



**A VERY SHORT,
FAIRLY INTERESTING AND
REASONABLY CHEAP BOOK ABOUT
QUALITATIVE RESEARCH**

↙
SECOND EDITION



DAVID SILVERMAN

 **SAGE**

Los Angeles | London | New Delhi
Singapore | Washington DC

On Finding and Manufacturing Qualitative Data

In the previous chapter, I gave you a taste of the way in which qualitative researchers can access fascinating data by observing mundane settings or by finding everyday features in extraordinary settings. I called such an approach 'ethnography'.

However, in order to simplify matters, I have so far glossed over two issues to which we must now turn. First, by no means do all ethnographers display the kind of attention to fine detail that I described. Some desire to tell exciting tales from the field. Others, especially in recent times, displace such detail with what I find to be a depressing concern with highfalutin' theory and experimental writing (see my discussion of post-modernism in Chapter 5).

Second, it bends reality considerably to imply that ethnography is today the main method of qualitative research and that observational material is the main data source. In a way this is hardly surprising given the plethora of materials that invite our attention. These extend beyond what we can observe with our own eyes to what we can hear and see on recordings, what we can read in paper documents and electronically download from the Internet, to what we can derive by asking questions in interviews or by providing various stimuli to focus groups.

However, despite this wide range of material, when it comes to actual research studies, there is hardly an even spread of methods. Nor is it the case that ethnography is just one among many methods. Instead of looking, listening and reading, the majority of contemporary qualitative researchers prefer to select a small group of individuals to interview or to place in focus groups. In this sense, by assembling a specific research sample, linked only by the fact that they have been selected to answer a pre-determined research question, such researchers prefer to 'manufacture' their data rather than to 'find' it in the 'field'. Despite their earnest claims to do something quite different from quantitative research (more 'humanistic', more 'experiential', more 'in-depth'), such manufacture of data to answer a specified research problem is precisely *the* method which quantitative research espouses.

Four crucial points

I have now raised four further points that demand to be addressed:

- The warrant and good sense of the terms I have been using (e.g. ‘manufactured’ data).
- The warrant for claiming that ‘manufactured’ data is in the ascendancy in contemporary qualitative research.
- The ‘so what?’ question (i.e. if there is such an ascendancy, does it matter?). What kinds of phenomena can you see by using your eyes and ears on the world around you that you might miss by asking questions of interview respondents?
- Since most qualitative researchers are not dopes, how have they looked at the world in such a way as to limit their preferred options in qualitative research design? How might these perspectives provide a kind of mental ‘blinkers’?

My first point raises an issue that critical readers may be already asking: what do you mean by ‘manufactured data’? Doesn’t it assume a dangerous polarity between what is ‘natural’ and what is ‘unnatural’ or ‘contrived’? As anthropologists like Mary Douglas (1975) have shown us, aren’t these precisely the kinds of cultural categories that we need to study in use rather than to impose? Isn’t all data ‘manufactured’ in the sense that ‘reality’ never speaks for itself but has to be apprehended by means of particular concerns and perspectives and by the simple logistics of research – for instance, where you place your VCR? Moreover, am I implying that there are intrinsically ‘good’ and ‘bad’ sources of data? By contrast, as all experienced researchers learn, doesn’t your choice of data always depend upon your research problem?

These are indeed important points which will require us to unpack such simplistic terms as ‘manufactured’ and ‘found’ data. This is precisely what I intend to do at length later in this chapter once I have offered you some more substance. At the cost of temporarily suspending these points, I will avoid getting bogged down too early in what may look like a game of wearisome definitions.

However, I want to answer the second point straight away. What evidence do I have for my assertion that ‘manufactured’ data is the pre-eminent concern of contemporary qualitative research?

My first evidence is merely anecdotal. For more than twenty years, I have been advising students who have chosen to do qualitative research projects. During that time, I find that around 90 per cent of my supervisees initially nominate interviews as their preferred data source. Of course, my sample may be skewed but I should point out that, particularly since

my retirement from a full-time university post in 1998, it includes students in many different institutions, disciplines and continents.

However, I have less anecdotal evidence. In the 1990s, I did a survey of two social science journals and found that, of the qualitative research articles published in the past 5 years, interviews and focus groups constituted between 55 per cent and 85 per cent of the total. More recently, I analysed the contents of one qualitative research journal between 2008–9. Of the 18 research articles it published, 17 were based on manufactured data (16 interview studies, 1 study based on focus group data).

How might things change in the future? It is possible that the burgeoning growth of the Internet will mean a slight increase in the proportion of published studies based on naturalistic data? Yet, once again, this may be limited by many researchers' preference to interview online participants rather than to analyse what they do on their PCs.

Why not just study the rich seam of naturally-occurring data on the Internet? Why not take advantage of the fact that the Internet now allows us to study past events as they happened through the use of net archives? As Kozinets points out:

Newsgroups, forums and other bulletin boards, blogs, mailing lists, and most other synchronous media are automatically archived. The Wayback Machine or Internet Archive captures snapshots of the Internet at certain points in time and saves them for future reference. Efficient search engines make accessible every interaction or every posting on a given topic to a specific newsgroup, or every posting by a given individual to any newsgroup. (2010: 72)

Kozinets describes such research as 'netnography' and observes that 'The analysis of existing online community conversations and other Internet discourse combines options that are both naturalistic and unobtrusive – a powerful combination that sets netnography apart from focus groups, depth interviews, surveys, experiments and on-person ethnographies' (2010: 56).

Subject to ethical constraints (Markham, 2011: 122–3; Kozinets, 2010: 137–40, 194–6), by looking at what people are actually doing on the Internet, we might observe the following netnographically related social facts:

- The text of a particular blog posting has been written and was posted.
- A certain social networking group has been formed, and certain accounts have been linked to it.
- A certain photo was uploaded to a particular photo-sharing community, and received 37 comments (Kozinets, 2010:133).

