

Introduction: Dentists, Dadaists or Discourse Analysts?

My starting point in this book is that qualitative research is currently facing a crisis. In part this derives from the fact that, in some fields, after a period of dominance, or at least of benign tolerance, it has recently come under increasing external pressure to demonstrate its value, and in particular its practical value for policymakers and practitioners of various kinds. Like other forms of publicly-funded activity, it is now being required to show that it 'adds value', and there have been attempts to steer funds back towards quantitative research, partly on the grounds that this alone can provide evidence of 'what works' in terms of policy and practice.¹

There are those who warn against exaggerating this threat. Thus, Atkinson and Delamont (2006) have pointed out that, despite these recent moves, qualitative work continues to be funded, and they also note the huge commercial success of ventures like the *Handbook of Qualitative Research*, now in its third edition (Denzin and Lincoln 2005). However, it seems to me that there is a danger of complacency here, in the face of powerful forces seeking to 'reform' social science in ways that will be detrimental to at least some kinds of qualitative work. But, in any case, the problems are deeper than this external threat. Indeed, in many ways they are symbolised by the very success of Denzin and Lincoln's *Handbook*. There are several aspects to this. One is the fact that qualitative inquiry now takes a wide variety of very different forms, supported by discrepant rationales. A second concerns the particular sorts of approach championed by Denzin and Lincoln.²

Many years ago, the American comedian Woody Allen wrote what he called a 'fantasy' entitled: 'If the impressionists had been dentists'. This is a series of

1 For discussions of these developments, see for example, Hammersley (2002:ch 1) and Biesta (2007).

2 While the *Handbook of Qualitative Research* is extraordinarily influential in many contexts, it is perhaps worth noting that there have been limits to the reach of its influence. These are not just to do with language barriers but also disciplinary fences. For example, while there has been considerable methodological debate within the fields of politics and international relations, and much promotion of the role of case studies (see Brady and Collier 2004; George and Bennett 2005), the *Handbook* rarely seems to be cited there.

imaginary letters from Vincent van Gogh to his brother Theo, in which he recounts his troubles as a dentist.³ The first letter begins:

Will life never treat me decently? I am wracked by despair! My head is pounding! Mrs Sol Schwimmer is suing me because I made her bridge as I felt it and not to fit her ridiculous mouth! That's right! I can't work to order like a common tradesman! I decided her bridge should be enormous and billowing, with wild, explosive teeth flaring up in every direction like fire! Now she is upset because it won't fit in her mouth! She is so bourgeois and stupid, I want to smash her! I tried forcing the false plate in but it sticks out like a star burst chandelier. Still, I find it beautiful. She claims she can't chew! What do I care whether she can chew or not! (Allen 1983:199)

To amusing effect, Woody Allen transposes the attitude or temperament of the artist on to a patently functional activity, dentistry. Putting it a little differently, he contrasts how we expect dentists to behave against modern (or, rather, modernist) ideas about the nature of art and the artist.

The relevance of this fantasy here is that, from a currently influential point of view, qualitative researchers could be, and to some extent are being, accused of misunderstanding their own role in much the same manner as this fictional van Gogh. A strong trend within academic qualitative inquiry over the past couple of decades, at least at the level of influential rhetoric, has been towards viewing it as a form of literature or art, with not just Impressionism but also Dadaism and Surrealism as important influences (see Chapter 7). And this trend is exemplified in the agenda of successive editions of the *Handbook of Qualitative Research* as well as elsewhere (Denzin and Lincoln 1994, 2000, 2005; see also Denzin 1997 and 2003; Lather 2007). However, as already noted, in the last few years of the twentieth century some powerful stakeholders began forcefully to reassert a view of social science as a functional activity. They have insisted that it should be designed to serve policymaking and practice directly; in other words, that it should be devoted to eradicating decay and promoting (social) health.

Of course, this conflict between Dada and dentistry by no means exhausts what is going on in qualitative research today. Indeed, we are now faced with a bewildering range of approaches, a situation of methodological pluralism or disarray. I will not attempt to map out all the available approaches in this book. However, I do discuss one other currently influential trend: towards the use of discourse analysis, particularly that concerned with detailed analysis of texts, usually transcriptions of 'naturally occurring talk' or of interview data. Not only has there been a considerable expansion in work focusing on discourse and narrative, but the ideas associated with some forms of discourse analysis have influenced qualitative research more widely. One of the most significant results is the raising of challenging questions about the sorts of data, the forms

3 This material is used by Dey (1993) in his guide to qualitative data analysis.

of inference, and even the kinds of conclusion common in much mainstream qualitative research.

In the opening part of this Introduction I will examine the demands for a functional, scientific approach to social science, then look at advocacy of 'Dadaist' qualitative research, and follow this up with an assessment of the claims of discourse analysts. In the final part of the Introduction there will be an outline of the contents of each chapter.

