Introduction: Making a Space for this Book

What do you imagine goes through the mind of authors as they write a book? Do you picture lonely geniuses or misguided bores who feel compelled to get into print knowing that their words will be misinterpreted or, still worse, ignored? Or do you think of the solid artisan who ploughs through masses of material in order to produce a work that will be acceptable to the maximum number of readers?

For the past decade or so, I have attempted to be such an artisan. The textbooks and edited readings I have produced (Silverman, 2004, 2005 and 2006) have aimed to be comprehensive and balanced. In pursuit of this aim, they have also been rather long. By contrast, this book is short and intentionally opinionated and partial. It also lacks many traditional textbook features such as bullet-point lists, exercises and recommended readings. So why on earth should you consider buying it?

If you want to be spoonfed or to find a quick read that you will dip into in order to scrape a pass on a research methods course, this is not the book for you. By contrast, my aim is to stimulate you by making you reconsider some assumptions that textbooks trade off.

This book comes with no claims to get you through a course. At best, an encounter with less conventional reading such as this may earn you some favour with teachers who are bored by pre-digested, textbook answers.

I know that university study increasingly seems like boring factory work in which you are required to produce a certain output while paying for the privilege. In such a context, there may be little time to look for intellectual stimulation. Why then should you put yourself out to venture beyond the minimum requirement?

To answer this question, in the next section I will explain why the mechanics of research might matter to you. After that, I will try to give you an idea why qualitative research has come to matter to me.

why might research methods matter to you?

I want to answer this question concretely. Rather than give you an abstract argument, I would like you to think about some of the things you currently do and might do in the future. An important finding by
qualitative researchers is that, rather than having one fixed version of who we are, we all move between multiple identities (see Holstein and Gubrium, 1995; Rapley, 2004; and Silverman, 1987: chapter 10). Think about three actual or possible identities: student, employee and citizen. Let us now consider the relevance of a knowledge of research methods for each identity.

**Student**

If you are a student on a research methods course, it is quite likely that you will be asked to carry out a small-scale research project. If so, you may be tempted to seek simple ‘cookbook’ answers to your research question. But, if you are wiser, you will look for a textbook that tries to provide you with practical examples of actual research studies and offers hands-on experience of analysing data (these are the objectives of my own texts).

However, if you are a brighter student, you may crave to know more. This book seeks to offer you an entry into broader questions that many textbooks necessarily have to gloss over. For instance, what is the underlying logic of qualitative logic? And what are the key debates about its future direction? This book sets out an unashamedly partial answer to such questions.

**Employee**

Now imagine that you are employed in a job that requires you to keep up with research studies in your area. If so, you will need to be able to evaluate the credibility of any relevant findings. Or imagine you have to commission research. Now you will want to know what kinds of studies (quantitative, qualitative or multi-method) are appropriate and what kinds of methods and data-analysis will give you the results you are seeking. Once again, you will need to go beyond the limited horizons of a standard textbook.

**Citizens**

Finally, we all are citizens. To a greater or lesser extent, we follow the news and want to take an informed position on current debates. With its focus on how people’s interaction shapes phenomena as diverse as organizations and families, qualitative research gives you its own trademark
understanding of everyday life. As I show in Chapters 1 and 4, by making mundane situations remarkable, qualitative research can bring into focus taken for granted forms of behaviour and open the way to new possibilities. As active citizens, this is information we need.

These are some of the identities you may move between. But what about my identity(ies)? How have they shaped what appears in the following pages?

### Why research methods matter to me

This section will involve a brief autobiographical account. Like many people, I stumbled into the subject of my first degree. I had studied economics at school and because that subject seemed to offer some potential for either a managerial or academic career, I applied to study for a degree in economics.

My first selection interview was spectacularly unsuccessful, in part because I had not properly prepared. When asked why I wanted to study economics at Nottingham University, I came up with the brilliant answer, ‘Because my best friend has applied for the same course’!

