Exercise 11.2

This gives you an opportunity to analyse your own data as soon as you obtain it.

1 Which questions does your preferred method of data analysis suggest? What interesting generalizations can you start to pull out of your data?

2 Do previous research findings seem to apply to your data? If not, why not? If so, how can you use your data to develop these findings?

3 How do particular concepts from your preferred model of social research apply to your data? Which concepts work best and hence look likely to be most productive?

12 Developing Data Analysis

CHAPTER OBJECTIVES

By the end of this chapter, you will be able to:

- Systematize and analyse field notes.
- Know what to look for in audiotapes.
- Feel confident about developing good data analysis.

12.1 INTRODUCTION

Chapter 11 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. For the analysis of interview data, see Moira Kelly's account of her research on pp. 19–25.

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. The beauty of qualitative research is that it gives you access to the nitty-gritty reality of everyday life viewed through a new analytic lens. Through the example that follows, you will learn how to take advantage of that access in order to focus and then refocus your data analysis.

12.2 A CASE STUDY

In the early 1980s (see Silverman, 1987: Chapters 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

TABLE 12.1 FOUR WAYS TO DEVELOP DATA ANALYSIS

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

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 of 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 sub-sample, 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 co-existed with (and was indeed sustained by) the maintenance of an autocratic policy.

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 12.1.

In the second half of this chapter, I discuss the more general research strategies available to you when your data, as here, is in the form of tape recordings of **naturally occurring data**. 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 field notes. Even if, in the final analysis, field notes can never rival the **reliability** of a good-quality tape and transcript, thoughtfully constructed field notes can provide the impetus for advanced data analysis.

TABLE 12.2 FUNCTIONS OF DETAILED FIELD NOTES

- To identify and follow *processes* in witnessed events
- To understand how members themselves *characterize* and *describe* particular activities, events and groups
- To convey members' *explanations* for when, why or how particular things happen and, thereby, to elicit members' theories of the *causes* of particular happenings
- To identify the *practical concerns, conditions* and constraints that people confront and deal with in their everyday lives and actions

Source: adapted from Emerson et al. (1995)

12.3 FIELD NOTES AND DATA ANALYSIS

12.3.1 *Why detail matters*

> Field researchers seek to get close to others in order to understand their way of life. To preserve and convey that closeness, they must describe situations and events of interest in detail. (Emerson, et al., 1995: 14)

By preserving the details of interaction, you are in a better position to analyse the issues set out in Table 12.2.

Like any set of animating questions, the kind of issues set out in Table 12.2 reflect a particular **model** of the social world. As in my study of heart clinics, Emerson et al. assume a **constructionist** or **ethnomethodological** model in which the meaning of events is not transparent but is actively constructed by the participants (**members**).

Two methodological imperatives flow from this model. First, a concern with what participants take to be *routine* or obvious. Second, a recognition that what is routine is best established through watching and listening to what people do rather than asking them directly. So, unlike much **ethnographic** fieldwork, the interview is not regarded as a major research tool. Instead:

> the distinctive procedure is to observe and record naturally occurring talk and interaction ... [while] it may be useful or essential to interview members about the use and meaning of specific local terms and phrases ... the researcher's deeper concern lies in the actual, situated use of those terms in ordinary interaction. (Emerson et al., 1995: 140)

Such a concern with what participants take to be ordinary and unexceptional gives a clear focus to making and analysing field notes. Data analysis can then develop through asking the sort of questions set out in Table 12.3.

TABLE 12.3 SIX GROUPS OF QUESTIONS FOR FIELD NOTE ANALYSIS

1. What are people doing? What are they trying to accomplish?
2. How exactly do they do this? What specific means and/or strategies do they use?
3. How do members talk about, characterize and understand what is going on?
4. What assumptions are they making?
5. What do I see going on here? What did I learn from these notes?
6. Why did I include them?

Source: Emerson et al. (1995: 146)

12.3.2 *Two ways of developing field note analysis*

Two practical rules have been suggested for developing ethnographic work beyond the initial questions shown in Table 12.3:

- thinking about what we can see as well as what we hear
- expanding field notes beyond immediate observations.