Qualitative inquiry as functional

Most qualitative researchers have probably regarded their work as relevant to policymaking and practice. However, much of it has been academic in character, in the sense of being aimed first of all at contributing to disciplinary knowledge. The primary audience here has been fellow researchers, despite hopes that it would reach more remote audiences and thereby shape policy and practice. As already noted, there have recently been influential demands, in several countries, that social science be much more directly policy- or practice-relevant. Furthermore, this has sometimes been associated with criticisms of qualitative research, on the grounds that it does not use what are seen as essential means needed for this purpose: the study of large samples so as to facilitate generalisation, the use of experimental designs to control for confounding variables, and so on. The emphasis, often, has been on the need to use randomised controlled trials and systematic reviews if we are to discover whether policies and practices have the effects claimed for them, and in order to check that they do not have serious negative consequences (see Oakley 2000, 2001; Mosteller and Boruch 2002; Slavin 2002).

The demand is that social science research should serve, *and demonstrate its contribution to*, evidence-based policymaking and practice. The other side of this is that policymaking and various forms of professional practice, especially in the public sector, should be based much more on, or more strongly informed by, research evidence. And it is argued that, if this is to be achieved, social science must be transformed to produce the kind of evidence that can provide an effective basis for decision-making by policymakers and practitioners.

Pressure in this direction emerged in the UK in the last years of the twentieth century, with severe public criticism, by powerful commentators, of research in the field of education (see Hammersley 2002:ch. 1). And qualitative inquiry was at the eye of this storm. It was charged by some with being largely irrelevant, weak in validity, and a waste of public funds. Furthermore, the pressure for research to be reformed spread outside the field of education. This was clearly evident in a speech by David Blunkett, then Secretary of State for Education and Employment, to the Economic and Social Research Council (ESRC) (Blunkett 2000; see Hammersley 2000).

More recently, a similar crisis has arisen in the United States; though there, as elsewhere, the roots can be traced further back. Once again the field of education has been the main target. In 1997 the National Institute of Child Health and Human Development published a report about teaching children to read, based on a meta-analysis of research involving highly restrictive criteria of inclusion modelled on those appropriate in medical research. Subsequently, the re-authorisation of the Elementary and Secondary Education Act ('No Child Left Behind') established randomised controlled field trials as the preferred methodology for federally-funded educational research. While the National Research Council's report on *Scientific Research in Education* adopted a more catholic approach, the framework was very much the logic of quantitative research (Shavelson and Towne 2002; see also Feuer et al. 2002). As a result of these developments, qualitative research has come under considerable threat in the USA, particularly as regards funding.

Denzin and Giardina argue that these developments are part of a broader trend:

Qualitative research exists in a time of global uncertainty. Around the globe, governments are attempting to regulate scientific inquiry by defining what is good science. Conservative regimes are enforcing evidence-, or scientifically-based, biomedical models of research. (Denzin and Giardina 2006a:ix-x)

Indeed, the shift towards a more functional conception of social research can be seen as integral to the emergence of so-called 'knowledge societies'.⁴ This involves a move away from what might be referred to as the patronage model for funding research towards the investment model. In these terms, research is no longer to be treated as a good thing in itself, with funds being provided by the State in the same way that they have long been for the arts, so that researchers are largely left to determine what is worth investigating and how this should be done. Instead, it is argued that funding research must be treated as an investment on which a satisfactory return is promised and delivered; and, along with this, that it should be highly responsive to external demand, whether that of the market or of democratically-elected governments.⁵

It is not hard to detect the influence of positivist assumptions about the nature of research behind these criticisms and proposals for reform. And they have stimulated vociferous protest from some qualitative researchers – who have dismissed them as 'regressive modernism', 'methodological conservatism' or 'methodological fundamentalism' (Lather 2004; Lincoln and Cannella 2004; Denzin and Giardini 2006b; Denzin et al. 2006).⁶ However, in my view this

4 On which see, for example, Gibbons et al. (1994) and Stokes (1997).

5 In the UK, and no doubt elsewhere, changes have also been taking place in state funding of the arts, in the same direction.

6 For some of the many responses of qualitative researchers to these developments, see Erickson and Gutierrez (2002); St Pierre (2002); Atkinson (2004); Bloch (2004); Maxwell (2004); Popkewitz (2004); Eisenhart (2005); Erickson (2005); Gee 2005; Howe (2005); Moss (2005a; 2005b); Schwandt (2005); Eisenhart (2006); Lather (2006); St Pierre (2006).

sort of dismissal is less than helpful: it is a mistake in intellectual terms, and probably unwise politically as well (Maxwell 2004). Above all, it discourages both a sober assessment of what is being proposed and careful examination of the criticisms that have been made of qualitative inquiry.

It is important to recognise that what is being demanded by those who would reform social science is not *simply* a reversion to the positivist past, nor is it a resort to irrationally held 'fundamentals'. Most new movements appeal to past ideas in one way or another – nothing is completely new. Moreover, the position of some advocates of reform can be seen as politically progressive in key respects.⁷ An example would be Ann Oakley's arguments (Oakley 2000). These challenge professional dominance – which, in recent times, sociologists have portrayed as the core of professionalism (see, for example, Larson 1977; Macdonald 1995) – in the name of democracy. She insists not simply that the evidential basis on which policymakers and practitioners make decisions should be scientifically sound but also that it be made publicly available, so that the decisions can be assessed by those on the receiving end. An important requirement for this, she argues, is that the research supplying the evidence must itself be methodologically transparent.