Yet despite the impressive range of social facts available on the Internet and elsewhere, most qualitative researchers still prefer to analyse manufactured data. As Potter and Hepburn observe:

[S]tandard methods handbooks present interviewing as the default choice for virtually every perspective (phenomenology, ethnography, grounded theory). The situation in sociology is similar. For example, in 2004 the journal *Sociology* published some 56 substantive articles – of these, 20 used interviews or focus groups (often with little justification) and just three used naturalistic data (working with the loosest of criteria). (2007: 277)

My final evidence for this preference derives from my reading of the job adverts for research posts that my daily newspaper, the *Guardian*, provides every Tuesday. Although I can't provide percentages, my impression is that, yet again, in the majority of cases, 'qualitative research' is associated with asking questions of respondents. Let me offer one example which I believe is fairly representative.

In 2003, I came across an advert asking for applications for a research post on a study of 'how psycho-social adversity is related to asthma morbidity and care'. The text of the advert explained that this problem would be studied by means of qualitative interviews. My immediate question was: How can qualitative interviews help to address the topic at hand? The problem is not that people with asthma will be unable to answer questions about their past nor, of course, that they are likely to lie or mislead the interviewer. Rather, like all of us, when faced with an outcome (in this case, a chronic illness), they will document their past in a way which fits it, highlighting certain features and downplaying others. In other words, the interviewer will be inviting a retrospective 'rewriting of history' (Garfinkel, 1967) with an unknown bearing on the causal problem with which this research is concerned.

This is not to deny that valuable material may be gathered from such a qualitative study, but rather that data analysis should address an altogether different issue – narratives of illness in which 'causes' and 'associations' work as rhetorical moves.

By contrast, a quantitative study would seem to be much more appropriate to the research question proposed. Quantitative surveys can be used on much larger samples than qualitative interviews, allowing inferences to be made to wider populations. Moreover, such surveys have standardised, reliable measures to ascertain the 'facts' with which this study is concerned. Indeed, why should a large-scale quantitative study be restricted to surveys or interviews? If I wanted reliable,

generalisable knowledge about the relation between these two variables (psycho-social adversity and asthma morbidity), I would start by looking at hospital records.

This asthma study seems to have been designed in terms of a very limited, if common, conception of the division of labour between qualitative and quantitative research. While the latter concentrates on data which shows people's behaviour, qualitative research is seen as the realm where we study in-depth people's experiences through a small number of relatively unstructured interviews. This led to what I perceive to be two blunders in the design of qualitative research. First, a failure to recognise that some research questions might be better studied using largely quantitative data. Surely the causal question posed here can be better addressed via a questionnaire administered to a large sample of asthma patients or by a survey of hospital records to see if there is any correlation between an asthma diagnosis and referrals to social workers and/or mental health professionals.

The second blunder is that the research design as stated appears to misunderstand the wide potential of qualitative research to study such things as the careers of asthma patients. Why can't qualitative research study behaviour? For instance, why not conduct an ethnographic study which observes whether (and, if so, how) doctors in hospitals and primary care facilities elicit histories from their patients relating to psycho-social problems? Why not study social work and hospital case conferences to see if such problems are recognised and, if so, what action is demanded? In short, why assume that qualitative research involves only researchers asking questions of respondents?

Moreover, the research design elects to present the main research question to respondents themselves. This causes two problems. First, as is well known in quantitative surveys, if respondents are made aware of your interests, this can affect their responses. Second, it can lead to lazy research in which careful data analysis is simply replaced by reporting back what people have told you.

As Clive Seale has pointed out:

This is a very common problem in all kinds of studies, but particularly ones where people mistakenly use a qualitative design to answer a question better suited to an experiment or quasi-experimental design. People decide, say, that they are going to see if TV violence encourages violent behaviour. Instead of doing a survey of what people watch on TV and a parallel survey of their tendency to violence, and then seeing whether there is a correlation (hoping that there are no spurious reasons for such a correlation of course) they just select a

group of people and ask them (more or less) ‘do you think TV watching causes violence?’. (Personal correspondence)

Now I want to deal with my third, ‘so what?’ question. In part, the asthma study already offers an answer: by eliciting ‘manufactured’ data, we limit considerably the range of phenomena we can discover and may, sometimes, end up pursuing a path better trod by our quantitative colleagues. However, because the ‘so what?’ question is important, I want to provide some further examples taken from a recent journal article. Since I will shortly point out what I take to be limitations in the approach used, I should emphasise that, in no sense do I take this to be a particularly poor or weak paper – how could I when the paper I have chosen actually cites my work approvingly!

The paper discussed below is taken from the field of organisations and management, but I have some evidence to suggest that the same trends apply in other substantive fields of social science. For instance, my 1990s survey of published articles revealed a similar situation in the study of health and medicine.

The paper, by Alison Linstead and Robyn Thomas (2002) is called: “‘What do you want from me?’ A poststructuralist feminist reading of middle managers’ identities’. Please forget for the moment the theoretical baggage mentioned in the title (‘poststructuralist’, ‘feminist’). The issue of the uses of such theory, if you are interested, is taken up in Chapter 5 of this volume.

Linstead and Thomas state in their abstract that their paper ‘explores the process of identity construction of four male and female middle managers within one restructured organisation’. Quite appropriately, they recognise that their sample is small (interviews with just four managers) and shows some recognition of the consequences of their selective use of extracts from these interviews. One such extract from an interview with Wayne is shown in Box 2.1.

Box 2.1 Interview extract

I've changed so much since I started here. A lot of my mates haven't survived the changes ... they were good guys but they weren't in control of what happened to them. I've been lucky, of course I have, but I've worked for it, I've never sat back, I've always tried to get more paper behind me ... you've always got to keep up but it's getting in front that gives you the insurance.

