Instead, Sacks's ambitious claim is throughout 'to be dealing with the real world' (1992, Vol. 1: 316). The 'machinery' he sets out, then, is to be seen not as a set of more or less useful categories but as the *actual* categories and mechanisms that members use. In this sense, he points out:

> I intend that the machinery I use to explain some phenomenon, to characterize how it gets done, is just as *real* as the thing I started out to explain. (1992, Vol. 1: 315, my emphasis)

How should we respond to Sacks's radical critique of ethnography? The first point is not to panic! Sacks offers a challenge to conventional observational work of which everybody should be aware. In particular, Sacks's lecture 'Doing "being ordinary"' (1992, Vol. 2: 215–21) is essential reading for every fieldworker.

However, awareness does not mean that everybody has to follow Sacks's radical path. So one response is to state something like 'thanks but no thanks'. For instance, 'grounded theory' is an equally respectable (and much more popular) way of theorizing (about) fieldwork.

To this effective but essentially defensive manoeuvre, we can add two more ambitious responses. First, we can seek to integrate Sacks's questions about 'how' the social world is constituted with more conventional ethnographic questions about the 'whats' and 'whys' of social life (Gubrium and Holstein, 1997). Or, second, as I describe below, we can make this everyday 'coding' (or 'interpretive practice') the object of inquiry by asking 'how' questions about talk-in-interaction.

TRANSCRIPTS AND DATA ANALYSIS

The two main social science traditions which inform the analysis of transcripts of tapes are conversation analysis (CA) and discourse analysis (DA). For an introduction to CA, see ten Have (1998); for DA, see Potter and Wetherell (1987) and Potter (1997).

In this book we are, of course, more concerned with the practicalities of doing qualitative research. In the rest of this chapter I will, therefore, deal with two practical issues:

- the advantages of working with tapes and transcripts
- the elements of how to do analysis of such tapes.

Why work with tapes?

> The kind of phenomena I deal with are always transcriptions of actual occurrences in their actual sequence. (Sacks, 1984: 25)

The earlier ethnographers had generally relied on recording their observations through fieldnotes. Why did Sacks prefer to use an audio recorder?

Sacks's answer is that we cannot rely on our recollections of conversations. Certainly, depending on our memory, we can usually summarize what different people said. But it is simply impossible to remember (or even to note at the time) such matters as pauses, overlaps, inbreaths and the like.

Now whether you think these kinds of things are important will depend upon what you can show with or without them. Indeed, you may not even be convinced that conversation itself is a particularly interesting topic. But at least by studying tapes of conversations, you are able to focus on the 'actual details' of one aspect of social life. As Sacks put it:

> My research is about conversation only in this incidental way, that we can get the actual happenings of on tape and transcribe them more or less, and therefore have something to begin with. If you can't deal with the actual detail of actual events then you can't have a science of social life. (1992, Vol. 2: 26)

Tapes and transcripts also offer more than just 'something to begin with'. In the first place, they are a public record, available to the scientific community, in a way that fieldnotes are not. Second, they can be replayed and transcriptions can be improved and analyses taken off on a different tack unlimited by the original transcript. As Sacks told his students:

> I started to play around with tape recorded conversations, for the single virtue that I could replay them; that I could type them out somewhat, and study them extendedly, who knew how long it might take . . . It wasn't from any large interest in language, or from some theoretical formulation of what should be studied, but simply by virtue of that; I could get my hands on it, and I could study it again and again. And also, consequentially, others could look at what I had studied, and make of it what they could, if they wanted to disagree with me. (1992, Vol. 1: 622)

A third advantage of detailed transcripts is that, if you want to, you can inspect sequences of utterances without being limited to the extracts chosen by the first researcher. For it is within these sequences, rather than in single turns of talk, that we make sense of conversation. As Sacks points out:

> having available for any given utterance other utterances around it, is extremely important for determining what was said. If you have available only the snatch of talk that you're now transcribing, you're in tough shape for determining what it is. (1992, Vol. 1: 729)

It should not be assumed that the preparation of transcripts is simply a technical detail prior to the main business of the analysis. The convenience of transcripts for presentational purposes is no more than an added bonus.

As Atkinson and Heritage (1984) point out, the production and use of transcripts are essentially 'research activities'. They involve close, repeated listenings to recordings which often reveal previously unnoted recurring features of the organization of talk.

Such listenings can most fruitfully be done in group data sessions. As described by Paul ten Have (1998), work in such groups usually begins by listening to an extract from a tape with a draft transcript and agreeing upon improvements to the transcript. Then:

> the participants are invited to proffer some observations on the data, to select an episode which they find 'interesting' for whatever reason, and formulate their understanding or puzzlement, regarding that episode. Then anyone can come in to react to these remarks, offering alternatives, raising doubts, or whatever. (1998: 124).

However, as ten Have makes clear, such group data sessions should be rather more than an anarchic free-for-all:

> participants are, on the one hand, *free* to bring in anything they like, but, on the other hand, *required* to ground their observations in the data at hand, although they may also support them with reference to their own data-based findings or those published in the literature. (1998: *ibid*)

Analysing tapes

There is a strongly inductive bent to the kind of research that ten Have and Sacks describe. As we have seen, this means that any research claims need to be identified in precise analyses of detailed transcripts. It is therefore necessary to avoid premature theory construction and the 'idealization' of research materials which uses only general, non-detailed characterizations.