However, while the advocates of research-for-evidence-based-practice may be neither conservatives nor fundamentalists, they do suffer from either amnesia or ignorance. They neglect the cogent implications of much work in the philosophy of science, and in social research methodology, over the past fifty years. As a result, there are serious problems with this neo-positivist view of the function and nature of research. For all its virtues, the randomised controlled trial is unable to deliver what it is held to promise, as regards most research questions in the social field. Not only does it not control all confounding variables, but its use will not be effective unless what is being investigated is a clearly distinct and standard 'treatment'. And this is rarely the case once we move away from drug trials. Furthermore, it does not avoid the severe problems of measurement that have always been a weakness of most quantitative research in the social sciences (see Howe 2004 and Hammersley 2005b). Similar problems arise with 'systematic' reviews (Hammersley 2001 and 2006b).

I am not denying that the research methods typically advocated in the name of 'evidence-based practice' can be worthwhile, but I *am* arguing that the claims made for them in the context of reforming social science are often excessive: they can neither provide absolutely certain knowledge about the effects of particular treatments nor tell us what is good policy or practice. All methods produce fallible knowledge; and while experimental methodology has distinctive strengths, like other methods it also involves some serious threats to validity. Similarly, proceduralisation of research does not and cannot produce

7 As indeed was early positivism, see Hammersley (1995:ch 1).

'transparency'. Finally, judgements about what is and is not desirable policy and practice necessarily depend upon evidence from more sources than research, and involve value assumptions whose justification research cannot provide, at least not on its own.

Some advocates of research for evidence-based policymaking and practice recognise this, at least some of the time. They acknowledge that the range of useful methods transcends the quantitative–qualitative divide. And there have been attempts to promote the value of qualitative work in the context of arguments for evidence-based policymaking and practice (see, for example, Davies 2000). Moreover, there has been quite a lot of qualitative work that is close to the functional model, in the sense that it is aimed at addressing quite specific, practically-relevant questions – for example in the fields of policy and programme evaluation and the gauging of political and social attitudes (see Walker 1986; Rist 2000; and Ritchie 2003).

There is, though, a more fundamental question than whether qualitative work can be functional. This is whether social research can or should have this character. Often, discussion of this centres on a contrast between what have been referred to as the engineering and enlightenment models (see Bulmer 1982; Hammersley 1995:ch 7 and 2002:ch 6). And, in the current external climate, the pressure seems to be all in the direction of the engineering model. In my view, it is important to recognise a distinction between academic and practical research, to treat both as of value, to stress their interdependence, and to insist that neither can substitute for the other. In particular, it is important to emphasise that academic research develops, refines and assesses ideas and methods in ways that are not usually possible in the practical context; and that are of great importance if we are to gain sound understandings of social phenomena. Furthermore, the production of knowledge relevant to human concerns and interests is worthwhile in itself, irrespective of whether it is found useful at any particular time, serves national or 'emancipatory' interests, and so on (Hammersley 1995: ch 7 and 2002). Currently, there is a danger that funds will dry up for research that is not practice- or policy-focused, or that cannot at least convincingly pretend to be; and this is a particular problem for qualitative work.

At the same time, it seems to me that the attitudes of some of the most vociferous defenders of qualitative research against recent criticism pose at least as serious a threat to genuine academic qualitative inquiry as that coming from those they dismiss as methodological fundamentalists or conservatives. This is because their defence relies, to a considerable extent, upon ideas that are fundamentally mistaken, often deriving from what is frequently labelled 'postmodernism'.⁸

⁸ This is a problematic term, one that has come to be used by many commentators in a dismissive way. I have tried to be as clear as possible about what I am taking it to refer to, while recognising that its meaning is quite vague.

A Dadaist alternative?

As I explain in Chapter 7, an important and sometimes neglected influence on postmodernist ideas has been art movements like Dada and Surrealism. Dada was an attempt to subvert art, and thereby society, to shock it into being something different. Today, in much the same way, some forms of postmodernist-inspired qualitative inquiry are concerned with subverting the notion of social science, in the apparent (perhaps Foucaultian) belief that this challenges wider forms of power. Furthermore, art and literature are sometimes believed to embody a kind of alternative ethos that is celebrated as running counter to what are seen as scientific forms of governance dominant in the West. In fact, the aim, often, is to challenge precisely those kinds of governance that lie behind calls for evidence-based practice.

As with Dadaism and Surrealism, in postmodernist writing there is a turn away from 'Western' notions of rationality, and an appeal to what these are taken to exclude: 'indigenous' or non-Western modes of thought and belief; non-representational forms of art; reliance on the intuitive, the unconscious or sheer chance; intense religious experience; and so on. The model of science, in particular the image of the scientific research report as clear and authoritative, is rejected in favour of modes of representation – for example, collage, poetry, and drama – that allow or encourage diverse interpretations on the part of audiences. Furthermore, the impersonal stance of science is replaced by a celebration of acknowledged subjectivity and ethico-political engagement.