Here is how this extract is interpreted by the researchers:

[it] rests on a justification that working hard and getting ahead is demanded by both circumstance and by who you are, and being a ‘good guy’ is not enough as you have to master the situation. Wayne sees this as achieved through qualifications, but these perhaps function as a sign for other activities that he does not mention. He also evinces a degree of paradoxical guilt that he is a survivor, that he is marked out as different from those men he was close to once, although this is precisely what his actions were intended to do. He is genuinely distressed that his friends lost their jobs, but has to remain hardened to this, to keep his sentiments masked, as he knows he could be next. (Linstead and Thomas, 2002:10)

What we have seen of this study raises three sets of questions laid out below:

- What are we to make of the authors’ commentary on this interview extract? What does it add to what any reader could make of it? Is it merely the kind of thing that a journalist might add to a report of a celebrity interview? If it is any different from these things, what warrant do Linstead and Thomas have to suggest the significance of what Wayne ‘does not mention’, to talk about ‘paradoxical guilt’ and to assert that Wayne is ‘genuinely distressed ‘but has ... to keep his sentiments masked’? In what sense is this social science analysis or what, perhaps unfairly, is sometimes called ‘psychobabble’?
- Like many qualitative interview reports, no stretches of talk are provided which include both the interviewee’s answer and the previous, adjacent interviewer’s question, request for continuation or display of understanding (e.g. ‘mm hmm’, ‘I see’). As Tim Rapley has pointed out, such an omission fails to recognise that ‘interview interactions are inherently spaces in which both speakers are constantly “doing analysis” – both speakers are engaged (and collaborating in) “making meaning” and “producing knowledge”’. (2004: 26–7)
- Research reveals that people invoke multiple identities in both everyday life (Sacks, 1992) and within interviews (Holstein and Gubrium, 1995: 33–4). Why limit research to interviews when you could observe identity construction within the organisation (e.g. by looking at committee meetings and/or employees’ files)?

As Clive Seale (personal correspondence) points out, Linstead and Thomas, like many interview researchers, have not treated the interview itself as an observational site, and have ended up ‘buying’ the respondent’s version.

This partly arises because, like the asthma study, this research on middle managers presents the main research question to respondents.

Qualitative researchers' use of the interview as their default method of data gathering is perplexing given what we know about how participants produce their accounts differently for different audiences. As Jonathan Potter and Amanda Hepburn put it:

Why produce materials that are flooded by social science agendas and researcher categories, where participants work with a range of different interview-related orientations to stake and interest, and where the parties shift between complex research-related footing positions? What is the special magic the interview provides that makes the very complex analytic task of dealing with those endemic and probably inescapable interview features worthwhile? (2007: 280).

Qualitative researchers' almost-Pavlovian tendency to identify research design with interviews has blinkered them to the possible gains of other kinds of data, for it is thoroughly mistaken to assume that the sole topic for qualitative research is 'people'.

Seale has noted how he seeks to contest this common supposition:

I find that, in order to counteract the tendency towards wanting to do interviews, it helps to repeatedly make the point that many textbooks assume that when one is going to do a research study one always wants to sample 'people' (rather than, say, documents). This helps [students] realise that all kinds of phenomena can be studied for social research purposes (e.g. building design, music lyrics, websites, small ads etc.) and it is then obvious that interviews aren't the only thing to do. (Personal correspondence)

Even when the choice of interviews is thought through (i.e. interviews undoubtedly give you far more rapid results than observation which, when done properly, can take months or years), many research reports offer journalistic 'commentaries' or merely reproduce what respondents say rather than provide detailed data analysis.

Think about how such a detailed analysis might be made of Linstead and Thomas's interview extract. You might begin from the positioning of Wayne's word 'lucky'. Note how it comes immediately after what might be heard as a criticism of his 'mates' who have lost their jobs. Such criticism of colleagues suggests a 'boastful' person who feels 'superior'. The sting of this is removed by invoking 'luck'. Indeed, throughout the extracts provided by Linstead and Thomas, their respondents beautifully

attend to and manage how we might hear them by invoking such downgrades immediately after what might be heard as ‘boasting’ or criticising others. In the next chapter, I examine in greater depth the importance of attending to such a *sequential* positioning of an account.

It is now time to turn to my fourth and final question: Why has this happened? In Chapter 5, I will discuss what I call the ‘interview society’ – the kind of cultural environment which has made qualitative interviews attractive to researchers. Here I will merely use a very broad brush to touch upon the intellectual currents which underlie this process.

The nineteenth century was the age of romanticism. In both music and literature, the earlier emphasis upon the use of conventional, classical forms was gradually replaced by a focus on the inner world of the artist. So artistic works came to be judged, in part, by how they gave access to the artist’s experiences and emotions. This meant that an eighteenth-century’s critic’s appreciation of a work by Mozart as ‘most scientific’ no longer made sense. As the next century wore on, although reference might still be made to the formal structure of works, it now became important to refer to both the composer’s and audience’s emotions as a standard of appreciation. If you have seen *Amadeus* in the cinema or theatre, its focus on Mozart’s ‘personality’ should remind you of the continuing power and appeal of romanticism.

The psychologist Kenneth Gergen points out very clearly what this kind of artistic Romanticism means for how we think about each other: ‘The chief contribution of the romanticists to the prevailing concept of the person was their creation of the *deep interior* ... the existence of a repository of capacities or characteristics lying deeply within human consciousness’ (1992: 208–9).

Gergen gives us a ready answer to my ‘why?’ question. It is only a short leap from thinking about the ‘deep interior’ of the person to favouring ‘in-depth’ interviews. Indeed, once we assume that people have a ‘deep interior’, then it is easy to see the contemporary appeal of a whole range of contemporary formats ranging from qualitative interviews to counselling and other ‘psy’ professions, to TV chatshows and celebrity magazines.

But, you may ask, am I really suggesting that it is an illusion to suggest that nothing lies between our ears? Doesn’t this contradict our own ‘experience’ that we have thoughts and feelings?

My answer to these questions is a bit complicated. No, I do not want to deny that we think and feel. What I would like to contest is the too-ready assumption that what happens between our ears is purely a *private* matter – until accessed by the skills of the research interviewer or counsellor.

Reading other people’s minds is certainly not a skill reserved for professionals. Indeed, as Harvey Sacks points out, we learn about the ability

of others to read our minds as children. Sometimes these others are teachers or even, as we may be told, an all-seeing God. Most regularly, however, they are our mothers. For instance, when children are asked what they have been doing, they can find their answer denied by their mother – who wasn't there – 'say[ing] "No you weren't" and the child then corrects itself' (Sacks, 1992, 1:115). So schizophrenics who believe that others can read their minds may only be mimicking adult-child talk.

But what about the surely paranoid delusion that other people can *control* your mind? Sacks give the example of somebody saying to you 'Remember that car you had?' Now, even though the car was not on your mind at all, you can't help but remember it. In that sense, the first speaker has indeed controlled your mind. As Sacks puts it, 'people aren't crazy for thinking that other people control their minds. That could not be a source for craziness. That could only be a matter of wisdom' (1992, 2:401).