Using your eyes

In a study of the social organization of a restaurant, 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, these are exceptions. Stimson (1986) has noted how 'photographs and diagrams are virtually absent from sociological journals, and rare in sociological books' (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, even without anything needed 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 casualized 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 field notes

> 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 field work journal to record problems and ideas that arise during each stage of field work.
4 A provisional running record of analysis and interpretation (discussed by Kirk and Miller, 1986: 53).

Spradley's suggestions help to systematize field notes and thus improve their reliability (see Chapter 14). Like Spradley, Miles and Huberman offer systematic ways

TABLE 12.4 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)

of expanding what gets recorded in field notes. They suggest writing 'contact summary sheets' or extended memos after each observation (Miles and Huberman, 1984: 50–1, 69–71).

An example of how to use a contact summary sheet to encourage analytic thinking is set out in Table 12.4.

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, field notes and contact sheets are, of course, only a means to an end – developing the analysis.

12.3.3 Developing analysis of field data

> The move from coding to interpretation is a crucial one ... . Interpretation involves the transcendance 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 comes 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 (Miles and Huberman, 1984: 21):

- *Data reduction* 'refers to the process of selecting, focusing, simplifying, abstracting, and transforming ... "raw" data' (ibid.). 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' (ibid.). 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 the provisional conclusions for 'their plausibility, their sturdiness, their "confirmability" – that is, their validity' (ibid.).

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 4, 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 usually 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.

12.3.4 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 field notes 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 field notes 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 fairy tale!

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 9, 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 children had unexpected effects of power with a significance far beyond medical encounters.

As I have noted elsewhere (Silverman, 2001), 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 also is more clear 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 15 and Bryman, 1988: 83–7). At best, 'grounded theory' offers an approximation of the creative activity of theory building found in good observational work, compared to 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 section below, I discuss one example of how simple tabulations were used to test an emergent hypothesis in the study of cancer clinics.

12.3.5 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, it 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 sub-sample 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.

TABLE 12.5 PRIVATE AND NHS CLINICS: CEREMONIAL ORDERS

	Private clinics ($n = 42$) (% in all such clinics)	NHS clinics ($n = 104$) (% in all such clinics)
Treatment or attendance fixed at patients' convenience	15 (36%)	10 (10%)
Social elicitation	25 (60%)	31 (30%)

Source: adapted from Silverman (2001: 243)

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.

This quantitative data was 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 12.5 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, the data 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 methods of ordering the world, then both types of data can inform the analysis of the other.

In Chapter 14, 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 field notes. I now, therefore, turn to the issues that arise in such coding.

12.3.6 Limits in coding field notes

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 13. 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 field notes. 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' (Atkinson, 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 'un-categorized 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 4, '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 suggests 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).

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"' (Sacks, 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 enquiry by asking 'how' questions about talk-in-interaction.

12.4 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 (2004).

In this book, however, 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.

12.4.1 Why work with tapes?

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

The earlier ethnographers had generally relied on recording their observations through field notes. 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 field notes are not. Second, they can be replayed and transcriptions can be improved and analyses take 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 ten Have, 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. (ten Have, 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. (ibid.)

12.4.2 *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 how to do CA. These are set out in Tables 12.6 and 12.7.

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 Sacks once pointed out, in doing CA we are only reminding ourselves about things we already know:

> 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

TABLE 12.6 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 (2001: 177)

TABLE 12.7 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 (2001: 177)

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)

12.5 CONCLUDING REMARKS

Using the examples of tapes and field notes, 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 always is 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 13, we consider how computer software can aid qualitative research. Then, in Chapter 14, the issues of validity and reliability are discussed.

KEY POINTS

Develop data analysis by:

- Working with data which is easy to collect and reliable.
- Focusing on one process within those data.
- Narrowing down to one part of that process.
- Comparing different sub-samples of the population concerned.

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) Martyn Hammersley and Paul Atkinson's *Ethnography: Principles and 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 Anselm Strauss and Juliet Corbin's *Basics of Qualitative Research* (Sage, 1990). Sacks's work on conversation analysis is discussed in my book *Harvey Sacks: Social Science and Conversation Analysis* (Polity, 1998). The case studies of the cancer and heart clinics discussed here are found in my book *Communication and Medical Practice* (Sage, 1987), Chapters 6–7.

Exercise 12.1

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 that 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 sub-samples 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.