The starkness of the conflict between a functional conception of social science and an 'artistic' interpretation of qualitative inquiry of this kind can be pointed up by Hughes' commentary on an exhibition concerned with 'Surrealist design'. He suggests that this phrase 'seems almost a contradiction in terms':

'Design' is for us strongly identified with industrial process, with modules, with the rationalisation of process into clear repeatability. To 'design' something implies that it can be made not just once, but again and again and again, without loss of quality and intensity [...] That an object is 'designed' implies, or seems to, that every aspect of it from the first pencil scribble to the finishing touch and on to its intended use by the proposed consumer has been thought about and brought into full consciousness. It would therefore seem so remote from the spirit, the *modus operandi*, of surrealism as to have nothing to do with it. (Hughes 2007:12)

Perhaps what is hoped, in the context of qualitative research methodology, is that an approach modelled on art can provide an alternative version of social design, one that escapes the mechanical and technical imagery that now governs policy discourse. Furthermore, it seems to be believed that the ethos which informs this alternative is intrinsically egalitarian and anti-imperialist in character.

What has come to be referred to as 'postmodernism' in many Anglo-American contexts, including in qualitative researchers' writings about methodology, draws its main ideas, of course, from influential French philosophy of the 1960s and 1970s. While by no means homogeneous or internally coherent, this challenged those forms of thought that had previously been dominant in French intellectual life, notably phenomenology, existentialism, humanistic Marxism, and structuralism. At the same time, there was also a significant inheritance from these sources. In particular, a great deal was taken over from structuralism.

Structuralists had reacted against the subjectivism of phenomenology and existentialism; notably, against their grounding of knowledge and reality in the immediacy of experience or the will of an individual subject. Instead, structuralists argued that what people perceive, what they desire, and so on, are constituted by underlying structures, whether those belonging to a universal human mind (Lévi-Strauss), a particular kind of culture (Barthes), or a particular type of society, such as capitalism (Althusser, Godelier). Post-structuralism inherited this displacement of the Subject, including rejection of any notion of a collective historical subject – for example, the idea that the working class can be seen as embodying the spirit of History under capitalism. This was a notion that had been characteristic of much twentieth-century Western Marxism, reflecting the influence of Hegel. The 'anti-humanism' of structuralism amounted to an abandonment of the idea that there is a teleology built into history: that the process of historical development reflects an unfolding of true human nature. For Lévi-Strauss, for example, the diverse social forms to be found across history and in different geographical areas simply represent realisations of the limited possibilities that can be generated by underlying cognitive structures. While he has a view of the long-term history of humanity, representing something of a golden age mythology, this is very far from any Hegelian teleology. Moreover, while some post-structuralists, notably Foucault, have been interested in the 'archaeology' and 'genealogy' of particular social forms, they treat shifts from one to another as largely contingent in character, rather than as part of any grand meta-narrative of self-realisation.⁹

What the post-structuralists rejected in structuralism was the assumption that there is a coherent set of underlying forces generating unified patterns of human consciousness and social life, *and the idea that these can be identified by scientific means*. They argued that what needs to be recognised is the fragmentation of, and tension within, human experience and cultures. Furthermore, they emphasised that philosophers, writers, and social scientists are themselves subjected to forces that generate fractures and fissures, rather than being Subjects

9 Though in interviews in the wake of may 1968 he did offer the prospect of escaping structural constraint, see Turkle (1992: 78).

who are in control of a single, coherent discourse. Thus, any idea that it is possible to produce a stable, genuine, complete representation of the social world can only be an illusion, and/or a rhetorical strategy designed to secure power through claiming expertise. For this reason, they rejected expert claims to knowledge about the world, and particularly those which offered a comprehensive theory, as not simply false but also oppressive. They saw this kind of scientific theory as the cause of what had happened in the Soviet Union – notably the Gulag. And they treated this as a product of Marxism, not just of communism, to which many on the French Left had long turned a blind eye (see Khilnani 1993; Christofferson 2005). In other words, it was not just that there could be no scientific politics, but also that the attempt to apply science to politics (or to life more generally) carried totalitarian implications. At the same time, they denied that intellectual work could ever be without ethical and political assumptions, implications and consequences.

The reception of these ‘postmodernist’ ideas in Anglo-American social science has been highly selective. The treatment of literature and art as a model for thought, rather than science, has been widely adopted by qualitative researchers.¹⁰ However, this valorisation of literature and art has been less discriminating than in the case of French post-structuralists; in the sense that it has adopted forms that are rather conventional in artistic terms. The emphasis on the politically reflexive character of all thought – that it is part of and affected by those situations of which it tries to make sense – has also been very influential, along with an associated radical epistemological scepticism or relativism. From this point of view, thought cannot but be political, and should be consciously directed towards political goals. However, the political orientation of postmodernist qualitative inquiry has been rather different from that of much French post-structuralism. It has not usually been concerned with challenging Marxism, but rather those kinds of liberalism that prevail in the USA and Britain. And the framework from within which this criticism comes is a form of Leftism that shares much in common with the humanist Western Marxism, especially Critical Theory, that post-structuralists were concerned to reject at least initially.