Sacks wants to show us how, as hearers, we depend upon our ability to read the speaker's mind in order to work out the next action required of us. In this respect, even apparently private matters can be viewed as social and structural.

Take the apparently extreme case of 'memory'. Surely 'memory' is something contained inside our heads and therefore 'private'? In response to such an assumption, Sacks invites us to think about those occasions when we had wanted to make a point but the present speaker had continued or someone else had grabbed the floor. In such circumstances, don't we often 'forget' the topic that we wanted to mention? As Sacks observes, 'if you don't get a chance to say it, when you then get a chance to say it, you've forgotten it' (1992, 2:27). In this respect, memory is not at all private or personal but 'in some perhaps quite dramatic way at the service of the conversation ... It is in some ways an utterance by utterance phenomenon' (1992, 2:27) or, as the novelist Julian Barnes has put it, 'The story of our life is never an autobiography, always a novel ... Our memories are just another artifice' (2000: 13).

But Sacks has still more shocks for romantics. If memory is not simply a private matter, nor is 'experience'. One way to understand this is through Sacks's discussion of storytelling.

Sacks shows us that, when we tell a story (unless we are a bore), we try to find an audience to whom the story will be relevant. Indeed, without such an audience, we may not even remember the story. Storytellers also prefer to display some kind of 'first-hand' involvement in the events they describe. Indeed, people are only entitled to have experiences in regard to events that they have observed and/or which affect them directly. For instance, in telephone calls, events like earthquakes are usually introduced in terms of how you survived. Indeed, such events tend to get discussed

less in terms of when they happened but more in relation to when we last talked – our ‘conversational time’ (Sacks, 1992, 2:564).

In this way, Sacks notes, we seek to turn events into experiences or ‘something for us’ (1992, 2:563). However, this shows that telling someone about our experiences is not just emptying out the contents of our head but organising a tale told to a proper recipient by an authorised teller. In this sense, experiences are ‘carefully regulated sorts of things’ (Sacks, 1992, 1:248).

Introducing the notion of ‘regulation’ into something so apparently personal as ‘experience’ is just one surprise that Sacks has in store for us. Moreover, for Sacks, in everyday life, we cannot even count on an objective realm of ‘facts’ to balance apparently subjective ‘experience’.

Scientists usually assume that they first observe facts and then seek to explain them. But, in everyday life, we determine what is a ‘fact’ by first seeing if there is some convincing explanation around. For instance, coroners may not deliver a verdict of suicide unless there is some evidence that the deceased person had a reason to take their own life (Sacks, 1992, 1:123). In that sense, in everyday life, only those ‘facts’ occur for which there is an explanation (1992, 1:121).

What is the import of Sacks’s revelation that, in many senses, what goes on between our ears is a public matter? To me, it suggests that qualitative researchers who unthinkingly prefer to use interviews are barking up the wrong tree. Blinded by a vision of people’s ‘deep interiors’, they remorselessly focus on accessing the insides of people’s heads rather than observing how we make ‘experiences’ and ‘motives’ publicly available in innumerable everyday contexts.

Sacks once made the telling remark that if his students were really interested in the insides of people’s heads, they should become brain surgeons rather than sociologists! In that way, they would discover that, contra romanticism, the only thing that lies between our ears is boring old grey matter.

Indeed, there is something quite curious about researchers providing commentaries on what people say to them in interviews. After all, being a competent member of society means that you can make sense of what strangers tell you without needing a skilled researcher to help you out. To render this situation even stranger, Sacks invents a device that he calls a ‘commentator machine’. He tells us that this hypothetical machine might be described by the layman in the following terms:

It has two parts; one part is engaged in doing some job, and the other part synchronically narrates what the first part does ... For the commonsense perspective the machine might be called a ‘commentator machine’, its parts ‘the doing’ and ‘the saying’ parts. (1963: 5)

For a native-speaking researcher, the ‘saying’ part of the machine is to be analysed as a good, poor or ironical description of the actual working of the machine (1963: 5–6). However, Sacks points out this sociological explanation trades off two kinds of unexplicated knowledge:

- (a) knowing in common with the machine the language it emits and
- (b) knowing in some language what the machine is doing. (1963: 6)

But to know ‘what the machine is doing’ ultimately depends upon a set of pre-scientific, commonsense assumptions based on everyday language and employed to sort ‘facts’ from ‘fancy’. It follows that our ability to ‘describe social life’, whether as laypeople or sociologists, ‘is a happening’ which should properly be the ‘job of sociology’ (1963: 7) to *describe* rather than tacitly to *use*.

I have answered my fourth question by using Sacks’s account of a weird commentator machine to show the strange kinds of blinkers that qualitative researchers place over their eyes when they unthinkingly elect to use interviews to answer their research questions. I might also add that this is a circular issue since research questions are often framed by using categories like ‘experience’ which foreshadow the collection of data by such (mistakenly) ‘in-depth’ methods.

Sacks’s insistence on the priority of describing the everyday ‘procedure employed for assembling cases of the class’ radically separates his position from contemporary romantic researchers. So when Linstead and Thomas identify ‘paradoxical guilt’ and ‘genuine distress’ in Wayne’s account of his job, they are working with what Sacks calls ‘undescribed categories’. As Sacks puts it, ‘To employ an undescribed category is to write descriptions such as appear in children’s books. Interspersed with series of words there are pictures of objects’ (1963: 7).

For Sacks, most sociologists get by through simply ‘pointing’ at familiar objects (what philosophers call ‘ostensive’ definitions). So they are able to give an account of what Sacks’s ‘commentator machine’ is ‘doing’ by invoking ‘what everybody knows’ about how things are in society – using what Garfinkel (1967) refers to as the ‘etcetera principle’ based on treating a few features as indicating the rest to any reasonable person. They thus pretend to offer a ‘literal’ description of phenomena which conceals their ‘neglect [of] some undetermined set of features’ (Sacks, 1963: 13).