The rejection that followed made me rethink my strategy. Before the next interview, I read *The economist* and followed its advice to read J.K. Galbraith’s book *The Affluent Society*. My comments on this book seemed to impress my next panel of interviewers and I was accepted to read for a BSc in Economics at the London School of Economics (LSE) with Industry and Trade as my special subject.

If I had followed my intended path, I might have ended up in industry or the British civil service. But fate intervened. Fed up with the rote teaching at my local secondary school, I convinced my parents to send me to a private tutorial college for the last six months of my pre-university studies. At this college, one of my teachers had newly graduated in sociology from LSE. It was one-to-one teaching and I was easily influenced. Almost overnight, I discovered sociology and the work of Karl Marx. Fortunately, the LSE BSc (Econ) was a relatively flexible degree and I was able to change my special subject.

In the 1960s, sociology at LSE was dominated by four figures: Tom Bottomore, Donald MacRae, David Glass and Robert McKenzie. From Bottomore and MacRae, I learnt that the main issues that mattered in British sociology arose in debates that grew out of nineteenth-century social theory in the work of Marx, Durkheim and Weber. Moreover, although Glass and McKenzie were researchers as well as theorists, the kind of research they favoured was mainly quantitative (demography and/or survey research). Indeed, the only research methods course around
at LSE was on statistics – albeit very entertainingly taught by Claus Moser. Even the advanced methods course that I subsequently took at the University of California, Los Angeles (UCLA), for my MA in Sociology, was largely concerned with the design of quantitative research. Only a graduate seminar with Mel Dalton, author of a great piece of research on middle managers (Dalton, 1959), gave me a hint of what might be gained from more ethnographic work.

When I returned to the UK from UCLA, I began my research career with a study of the beliefs and values of junior ‘white collar’ workers. Influenced by sociological theories of class and social status deriving from the German sociologist Max Weber, I wanted to see if the way you perceived yourself was influenced by where you worked and by your future job prospects.

I used a structured interview schedule and my methodology was cast in the standard forms of quantitative research: an initial hypothesis, a two-by-two table and statistical tests (see Silverman, 1968). If I had completed this study, my future career might have taken a completely different path.

However, I started to have nagging doubts about the credibility of my research. Although I could manipulate my data so as to provide an apparently rigorous test of my hypotheses, this data was hardly ‘raw’ but mediated by various kinds of interpretive activities. Not the least of these arose in my administration of the interview schedule.

As I was interviewing my respondents, I was struck by the need to go beyond my questions in various, unforeseen ways so as to obtain the sort of answers I wanted. Perhaps, I thought, I hadn’t pre-tested my questions properly. It was only much later that I learnt that how we make sense in conversations necessarily relies on everyday conversational skills that cannot be reduced to reliable techniques (see Rapley, 2004).

In any event, I abandoned this study and turned to organization theory in a work that was to be both my PhD and a successful textbook (Silverman, 1970). The approach I used was influenced by a mid-twentieth century student of Weber, Alfred Schutz. Schutz’s phenomenology of the everyday world was concerned with the structures of everyday life. It made an easy link, when in 1971–2, I was introduced to the study of the methods we all use in everyday life (ethnomethodology) by Aaron Cicourel who was a visitor at Goldsmiths’. Ultimately, this led to a book (Filmer et al., 1972), which enjoyed a short-lived fame as an early British text sympathetic to ethnomethodology.

After a period immersed in organization theory and philosophy, by the mid-1980s I had moved first into ethnomethodologically-inspired ethnography and then into conversation analysis. I spent the following decade exploring the uses of two contemporary social science theories.
An ethnography of the personnel department of a public sector organization (Silverman and Jones, 1976) was heavily influenced by Harold Garfinkel’s (1967) ethnomethodology. And an analysis of literary texts (Silverman and Torode, 1980) derived from Ferdinand de Saussure’s (1974) semiotics (see Chapter 3). These studies confirmed my belief in the value of theoretically-informed research – a belief affirmed throughout the present text.

However, guiding principles tend to be double-edged. So, while we should assert their benefits, we should also be aware of their possible costs. Looking back on this early work, I now feel that it was a trifle over-theorised. Perhaps I had been so enthused by a newly discovered theory that I hadn’t allowed myself to be sufficiently challenged, even surprised, by my data.