¹⁰ There is an interesting historical parallel to this. In seventeenth-century England a move by Philip Sidney and others towards making poetry a practical art that is concerned with the inculcation of virtue preceded Bacon’s reorientation of natural philosophy into a form that was to be useful to the state (see Gaukroger 2001:ch 2). Of course, the words ‘poetry’, ‘art’, ‘philosophy’, and ‘science’ had rather different meanings then. Furthermore, the guiding ideas were also different. The great value of poetry, according to Sidney, was that whereas natural philosophers and historians could only describe the corrupt world that existed after the Fall, poetry as fiction could invent a nature which more closely mirrors eternal archetypes, conceived in Platonic as well as Protestant terms. Furthermore, for him, poetry does not simply tell people about virtue, it moves them to virtuous action.

In methodological terms, what postmodernist interpretations of qualitative inquiry have, in particular, are science's emphasis on seeking to test ideas against empirical evidence; its ideal of clear, 'plain' argumentation; and the notion that the purpose of inquiry is a 'value-neutral' pursuit of knowledge. The notion of evidence has been questioned on the grounds that it implies the 'metaphysics of presence': that there is some form of data that is simply given directly to us by an independent reality, and whose validity we can therefore treat as absolutely certain. Similarly, drawing on post-structuralist notions of language as necessarily unstable, constitutive and perhaps also deceptive, it has been insisted that any attempt to privilege clear argument that states conclusions and grounds for them is ideological in character. It is held to be a falsification of how discourse operates, and one that has negative political or ethical implications and consequences. Finally, any idea that research can produce knowledge that is superior to the experience of ordinary people, particularly those who have been socially marginalised or oppressed, is challenged as elitist, this challenge drawing on the resources of epistemological scepticism and relativism mixed with standpoint theory. And it is claimed that all forms of research, including – in fact, perhaps especially – social science, are themselves implicated in processes of marginalisation and oppression; that, in fact, this is built into the modes of thinking and writing on which they depend.

Now, there are serious problems with all this. First, these arguments are founded on some very questionable epistemological assumptions: ones that have long been subjected to extended discussion in the philosophical literature, and which do not so much re-orient qualitative inquiry as turn it into a form of politically-inspired literature or art. Moreover, it is not clear what arguments the advocates of this sort of postmodernism could consistently put forward, in their own terms, to justify what they propose and practice, or how they could believe that these would be found persuasive by others. How can they avoid assuming precisely what they purport to abandon, for example the idea that their position is in some sense superior to what it opposes? Even if we put aside this performative contradiction, their specific arguments are rarely convincing. They frequently take for granted precisely what those who disagree with them need to be persuaded about. For example, they treat it as axiomatic that research is inevitably political and partisan and should be explicitly so; or that the corrigibility of evidence means that it cannot provide a means of testing our ideas about the world. Yet there are well-known arguments in the philosophical and methodological literature which throw doubt on both these assumptions (see Hammersley 1995 and 2000).

More sophisticated exponents of postmodernism argue that it does not offer a new alternative approach but is necessarily parasitic on mainstream forms of qualitative work. In other words, its role is as a corrective, or perhaps even as a laxative – serving to free up the thinking of qualitative researchers in the face of an inevitable drift towards closure, towards specifying 'methods', and producing overly definitive conclusions.¹¹ However, while there is undoubtedly the

occasional need for such medicine, it is unclear why postmodernism is required for this purpose – there is plenty of scope within mainstream ideas about social science for generating scepticism and creativity. Furthermore, using postmodernism in this way amounts to a celebration of excess that kills rather than cures the patient.

The final point I want to make here, all too obvious I would have thought, is that this postmodernist image of qualitative inquiry is not only ill-conceived but its prominence at the present time, not least in arguments against what it dismisses as methodological conservatism, is potentially very damaging – not just to qualitative research but to social science more generally. This is because it is, quite literally, indefensible; and also because the polarisation it promotes encourages precisely the neo-positivism it complains of. Unless this dynamic can be interrupted, the future of qualitative research is endangered (Hammersley 1999:584).

A central element of postmodernist-inspired qualitative research is what has come to be called constructionism, the idea that multiple realities can be, and are, constituted through discursive practices. Under the influence of postmodernism, this often stimulates a radical reflexivity in terms of which social scientists must continually attend to and display how their work is constructing the social world. However, within a different version of qualitative inquiry, also highly influential today, constructionism is taken to carry somewhat different implications: it demands analytic explication of the discursive practices that people employ in their everyday lives. Here, often, commitment to science is retained, albeit modelled on phenomenology or linguistics rather than natural science. The explicit aim is to describe rather than to explain.

Discourse analysis

The term ‘discourse analysis’ covers a variety of approaches.¹² But the distinctive identity of some of these, the ones I will focus on here, arises from an insistence on detailed analysis of transcriptions of audio- or video-recordings, and perhaps also the presentation of these data in research reports so as to allow readers to evaluate the analyses presented. Furthermore, closely associated with this is a denial that we can rely on interview or documentary data to draw inferences about ‘the world outside’ these texts. While some discourse analysts employ interview data, in doing so they are solely concerned (purportedly at least) with examining the discursive resources and strategies displayed in these data – rather than with drawing inferences either about the people and places referred to or about the attitudes and perspectives of the interviewees.