Such neglect cannot be remedied, as some researchers claim, by assembling panels of judges to see if they see the same thing (e.g. inter-coder agreement as a basis for claiming that Linstead and Thomas’s commentary on Wayne’s account is reliable). Such agreement offers no solution because it simply raises further questions about the *ability* of members of

society to see things in common – presumably by using the ‘etcetera principle’ as a tacit resource (see Clavarino et al., 1995).

Sacks’s problem is how we can build a social science that does better. ‘In some way, we must free ourselves from the common-sense perspective’ (1963: 10–11) employed in our use of ‘undescribed categories’. For Sacks, the solution is to view such categories ‘as features of social life which sociology must treat as subject matter’ rather than ‘as sociological resources’ (1963: 16).

What looks like a complicated theoretical solution turns out, however, to involve a quite straightforward direction for research. We must give up defining social phenomena at the outset (like Durkheim’s initial definition of ‘suicide’) or through the accounts that subjects give of their behaviour (Sacks’s ‘commentator machine’). Instead, we must simply focus on what people *do*. As Sacks puts it, ‘whatever humans do can be examined to discover some way they do it, and that way would be describable’ (1992, 1:484).

Sacks concedes that this kind of research can seem to be ‘enormously laborious’ (1992, 1:65). However, he denies critics’ claims that it is trivial. You only need to look at the ability of both laypersons and conventional researchers consistently to find recognisable meaning in situations to realise that social order is to be found in even the tiniest activity. The accomplishment of this ‘order at all points’ (1992, 1:484) thus constitutes the exciting new topic for social research.

Beginning with the observability of ‘order at all points’, our first task should be to inspect the ‘collections of social objects – like “How are you feeling?” – which persons assemble to do their activities. And how they assemble those activities is describable with respect to any one of them they happen to do’ (1992, 1:27).

So far, I have been using Sacks’s brilliant insights to support my critique of the naive use of interview data. But Sacks also has positive things to say about what we can learn through observation. Some of Sacks’s ideas are set out in Box 2.2.

Box 2.2 Sacks on observing everyday life

Take the example of socialising. The ability of some people to be able to enter into conversations with attractive strangers is something that puzzles a lot of us. Indeed, books with titles like HOWTO WIN FRIENDS usually sell very well. What is the knack involved?

(Continued)

(Continued)

Have you ever said 'hello' to a stranger and been rebuffed? The problem is that such a greeting implies that you already knew the person concerned and hence had 'an initial right to use "Hello"' (Sacks, 1992, 1:103). Hence a stranger need not return your greeting.

As Sacks says, one solution to this problem is to begin with questions to a stranger such as:

'Don't I know you from somewhere?'

'Didn't I see you at such-and-such a place?'

'Aren't you so-and-so?' (1992, 1:103)

The advantage of the question form is that it is properly receipted by an answer. So not to answer a question, even if you suspect the motives of the questioner, is a difficult act to bring off. Moreover, having got that answer, the questioner properly may ask *another* question. In that way, conversations get started.

All this means that questions can be an effective 'pickup' device. Indeed, in an exercise where Sacks asked his class to provide examples of utterances which might start conversations with members of the opposite sex, around 90 per cent were questions (1992, 1:49).

Among such questions, routine requests are a particularly powerful pickup device. In addition to the obligation to provide an answer to a question, there is the expectation that we should not be needlessly rude to a stranger making a request for something as mundane as, say, the time. Moreover, the requester knows that (s)he will get a standard, quick response and thus will soon be in a position to ask a further question which may start a longer conversation, for example:

A: When does the plane arrive?

B: 7:15

A: Are you going to San Francisco also? (1992, 1:103).

So questions can be good pickup devices when you happen to find yourself in physical proximity to a stranger. However, things get more complicated when the person you are interested in is part of a larger crowd involved with you in a multi-party conversation. In this situation, Sacks asks, how do people manage to set up a purely two-party conversation?

One possibility is to ask if anyone wants a drink and then to return with the drink to sit next to the particular target of your attention (Sacks, 1992, 2:130). In this way, the right 'territorial' situation can be created. Alternatively, one can try waiting until everybody other than the targeted party has left or, more reliably, if there is music, offer an invitation to dance (1992, 2:131). Indeed, the institution of the dance can be seen as a nice solution to the problem of transforming multi-party into two-party conversation (although the noise of modern clubs may limit this possibility).

I hope you agree with me in finding these examples fascinating. However, you may have another gripe: you may understandably wonder what their relevance is to the 'big' issues out there in society. For all my criticism of Linstead and Thomas's paper, you may say, at least it dealt with important aspects of modern life such as the modern work rat-race. By shifting our gaze from interviews to observation, are we in danger of narrowing our gaze to the minutiae of 'pickups'?

Happily, Sacks shows how a detailed attention to the language we use relates to much wider political issues than how people present themselves. Take the methods used by racists to link particular 'evils' to the work of people with certain identities (e.g. Catholics, Jews, blacks, Muslims). People don't just simply 'fall' into certain categories, rather we identify people by choosing one of many categories that could be used to describe them. It then follows that

what's known about that category is known about them, and the fate of each is bound up in the fate of the other ... [so] if a member does something like rape a white woman, commit economic fraud, race on the street, etc., then that thing will be seen as what a member of some applicable category does, not what some named person did. And the rest of them will have to pay for it. (Sacks, 1972: 13)

Not only do Sacks's observations give us a useful hold on how racism works, but they also provide a way of describing one aspect of another 'big' issue – social change. For Sacks, one way we could identify social change would be by noticing 'shifts in the properties of categories used in everyday language and in how these categories were actually applied' (1979: 14). For instance, since 9/11 think how the usage of the category 'Muslim' has been transformed.

So Sacks can give us a grip on apparently 'important' issues like racism or social change. However, we have to be careful here because Sacks

rejected ‘the notion that you could tell right off whether something was important’ (1992, 1:28). He uses the case of biology to show how the study of an apparently minor object (‘one bacterium’) can revolutionise our knowledge. Why assume, for instance, you need to look at states and revolutions, when, as Sacks shows, some apparently tiny object like a question to a stranger ‘may give an enormous understanding of the way humans do things and the kinds of objects they use to construct and order their affairs’ (1992, 1: 28)?

These extended examples drawn from Sacks’s trailblazing lectures forty years ago illustrate what qualitative researchers can learn about the world without needing to interview anybody. They suggest that, all things being equal, we have no need to ‘manufacture’ data and should prefer to examine what I have called ‘found’ data.