Heritage sums up these assumptions as follows:

> Specifically, analysis is strongly 'data-driven' – developed from phenomena which are in various ways evidenced in the data of interaction. Correspondingly, there is a strong bias against *a priori* speculation about the orientations and motives of speakers and in favour of detailed examination of conversationalists' actual actions. Thus the empirical conduct of speakers is treated as the central resource out of which analysis may develop. (1984: 243)

In practice, Heritage adds, this means that it must be demonstrated that the regularities described can be shown to be produced by the participants and attended to by them as grounds for their own inferences and actions. Further, deviant cases in which such regularities are absent must be identified and analysed.

However, the way in which CA obtains its results is rather different from how we might intuitively try to analyse talk. It may be helpful, therefore, if I conclude this section by offering a crude set of prescriptions about things to do and things to avoid in CA. These are set out in Tables 11.4 and 11.5.

TABLE 11.4   How to do CA

1   Always try to identify sequences of related talk
2   Try to examine how speakers take on certain roles or identities through their talk (e.g. questioner–answerer or client–professional)
3   Look for particular outcomes in the talk (e.g. a request for clarification, a repair, laughter) and work backwards to trace the trajectory through which a particular outcome was produced

Source: Silverman, 1998b

TABLE 11.5   Common errors in CA

1   Explaining a turn at talk by reference to the speaker's intentions
2   Explaining a turn at talk by reference to a speaker's role or status (e.g. as a doctor or as a man or woman)
3   Trying to make sense of a single line of transcript or utterance in isolation from the surrounding talk

Source: Silverman, 1998b

If we follow these rules, the analysis of conversations does not require exceptional skills. As Schegloff puts it, in his introduction to Sacks's collected lectures, all we need to do is to 'begin with some observations, then find the problem for which these observations could serve as ... the solution' (Schegloff in Sacks, 1992, Vol. 1: xlviii).

This means that doing the kind of systematic data analysis that CA demands is not an impossibly difficult activity. As Harvey Sacks once pointed out, in doing CA we are only reminding ourselves about things we already know. As Sacks remarks:

> I take it that lots of the results I offer, people can see for themselves. And they needn't be afraid to. And they needn't figure that the results are wrong because they can see them ... [It is] as if we found a new plant. It may have been a plant in your garden, but now you see it's different than something else. And you can look at it to see how it's different, and whether it's different in the way that somebody has said. (1992, Vol. 1: 488)

CONCLUDING REMARKS

Using the examples of tapes and fieldnotes, we have seen how data analysis can be developed after the first stages. However, as I have implied throughout, good data analysis is never just a matter of using the right methods or

techniques but is always based on theorizing about data using a consistent model of social reality. This commitment to theorizing about data makes the best qualitative research far superior to the stilted empiricism of the worst kind of quantitative research.

However, theorization without methodological rigour is a dangerous brew. In Chapter 12, we consider how computer software can aid qualitative research. Then, in Chapter 13, the issues of validity and reliability are discussed.

SUMMARY

Data analysis can be developed in five ways:

1 Focus on data which are of high quality and are easiest to collect.
2 Focus on one process within those data.
3 Narrow down to one part of that process.
4 Compare different subsamples of the population using the comparative method.
5 Generate topics with a scope outside the substantive area of the research.

Glaser and Strauss's (1967) famous account of grounded–theory offers one way of developing analysis of observational data. It involves three stages:

1 an initial attempt to develop categories which illuminate the data
2 an attempt to 'saturate' these categories with many appropriate cases in order to demonstrate their relevance
3 developing these categories into more general analytic frameworks with relevance outside the setting.

Developing simple counting mechanisms can be a further useful way of identifying deviant cases and thereby developing generalizations. However, you should resist the temptation to try to count everything and try to base your analysis on a sound conceptual footing – often linked to actors' own methods of ordering the world.

If you want to try conversation analysis on transcripts, follow these rules:

1 Always try to identify sequences of related talk.
2 Try to examine how speakers take on certain roles or identities through their talk (e.g. questioner–answerer or client–professional).
3 Look for particular outcomes in the talk (e.g. a request for clarification, a repair, laughter) and work backwards to trace the trajectory through which a particular outcome was produced.

Further reading

Miles and Huberman's book *Qualitative Data Analysis* (Sage, 1984) provides a useful treatment of coding observational data. For a more recent discussion, see Robert Emerson et al.'s *Writing Ethnographic Fieldnotes* (University of Chicago Press, 1995). Hammersley and Atkinson's *Ethnography: Principles in Practice* (Tavistock, 1983), Chapters 7–8, is a classic discussion of how to analyse ethnographic data. A development of some of these ideas can be found in Martyn Hammersley's *What's Wrong with Ethnography? Methodological Explorations* (Routledge, 1992). A relatively recent treatment of 'grounded theory' is to be found in Strauss and Corbin's *Basics of Qualitative Research* (Sage, 1990). Sacks's work on conversation analysis is discussed in David Silverman, *Harvey Sacks: Social Science and Conversation Analysis* (Polity, 1998). The case studies of the cancer and heart clinics discussed here are found in David Silverman, *Communication and Medical Practice* (Sage, 1987), Chapters 6–7.

Exercise 11.1

This exercise is based on the various ways to develop data analysis discussed in this chapter. With reference to your own data:

1 Focus on one process within those data. Now narrow down your focus to one *part* of that process. Survey your data in terms of this narrow focus. What can you now find?
2 Compare different subsamples of your data in terms of a single category or process. What does this show?
3 Decide what features of your data may properly be counted and tabulate instances of a particular category. What does this tabulation indicate? Identify 'deviant' cases and explain what you will do with them.
4 Attempt to develop your categories into more general analytic frameworks with relevance outside the setting you are studying.