Such over-theorisation is an ever-present danger given that many social science disciplines still, I believe, run in fear of being discovered, like the fabled Emperor, without any clothes (for a recent valuable exception see Kendall and Wickham’s (1999) fine text on the practical research uses of Foucault’s ideas). It is for this reason that what has been called the ‘post-modern’ period of experimental ethnographic writing comes under fire in Chapter 5 of this book.

In my later research, I tried to find a better balance between the theoretical ‘armchair’ and the empirical ‘field’. In both an ethnography of hospital clinics (Silverman, 1987) and a conversation analytic study of HIV-test counselling (Silverman, 1997), I adopted a more cautious approach to my data, inductively establishing hypotheses, using the comparative method and identifying deviant cases. In both studies, unlike my earlier work, I explored ways of making my research relevant to a wider, non-academic audience in a non-patronizing way (see Chapter 4).

However, these later studies also derived from two related methodological assumptions present in my 1976 study of a personnel department. All three studies were based not on interviews but on naturally-occurring data (see Chapter 2). And all of them looked at how the participants talked to one another and focused on the skills they used and the local functions of what they did.

To sum up: research methods matter to me because my attempts to do worthwhile social research have brought me face to face with issues of principle that cut across both methodological and theoretical issues. This book is based on the lessons that research practice have taught me. It brings to the fore a number of positions that are implicit in my textbooks: a demand that qualitative research be methodologically inventive, theoretically-alive and empirically rigorous.

This will give you some sense of ‘where I am coming from’. However, this section would not be complete without a further autobiographical
note. The first three chapters of this book make considerable use of the insights of the American sociologist Harvey Sacks. Sacks hardly figures in most contemporary courses on social theory or qualitative methodology. So why introduce him here?

In September 1964, after my first degree at the London School of Economics, I enrolled as a graduate student and Teaching Assistant in the Department of Sociology at the University of California, Los Angeles. By chance this coincided with Harvey Sacks’s first set of lectures in this very department.

This might have offered an entirely new direction to my thought. Unfortunately, like nearly everybody else at the time, I had never heard about Sacks or his ideas. Moreover, given my background in the nineteenth-century theoretical sociology then fashionable in the UK, I probably would not have been interested anyway.

In June 1972, I met Sacks at the celebrated Edinburgh ‘Ethnomethodology and Symbolic Interactionism’ Conference. I remember the originality of what Sacks had to say (his lecture on dreams at that conference is not unlike the one reprinted in his published lectures, 1992, (2): 512–520). I can also recall my ex-tutor at LSE, Ernest Gellner, noisily walking out in disgust from the hall during Sacks’ talk. This culminated in a paper (Gellner, 1975) which Sacks’s colleague, Emmanuel Schegloff, fairly dismisses as ‘intellectually evasive’ (Sacks, 1992b: x footnote 2).

My contact with Sacks’s work was deepened by reading the photocopied versions of his lectures which circulated in the early 1970s. Sacks’s inspiration allowed me to turn full circle and to link my earlier theoretical interests with research practice. For Sacks re-opens a debate between ethnography and the nineteenth-century social theorist, Emile Durkheim (see also Gubrium: 1988). As an example of this, you might try to guess the author of the following observation: ‘Folk beliefs have honorable status but they are not the same intellectual object as a scientific analysis’.

If you guessed that the author was Emile Durkheim you were wrong – but also right – in the sense that the observation could be seen as being true to Durkheim’s dictum to treat ‘social facts’ as ‘things’. Its actual author was the anthropologist (and Sacks’s colleague) Michael Moerman (1974: 55).

Unlike Durkheim, ethnographers can take from Sacks a concern with understanding the ‘apparatus’ through which members’ descriptions are properly (i.e., locally) produced. And this message has been taken on board by sociological ethnographers who, like Gubrium (1988), are centrally concerned with the descriptive process.