So far this has been quite a partisan treatment of the debate about how qualitative research should properly be conducted. Indeed, I am sometimes, mistakenly, accused of being ‘anti-interview’.

I now want to slow down, as it were, and to go through the debate in a more measured way, taking account of the counter-arguments made by its critics. In doing so, I will replace the somewhat clumsy term ‘found data’ with the more commonly used description ‘naturally occurring data’ to denote material that appears to arise without a researcher intervening directly or providing some ‘stimulus’ to a group of respondents.

The rest of this chapter will consider the answers to a set of questions that arise from the position I have so far taken:

- What are the basic arguments for preferring naturally occurring data?
- What are the limitations of these arguments?
- Is there a way forward which takes on board the (good) arguments of both sides?

Why naturally occurring material is special

This will be quite a short section which will serve to recap what I have been saying up to now by linking Sacks’s pioneering work to the arguments of some contemporary researchers. As we have seen, Sacks implied that, when researchers offer commentaries on interviewees’ statements, they tend to use commonsense or purely research-driven categories.

Of course, researchers can avoid this problem by simply doing a ‘content analysis’ which will identify respondents’ own categories and count how frequently they use them. Unfortunately, there are two reasons why this is no real solution to the problem that Sacks has raised.

First, when an interviewee uses a particular category (e.g. Wayne's references to 'keeping up' and 'getting in front'), one cannot reliably know whether (and how) he actually uses such a category outside the interview context. What we do know from researchers like Holstein and Gubrium (1995) and Rapley (2004) is that interviewees fashion their categories from researchers' categories (e.g. 'tell me your story') and activities (e.g. 'uh huh').

Second, if categories are utilised in particular contexts rather than simply pouring out of the insides of people's heads, any method we use (even content analysis) cannot transform what interviewees say into anything more than a category used at a particular point in some interview. It follows that if we are interested in institutions rather than interviews, our first thought should be to study those institutions themselves. As Sacks puts it, this means 'attempting to find [categories] in the activities in which they're employed' (1992, 1:27).

Sacks's detailed arguments have largely been ignored by most qualitative researchers, but it is wrong to assume that this means that Sacks (and myself) are completely out on a limb. In particular, some influential contemporary ethnographers contest the conventional assumption, deriving from the early work of Howard Becker, that interviews give us direct access to people's perceptions and that the role of observation is merely to see if such perceptions and meanings are 'distorted' (Becker and Geer, 1960).

Unlike Becker, later ethnographers do not always agree that interviews should play a significant role in field research. For instance, in a book devoted to the writing of ethnographic fieldnotes, we find the following pointed comment:

[E]thnographers collect material relevant to members' meanings by focusing on ... naturally occurring, situated interaction in which local meanings are created and sustained ... Thus interviewing, especially asking members directly what terms mean to them or what is important or significant to them, is *not* the primary tool for getting at members' meanings. (Emerson et al., 1995: 140)

Sometimes interview researchers will concede Emerson et al.'s point but raise a practical objection. They say that, although interview data can raise the problems of interpretation to which I have been alluding, we often, perforce, must interview simply because we cannot obtain access to the 'naturally occurring situated interaction' to which Emerson et al. refer.

Say you are interested in 'the family'. Surely it will be difficult to obtain access to people's homes in order to understand their family life?

My answer to this question is that the likely unavailability of data which it assumes is actually a giant red herring. In a paper on methodological issues in family studies, Gubrium and Holstein (1987) show how much sociological work assumes that 'family life' is properly depicted in its 'natural' habitat – the home. However, this involves a number of commonsensical assumptions, for example, that families have 'inner' and 'outer' sides (the 'inner' side is located in the household) and that outside of households we obtain only a 'version' of this 'prime reality'.

Conversely, they argue that the 'family' is not a uniform phenomenon, to be found in one setting, but is 'occasioned' and 'contexted'. 'Family' is a way of interpreting, representing and ordering social relations. This means that the family is not private but inextricably linked with public life. So the household does not locate family life. Instead, the 'family' is to be found wherever it is represented.

This means that family studies do not need to be based on either obtaining access to households or interviewing family members. This is because what the 'family' is, is not some stable, unitary object. So, if you are interested in the family, simply study wherever this institution is invoked. If you cannot access a household (or do not want to), try the law courts, probation service, paediatric clinics, newspaper stories and advice columns and so on.

Gubrium and Holstein's alternative direction for family studies closely fits Sacks's approach, while opening up a number of fascinating areas for family studies. Once we conceive of the 'family' in terms of a researchable set of descriptive practices, we are freed from the methodological and ethical nightmare of obtaining access to study families 'as they really are', that is, in their own households.

Issues of household location and privileged access now become redefined as topics rather than troubles – for example, we might study the claims that professionals make for such access. This underlines Gubrium and Holstein's point that family knowledge is never purely private. Even in interviews, family members will themselves appeal to collective representations (like maxims and the depiction of families in soap operas) to explain their own behaviour. Family members also present the 'reality' of family life in different ways to different audiences and in different ways to the same audience.

Of course, Gubrium and Holstein's arguments apply well beyond family studies. They show that, when researching *any* institution, lack of access should not lead us to assume that interviews are the only way forward.

Following Sacks, we can carry this argument even further than either Emerson or Gubrium and Holstein would probably want to go. Take Jonathan Potter's position on this debate. Potter (1996, 2002) has

roundly criticised researchers who use his own approach (discourse analysis) for depending too much on interview data and has argued for a greater use of naturally occurring data. Closely following my concept of 'manufactured' data he shows how interviews, experiments, focus groups and survey questionnaires are all 'got up by the researcher'. Instead, he proposes what he humorously calls 'the dead social scientist test'. As he describes it:

The test is whether the interaction would have taken place in the form that it did had the researcher not been born or if the researcher had got run over on the way to the university that morning. (Potter, 1996: 135)

Potter's test is a useful device for asking questions at the initial stage of research design. However, how far can we take it? Am I (and Potter) saying that interviews and the like are always off-limits to competent qualitative researchers? To answer this question, I must move on to the limits of this extreme position.