11 This, for example, seems to be the view adopted by MacLure (2003 and 2006).

12 For an outline and guide to the literature, see Hammersley (2003b). See also the appendix to MacLure (2003).

There seem to have been two, in some respects at least partly overlapping, pressures behind the turn to studying discourse within qualitative research in the second half of the twentieth century. First, there was the desire to capitalise on what was perceived to be an increased analytic rigour made possible by the availability of portable means of audio-, and later of video-, recording. These devices transformed the sort of data that qualitative researchers could rely upon – previously fieldnotes had been used to record both observations and interview responses. Transcripts of electronic recordings made clear the degree to which fieldnotes were a highly selective source of data that often contained descriptive errors. With extensive audio- and video-recordings, the data became more detailed and more accurate, for example in terms of capturing the actual words used. One effect was to bring home the complexity both of social interaction in the settings observed and of informants' answers in interviews, and this – along with the opportunities afforded by detailed transcription – encouraged much more micro-focused analysis than previously.¹³ Indeed, there has probably been a tendency not only to use these new means of data recording wherever possible, but also to select those research foci and settings of inquiry that allow their use. Above all, what audio-recording enabled was analysis of the actual language used: whole stretches of what had been said by the people studied could now be 'preserved' in transcripts in a way that had not been possible in writing fieldnotes. This was the new opportunity that discourse analysis exploited.

The other driving force behind the turn to discourse, as already mentioned, was the influence of constructionism, interpreting this term broadly. In early usage, it referred to little more than an emphasis on the active role that human beings play in making sense of their surroundings, devising forms of action, and thereby producing as well as reproducing social institutions (see Berger and Luckmann 1969). However, constructionism of this kind later came to be transformed through contact with influential linguistic turns in twentieth-century philosophical thought. This led to the conclusion that social phenomena are brought into being as particular types of phenomenon (situations, actions, events, institutions, etc.) through discursive practices. Within social science, this sort of constructionism was influenced both by phenomenologically-inspired ethnomethodology and by structuralism and post-structuralism; though the balance of influence between these very different traditions of thought has varied considerably across forms of discourse analysis.

What is distinctive about constructionism here, then, is that it takes the view that social phenomena can only be understood by describing *the processes by*

13 I am not suggesting that this is the only possible consequence of relying on transcriptions of electronic recordings. For example, Clifford has suggested that it 'can produce a more polyphonic final ethnography' (Clifford 1990:57).

which they are culturally constituted as the things they are. What is involved, if this approach is followed through consistently, is a fundamental re-specification of the goal of inquiry from that which is characteristic of mainstream social science. The focus becomes, not the phenomena themselves, but rather the processes by which they are discursively produced. This shift can be illustrated by looking at changes in the sociological study of social problems influenced by constructionism. Where, previously, sociologists had studied social problems by investigating what factors generated them – the causes of crime, of financial corruption, of environmental damage, etc. – social constructionists turned their attention to *how such phenomena come to be defined as social problems*. Initially this involved a focus on the role of various pressure groups in promoting particular social problems and the strategies they employ, including rhetorical ones. But, later, for some writers, the topic shifted entirely to the discursive practices involved in formulating particular actions or practices as problematic, as needing remedy, as being the product of certain causes, and so on (Holstein and Miller 1993).

Constructionism also raises questions about the sorts of data that many qualitative researchers have routinely used in the past, and continue to use today; and, even more significantly, about the kinds of inference they employ in analysing those data. This has been most obvious in disputes over the use of interviews (see Chapter 5), but the implications extend more widely. Much qualitative inquiry has employed data to draw conclusions about what happens in particular types of situation, what the orientations of particular types of actor are, what strategies they use, what consequences these have, and so on. But constructionists question the validity of such inferences, arguing that the focus ought to be, and for reasons of methodological rigour *must* be, on the discursive practices that can be identified in transcriptions of talk or in documents. Inferences beyond these to phenomena ‘behind’ the talk or the writing, including motives and interests, are often ruled out on epistemological and/or ontological grounds.

The influence of ethnomethodological conversation analysis is particularly important here because of its explicit attempt radically to re-specify the focus of qualitative inquiry (Button 1991; Button and Sharrock 1993; Watson 1994). This amounts to a shift away from investigation of the features and causes of particular events, actions, and institutions towards a focus on the methods or practices that are held to generate whatever intelligible sense participants (and, for that matter, researchers) make of them. Furthermore, these methods or practices are assumed to be involved not just in the recognition of social phenomena but also in their production, in ‘bringing off’ events, actions, and institutions as of distinctive kinds.

Ethnomethodological conversation analysis has had considerable influence within the broader field of discourse analysis, albeit often in watered-down form and mixed with other sets of ideas. Sometimes it has been combined with

epistemological scepticism, such that events, actions, and institutions are held to exist only in accounts of them; though, inevitably, this scepticism has usually been only selectively applied. At the same time, and sometimes by the same people, constructionist analysis has often been inserted into more or less taken-for-granted accounts of the nature of the wider society (see, for example, Wetherell and Potter 1992). Here, to some extent, its function becomes parallel to what previously would have been called ideology critique.