In a series of ethnographies, Gubrium has shown how, in local environments, descriptions are co-operatively assembled. For instance, in an Alzheimer’s patients support group, one caregiver could be variously
described as an exemplary devoted wife or as a ‘second victim’ who showed the bad effects of ‘over-devotion’ (Gubrium, 1988: 100–101). In a residential treatment centre for emotionally disturbed children, ‘disturbance’ tended to be framed as a ‘therapeutic’ problem during the day shift and as a problem of ‘patient management’ at night (1988: 103–106). Finally, at a physical rehabilitation hospital, ‘progress’ was defined in ‘educational’ terms to patients who were told they could only get better through their own efforts. However, for insurance companies who were paying the bill for these patients, progress was related to medical interventions. Finally, for families of patients at this hospital, successful progress was related to medical interventions; lack of progress to lack of patient motivation (p. 107).

Although Gubrium’s work is clearly not conversation analysis, he does write ethnographies concerned with how descriptions are locally accomplished. Acknowledging his debt to Sacks, Gubrium shows how participants use time, space and audience to produce ‘sensible’ accounts.

If a case can be made out for Sacks’s influence on (some) ethnography, it would appear, at first sight, difficult to make the same case for psychology. After all, Sacks’s analysis of the sequential organization of conversation clearly reveals the inadequacies of any analyst’s attempt to treat any utterance as an expression of someone’s thoughts or, indeed, of any other apparently ‘psychological’ categories.

By contrast, in hearing how what they have just said is heard, speakers discover what they meant after they have spoken (for examples of this, drawn from AIDS counselling interviews, see Silverman, 1997: 78–84 and 100–106). The critical implications of this for any psychology of, say, ‘motive’ are effectively underlined by Heritage (1974: 278–9), who reveals the inadequacies of any social psychology which tacitly treats commonsense as both a resource and a topic.

More recently, Derek Edwards, while reiterating Heritage’s critique, has called for a psychology able ‘to follow Sacks and look at how people use categories interactively’ (1995: 582). Accepting that ‘talk is action not communication’, Edwards argues for a psychology which draws from Sacks and CA the assumption that: ‘No hearable level of detail that may not be significant, or treated as significant by conversational participants’ (Edwards, 1995: 580).

The direction in which Edwards wants to take psychology clearly leads towards what has become called Discourse Analysis or DA (see Potter, 2004). Writers who use the term DA have undoubtedly succeeded in showing the unlikely legacy that Sacks has given psychology. In a more obvious way, Sacks’s contemporary influence also extends beyond conversation analysis and into ethnography, whether located in Departments of Anthropology, Sociology or even Education (see Baker, 2004 and Freebody, 2003).
Despite this widespread influence, I suspect that the work of Harvey Sacks is often not even a minor component in contemporary social science courses. In part, no doubt, this reflects the unavailability of Sacks’s lectures in published form until 1992. However, I guess that it also reflects either plain ignorance in the social science community or straightforward prejudice against ‘another of those ethnos’.

Perhaps one way to make teaching Sacks more inviting to social scientists is to suggest that it might be a successful way to introduce more life into certain tired courses. Personally, I have never wanted to teach courses in social theory which, in my prejudiced way, I assume often contain empty syntheses, deadening critiques and the latest fashions in jargon. Certainly, the consumers of such courses respond as if this is what they have been taught.

How invigorating then to introduce some of Sacks’s examples on to such courses such as ‘the baby cried’ and the Vietnam pilot (Silverman, 2006: 181–94). Talking through such examples with students must surely convey a little of the liveliness of social theory and its potential to deal with the world around them. Even more obviously, it is difficult to see how a course on research methodology could not gain by using material from Sacks. In this sense, I would demand that Sacks’s writings should be basic reading on introductory classes in social theory and method.

As we shall see, Sacks recommended his method as a method anyone could use. In this sense, his lectures and other writings offer a toolbox rather than a museum exhibition. It is that toolbox which I hope to help make more widely known. Like my book on Sacks (Silverman, 1998), this work tries to reach a wider audience of scholars and students, particularly those who have never read Sacks, perhaps because they have assumed him to be ‘one of those ethnos’.