Some limitations in the argument: why manufactured data can never be entirely off-limits

I am aware that much of this chapter may so far read as a polemic which seeks to lay down the law about what constitutes 'good' and 'bad' research. I ought to stress, once again, that, consonant with the mandate of this series, what you are reading are merely my own views and there are plenty of good qualitative researchers who part company with some or even all of my argument.

However, even in this context, editorial balance is never a bad thing. So, without withdrawing anything that I wrote earlier, I will now show that there are a number of reasons why we should not take the undoubted appeal of naturally occurring data too far. As I shall argue:

- no data are intrinsically unsatisfactory
- no data are 'untouched by the researcher's hands'
- polarities like naturally occurring data versus manufactured data are rarely helpful if carried too far
- apparently 'good quality' data do not guarantee 'good quality' research
- everything depends upon how you analyse data rather than the data's source.

— No data are intrinsically unsatisfactory

This reiterates one of the few principles about which all experienced researchers can agree. There are no ‘good’ or ‘bad’ data. In assessing the value of any data source, everything depends on what you want to do with them and on your research question. For instance, as Patrick Brindle has asked (Personal correspondence), how are we to study the social history of past events in living memory without recourse to interviewing?

Such a pragmatic approach to interview data is reinforced by critical comments made by Clive Seale when he read a first draft of this chapter:

Is it not the case that in medieval times (i.e. before the Romantic movement) when people wanted to find out something (e.g. the people who constructed the Domesday book) they went and asked people for reports? Do we feel that Booth, in his survey of London poverty, was a ‘romantic’ because he relied on respondents’ reports rather than observations? Is it the case that conventional qualitative interviews are always trying to get at the secret inner core of a person, rather than just asking them to report on something they have seen, heard, done etc.? Might it be the case that people think of interviews first for quite pragmatic and commonsense reasons: because this is how anyone tries to find out about experiences they don’t know much about: by asking some people who have had those experiences. (Personal correspondence)

Seale’s pragmatic approach is illustrated by how Tim Rapley (2004) has chosen to work with interview data. Rapley uses conversation and discourse analysis – a theoretical position deriving from Sacks and Potter. This would appear to rule out using what I have called ‘manufactured’ data. However, Rapley’s topic in his PhD research was precisely how identities get produced in research interviews. Hence not only did he work with interview data, he actually borrowed this data from somebody else’s study. Yet this use of (second degree) manufactured data was fully justified by his research topic. Indeed, even Jonathan Potter has recently used (manufactured) focus group data for precisely the same reason (Puchta and Potter, 2004).

— No data are ‘untouched by the researcher’s hands’

As I observed earlier in this chapter, isn’t the idea of ‘manufactured data’ somewhat slippery? Doesn’t it assume a dangerous polarity between what is ‘natural’ and what is ‘unnatural’ or ‘contrived’? Even when we think we are not ‘intervening’ in the field (e.g. by posing questions to

research subjects), our data cannot be entirely 'natural' but will be mediated by the presence of our recording equipment and by the process of obtaining informed consent as required by contemporary ethical standards. Hence isn't it better to refer to 'naturalistic' data since no data is ever untouched by human hands? But, if this is the case, as Susan Speer rightly observes, what happens to the 'dead scientist test' (2002: 516)?

● Polarities are usually unhelpful in research

I have called the opposition between 'manufactured' and 'naturally occurring' data a 'polar opposition', that is, it assumes that you have to choose one pole or the other. However, it is usually a good rule of thumb that such polarities work better in the lecture hall than in actual research. Generally speaking, social science should investigate such polarities rather than use them. For instance, as anthropologists like Mary Douglas have shown, we need to investigate how different groups distinguish between what is 'natural' and 'artificial' for them.

● 'Good quality' data does not guarantee 'good quality' research

Making videos of people engaging in their ordinary activities might appear to be at the other end of the continuum to posing questions to a respondent asked to assume the identity of an interviewee. However, it is dangerous to assume that using the former kind of material guarantees research of high quality. Not only are there always technical issues (which recording equipment you use and where you place it), but your video data will never speak for itself. Instead, you will need to work your way through a number of complicated problems: how will you transcribe and analyse your videos, will you simply use illustrative examples or will you try to be more systematic (and, if so, how)?

Even if you just observe, you will need to find some way of recording your observations. Despite ethnographers' attention to the logic of writing their fieldnotes (see Emerson et al., 1995), most do not confront fully the problematic character of how we describe our observations. Put at its simplest, this relates to what categories we use. As Sacks says:

Suppose you're an anthropologist or sociologist standing somewhere. You see somebody do some action, and you see it to be some activity. How can you go about formulating who is it that did it, for the purposes of your report? Can you use at least what you might take to be the most conservative formulation – his name? (1992, 1:467)

As Sacks suggests, this apparently trivial problem is actually not resolvable by better technique, like detailed note-taking. Rather it raises basic analytic issues: 'The problem of strategy ... may not be readily handleable by taking the best notes possible at the time and making your decisions afterwards. For one, there is an issue of when it is for the Members that it turns out who did the thing' (1992, 1:468).

In fact, many contemporary ethnographers, now aided by advanced software packages, ignore this problem. In the way Sacks suggests, they simply put in some set of categories derived from lay usage (1992, 1:629). By doing so, of course, we are no wiser of how, in situ, categories are actually deployed and enforced, nor how violations in category use are actually recognised (1992, 1:635–6).

Everything depends upon how you analyse data

While, in many (but not all) senses there cannot be 'bad' data, there certainly can be flawed data analysis. Such flaws can arise, for instance, where we focus on just one interview extract without analysing its position in a conversation or comparing it to other extracts which might tell a different story. A flaw more relevant to my present argument is when we treat what people say in interviews (or elsewhere) as providing a simple picture of the inside of their heads.

But this need not be the case. Following Sacks, we can treat what people say as an account which positions itself in a particular context (e.g. as somebody responding to an interviewer's question and/or as a person claiming a particular identity, i.e. as a 'family member', 'employee', 'manager' etc.). Here the researcher is viewing what people say as an *activity* awaiting analysis and not as a *picture* awaiting a commentary.