It is certainly true that discourse analysts have pointed to some serious problems with older forms of qualitative research. However, neither the criticisms nor the means adopted to avoid these problems are entirely convincing. For example, conversation analysis makes appeal to the concrete reality and representational capacity of transcripts or audio-recordings in ways that would be difficult to defend on its own terms (see Chapter 6). Moreover, if applied consistently, ethnomethodological arguments undermine even the work of most conversation analysts (see Lynch 1993). It is also important to recognise the cost of the sort of refocusing of social science that ethnomethodology recommends. This undermines the traditional ethnographic concern with documenting the character of events and actions in particular times and places, and the attempt to locate these within some picture of the wider society. Also threatened is the idea that qualitative research is able to capture differences in people's perspectives and orientations. Indeed, virtually the whole enterprise of traditional social science is put in question. Of course, if the arguments were sound, then these costs would have to be borne; but they are not.

As yet, at least, most qualitative researchers have not become discourse analysts – instead, they have combined an increased interest in discourse and its constitutive character with a continuing commitment to studying particular people and situations, sometimes giving more emphasis to the 'voices' of those whom they study than previously. However, in some important respects, this sort of eclecticism simply compounds the problems.

The conflicts between a functional and an aesthetic conception of social science, between dentistry and Dadaism as models, and between discourse analytic and older approaches to qualitative research, form the background to this book. Of course, these models do not represent ends of single-stranded, clearly-defined continua. Rather, they are positions in a complex, multi-dimensional, terrain; one that is by no means fixed in its geography. While there is no justification for the kind of bulldozing of it that seems to be proposed by some would-be reformers of social science, in my view there is certainly a need for some landscaping, albeit more on the model of a *jardin anglais* rather than that of a French formal garden!

Reactions to the current methodological pluralism vary sharply: from 'let a thousand flowers bloom' to the idea that we can draw everything together into a 'third way', characteristic of some advocates of mixed methods (see Tashakkori and Teddlie 2002). Above all it seems to me essential that we try to

learn from the different approaches to qualitative research that have been developed, while at the same time engaging in robust assessment of them. This is the task to which this book is devoted. Without wishing to over-dramatise, in my view the survival of social science depends upon our capacity to make sound and realistic assessments of its current state and of how it should develop. The need for this applies as much to quantitative as to qualitative work; indeed, the distinction between the two approaches itself demands scrutiny. However, in this book I restrict my focus to qualitative research.

The chapters

Chapter 1 looks back at the rise of qualitative research over the second half of the twentieth century, focusing on some key respects in which it has failed to realise its potential. I argue, first of all, that it has not fully lived up to the claims made for it in early battles with quantitative researchers. Advocates argued the superiority of qualitative work on the grounds that it offered genuine understanding of people's perspectives and actions, and took proper account of the processual character of human social life. Yet, in both these areas, the achievement has been much less than promised. Secondly, I argue that qualitative research has failed to develop adequate responses to the criticisms made of it by quantitative researchers: as regards the need for measurement, for causal validation, and for generalisation. I examine the standard responses by qualitative researchers to these criticisms and argue that none of them is very effective. In the course of the discussion I suggest some reasons for the two sorts of failing I have identified, and explain why they are significant.

Chapter 2 takes up one part of this, looking at the argument, frequently used by qualitative researchers, that their approach is superior because it captures the complexity of the social world, as against the oversimplified pictures provided by general theories and the reified measurements generated by quantitative research. I argue that all analysis involves both theoretical abstraction and data reduction. Moreover, even if it were possible, simply displaying reality in all its complexity would not serve the goal of research, or any other purpose. This does not mean that qualitative researchers are wrong about the failings of quantitative work, or that the strategies they use to deal with complexity are entirely ineffective. But it does mean that more thought needs to be given to what is involved in the tasks of describing, explaining, and theorising social phenomena in the context of qualitative inquiry.

The next two chapters follow on from this, examining two influential approaches that were designed to enable qualitative inquiry both to respect complexity and to generate theories about the social world. The first of these, Clifford Geertz's notion of 'thick description', has come to be very widely cited in recent times. However, the label is often used in a ritualistic way – to

mean little more than a description in which details of the character of a particular event, situation, or person are woven together with more abstract theoretical ideas, usually derived from the relevant literature. In Chapter 3, I examine what Geertz meant by 'thick description', and what he drew from the term's original use by the philosopher Gilbert Ryle. I also assess how far it represents an adequate solution to the problem of dealing with complexity. I conclude that it largely fails to do this, for all its suggestive value in indicating what sort of description may be required if we are to understand many aspects of human social life. This is because, while Geertz acknowledges the fundamental dilemmas that face social science, he does not tackle them effectively.