To such an audience, I want to show that there is no sectarianism or petty-mindedness in Sacks’s work. Instead, there is intellectual breadth and rigour. Whether or not we follow the path that Sacks sets out is perhaps less important than whether we respond to the questions that Sacks poses about social science. After more than thirty years they are, I believe, still vitally important and still largely unanswered.

Like only Wittgenstein before him (see Chapter 1), and nobody since, Sacks had the ability to turn the apparently trivial into the gripping and insightful. As with other important thinkers, we keep Sacks’s work vital by treating it as an inspiration or, more prosaically, as a toolbox. Indeed, the first three chapters of this book will have more than served their purpose if you are sufficiently stimulated to turn to Sacks’s own writings to locate Sacks’s legacy for yourself.
However, these biographical reflections should not mislead you into thinking that this is a book about Harvey Sacks or conversation analysis. What, then, is it about?

**Organization of this book**

Having provided some sort of context to what I am doing here, now is the time to lay my cards on the table and to offer you a brief chapter by chapter synopsis of what follows. There has been much discussion about whether the skills needed to do good qualitative research can be taught directly or can only be learnt through a long apprenticeship (Hammersley, 2004). This book is an attempt to hedge my bets in that debate. If my earlier textbooks suggested that direct teaching was sufficient (albeit aided by challenging exercises), the present volume attempts to convey some of the strategies and ‘tricks’ (see Becker, 1998) that I have learnt through my own long apprenticeship in the trade.

As I have already noted, research is worthless unless it recognises its theoretical assumptions. So the first chapter of this book shows you the kinds of theoretically-based questions that qualitative researchers can effectively pose. Using photographs and extracts from novels and plays, it reveals a tradition in which apparently routine activities can be made interesting and extraordinary things shown to have routine features.

Having seen what kinds of questions we can usefully ask, the next two chapters take you into nitty-gritty questions about the practice of qualitative research. It is a truism that data-collection methods can only be judged in relation to your research topic. However, in Chapter 2, I try to show why, all things being equal, it usually makes sense in qualitative research to begin with data found in the everyday world. This means that what I call ‘manufactured data’ (e.g., including interviews and focus groups) should be used only as a last resort – particularly where a ‘quick fix’ is more important than in-depth knowledge of some phenomena.

Whatever data-collection method we use, we need to be less precious about the sanctity of our ‘own’ data. Indeed, secondary data-analysis is a very important, if usually unacknowledged, method in qualitative research (see Corti and Thompson, 2004; and Akerstrom et al., 2004). Above all, it is the quality of our data-analysis rather than the source of our data that ultimately matters. This means that time spent gathering data and doing literature reviews should be far less than the time given to analysing data and writing up your conclusions (see Silverman, 2005). Chapter 3 develops a strategy that I believe is a key to success in effective data analysis –
avoiding appealing instances or examples and seeking out and analysing sequences in your data.

However, even if you do theoretically-inspired research with well-analysed data, people can still rightly ask: ‘so what?’ ‘Pure’ research is undoubtedly important but it should not blind us to the need to think through what contribution our research might make to ‘society’ and, indeed, what we mean by ‘society’. Fortunately, as I show in Chapter 4, when properly conceived, qualitative research has a unique contribution to make to our understanding of how things in society work and how they can be changed.

Chapter 5 offers another way of answering the ‘so what?’ question. It asks in what ways qualitative research demands attention and claims to be of value. Unlike Chapter 4, I am concerned here with what qualitative research is rather than with what it does. I look at the claims that contemporary qualitative research makes about itself and find some of them to be misguided. I conclude by proposing an alternative aesthetic justification for our trade which reminds us of what we share with our quantitative cousins.

One final word is in order. I have emphasized that what follows reflects my own views. Although I have not been wilfully controversial, I have fully taken up my Editor’s invitation to speak my mind. So you should not be surprised if some of my arguments do not fit neatly with what you read elsewhere or with what your teachers tell you. Throughout my academic career I have never sought converts but cherished students with a sparkle in their eyes who can think for themselves. So if I have given you pause to reflect, I will not be dissatisfied even if you end up taking positions quite opposed to mine.