This debate shows that, as Clive Seale has noted, a lot depends on the claims you make about your analysis. For Seale,

interviews can be treated as a 'resource' rather than a topic as long as researchers are aware of the problem of relying on someone else's report, who often has particular interests in presenting a particular version. If these are taken into account when drawing (cautious) conclusions, then I can't see why one can't do that with interviews too. (Personal correspondence)

If you go further than Seale and treat interview talk as a topic, then both interviews and tapes can be studied as courses of action. Indeed, the distinction between the interview and observation depends on an

unexamined separation between ‘thinking’ and ‘doing’ (Atkinson and Coffey, 2002: 813).

All this seems to suggest that this has been a worthless debate or, at best, a debate only useful to clear your mind about a dangerous polarity. However, I would not have wasted your time if I believed this was entirely the case. In fact, I think this debate raises a number of issues which are central to the conduct of qualitative research. I will, therefore, conclude this chapter by suggesting a modest way forward.

A way forward

This section unashamedly draws upon a very useful discussion of these issues in the journal *Discourse Studies* (2002) based upon an article by Susan Speer with responses by, among others, Jonathan Potter. Although Speer begins by questioning the polarity between naturally occurring and contrived data and Potter by supporting it, both conclude by conceding that, ultimately, everything turns upon your research topic rather than upon choosing one side of this polarity.

For instance, even though Speer is uncomfortable with the assumption that there is such a thing as ‘naturally occurring data’, she recognises that research interviews or ‘other manufactured’ methods of gathering data may not be the best way to research certain topics. So, if you want to study, say, how counselling gets done, why seek retrospective accounts from clients and practitioners or use a laboratory study? Equally, if you are studying gender, she notes that you should be wary of basing your research on interviews where respondents are asked to comment on gender issues. As she observes, you are much more likely to gather reliable data by studying how people actually *do* gender in everyday environments, for example in meetings, email messages and so on (Speer, 2002: 519–20).

Speer also provides a second way to take this debate forward. Instead of making a rigid distinction between manufactured and naturally occurring data, she suggests that we should simply examine how far any research setting is *consequential* for a given research topic.

For instance, in one laboratory study cited by Schegloff (1991: 54), limitations were placed on who could speak. This made the experimental setting consequential for its topic of ‘self-repair’ and undercut its conclusions. Without such limitations, the study would have been sound.

A second example is a study I reviewed for a journal. Its topic was humour in testicular cancer consultations. The data was derived from interviews with patients. Moreover, there was some evidence that patients had been asked directly about using jokes in their consultations. As I have suggested earlier (and as Speer notes about gender research), such direct

questions will influence what people say and are not usually a useful way to investigate a phenomenon.

My final example of how the research setting can impinge on the reliability of data is Drew's (1987) own study of humour. Drew's use of a video-camera might have been consequential if, say, Drew was concerned with the frequency of laughter. However, his focus was on how jokes get done and he argued that the presence of the camera was irrelevant.

In all these cases, the issue, as Schegloff (1991) has put it, is whether the research environment was *procedurally consequential*, that is, whether how the data was gathered influenced its reliability. It demands that researchers attend to and demonstrate that they have thought through the extent to which their findings may simply be an artefact of their chosen method. In this respect a concern to overcome 'procedural consequentiality' is more important than the side of the natural/manufactured polarity upon which your data fall.

Concluding remarks

Through the work of researchers influenced by Sacks, as well as that of linguistically oriented ethnographers, a new programme is starting to assume more importance in qualitative research. Rather than seek to avoid 'bias' through the use of 'neutral' or 'objective' research instruments, this programme treats all research contexts as thoroughly social, interactional occasions (Speer, 2002). Given this position, it follows that the default data source for such researchers are those contexts which societal members ordinarily assemble for themselves. Faced with the ubiquity and complexity of such contexts, why would any researcher seek to create a special research setting in order to study how people behave? To those who argue that some members' practices are difficult to access, we can agree but point out that such unavailability is only apparent and based on commonsense assumptions about where phenomena (e.g. 'the family') are to be found.

Yet, despite these cogent arguments, manufactured research settings, such as interviews and focus groups, have become predominant in qualitative research and even ethnographers usually feel compelled to combine and test their observations by asking questions of informants.

In the light of a recent debate in *Discourse Studies*, I have reassessed the value of the concept of 'naturally occurring' data and its relevance to the programme of qualitative research. Of course, in all research, choice of data must, in part, depend upon our research problem. Equally, there is no question that all polarities should be investigated – particularly where, as here, they involve an appeal to 'nature'.

The moderate tone I have recently introduced should not conceal the strong impulses I derive from my own research experience. This teaches me that, all things being equal, it is usually a good ploy (and certainly an aid to the sluggish imagination) to begin a research project by looking at naturally occurring data. While iron rules are rarely a good idea in research, this rule has worked for me and many of my students. In the next few lines I will explain why I think this is so.

Harvey Sacks continually reminded his students that it turns out our intuitions rarely give us a good guide to how people actually behave. We cannot rely on our memory of what someone said because such memory will not preserve the fine detail of how people organise their conversation. Nor is this problem soluble by using mechanical equipment to record research interviews, for people's own perceptions are an inadequate guide to their behaviour.

By contrast, naturalistic data can serve as a wonderful basis for theorising about things we could never imagine. As Sacks puts it, using what ordinarily happens in the world around us means 'we can start with things that are not currently imaginable, by showing that they happened' (1992, 1:420).

Jonathan Potter has recently extended Sacks's arguments and I can do no better than present below the five virtues that Potter finds in working with naturally occurring data:

- Naturalistic data does not flood the research setting with the researcher's own categories (embedded in questions, probes, stimuli, vignettes and so on).
- It does not put people in the position of disinterested experts on their own and others' practices and thoughts.
- It does not leave the researcher to make a range of more or less problematic inferences from the data collection arena to topic as the topic itself is directly studied.
- It opens up a wide variety of novel issues that are outside the prior expectations embedded in, say, interview questions.
- It is a rich record of people living their lives, pursuing goals, managing institutional tasks and so on. (Adapted from Potter, 2002: 540)

None of Potter's five points deny that interviews or experiments can ever be useful or revealing: 'However, they suggest that the justificatory boot might be better placed on the other foot. The question is not why should we study natural materials, but why should we not?' (Potter, 2002: 540).