In Chapter 4, the focus is on analytic induction. This too starts from the complexity of actually occurring social phenomena, but in sharp contrast to thick description it is aimed at the systematic production of theory that has the character of specifying universal, albeit conditional, laws. Where much ethnography has been concerned with studying particular cases in their uniqueness, albeit at the same time both using and contributing to more general ideas, analytic induction has quite the opposite concern: in most versions, idiosyncratic aspects of particular cases are eliminated in order to discover the essential features that mark them out as belonging to causally homogeneous categories. Of course, the term 'analytic induction' has been used in several different ways, and I argue that even the most useful discussions of it (from Znaniecki to Becker) involve some interpretative puzzles and unresolved problems. My aim here is to spell out the logic of analytic induction and to assess its value as a model for qualitative analysis directed at producing theories.

In the next couple of chapters, the focus switches to issues raised by discourse analysis. As we saw earlier, the approaches coming under this heading have generated some fundamental criticisms of established qualitative research practice. One area where the challenge has been sharpest concerns the use of interview data. This is ruled out by some forms of discourse analysis; and, even where it is not, the uses to which such data can be put are highly restricted. The arguments here have come to be labelled 'the radical critique of interviews'. While, previously, qualitative researchers, like other social scientists, had used interviews to draw on informants' knowledge about the people and situations being studied and to document the perspectives that shape their behaviour, it is now commonly argued that these uses are illegitimate. Instead, at most, interview data must be examined solely for what they can tell us about the discursive practices or kinds of interactional performance that take place in interview (and perhaps other) situations. In this chapter, co-authored with Roger Gomm, this radical critique is outlined and assessed. It is argued that while it points to important dangers associated with exclusive reliance on interview data, the grounds for restricting the use of such data so narrowly are not convincing.

Chapter 7 examines some of the ideas that stimulated this 'radical critique' and the forms of qualitative research they recommend. The focus is twofold:

conversation analysis, and the seminal form of discourse analysis initially proposed and practised by Potter and Wetherell. Both of these are usually treated as self-sufficient approaches to studying the social world, rather than as mere methods that can be combined with others. And there are two areas where their conflict with other approaches is clearest. First, they reject the attribution of substantive and distinctive psycho-social features to particular categories of actor as a means of explaining human behaviour. Secondly, they abandon the use of what people say about the world as a reliable source of information for analytic purposes. These two negative commitments mark conversation analysis and discourse analysis off from almost all other kinds of empirical research in the social sciences. In this chapter, I consider how sound the justifications are for these radical methodological commitments. I conclude that, while these forms of research have made important substantive contributions, they involve serious problems and that neither approach should be treated as a self-sufficient paradigm.

In most forms of discourse analysis, a constructionist interpretation is applied to the phenomena being investigated, *but not to the research process itself*. If this further step is taken, as in the case of some ethnographic postmodernism, any notion of research as producing knowledge about independently existing social phenomena is undercut: it becomes a form of imaginative literature or political rhetoric. I outlined this development and indicated the problems with it earlier in this Introduction. In Chapter 7, I develop further the argument that it draws much of its inspiration from Dadaism and Surrealism, via French post-structuralism and the work of anthropologists associated with *Writing Culture* (Clifford and Marcus 1986). I suggest that despite some watering down and mixing with other sources, this kind of qualitative research inherits some fundamental misconceptions from post-structuralism, and that it amounts to an aping of work in the arts and humanities in much the same manner that earlier social scientists sought to mimic the work of natural scientists. Moreover, it is, if anything, even more dangerous since what is being treated as a model is not a form of inquiry, but quite a different enterprise. The result is that, in effect, postmodernist work amounts to the abandonment of research.

In the past few decades, considerable attention has been given to the rhetorical dimension of the texts that social researchers produce, particularly qualitative work. This is the focus of the next chapter. Some of this literature mounts a critique of the realism that has previously been integral to research writing. A key argument here is that, contrary to what is often claimed for it, such writing employs rhetorical devices, rather than using language that is transparent. Moreover, many of these devices are the same as those that can be found in realist novels, where the realities produced are knowingly fictional. Under the influence of post-structuralist and constructionist ideas, the critics argue that social science similarly creates the realities it purports to describe and explain, rather than simply capturing or representing them. Given this, it is sometimes

concluded that the use of a realist writing genre is a form of deception or inauthenticity, one that is geared to establishing and maintaining the authority of social science, while perhaps also serving to reproduce the socio-political status quo. Furthermore, on the basis of these ideas some commentators call for social research to be a *just* rhetoric, a rhetoric designed to promote social justice – for instance, by challenging expert claims to knowledge and thereby opening up the space for marginalised or subordinated discourses to be heard. In Chapter 8, I assess the significance of the rhetorical dimension of research, and challenge this anti-realism. In doing so, I focus on three issues: the differences between inquiry and other activities that produce written texts; the relationship between rhetoric and truth; and the question of the legitimacy or illegitimacy of particular forms of research rhetoric. In the course of the chapter, I compare this more recent work on research texts with the longstanding discipline of Rhetoric, which dates from the time of the Sophists in ancient Greece.

The final main chapter addresses the perennial issue of the criteria by which qualitative research should be evaluated. At the present time, there are powerful demands for ‘transparent’ criteria, for example in order to serve systematic reviewing and evidence-based practice. However, some qualitative researchers have argued that criteria are neither possible nor their pursuit desirable. At issue here, in part, is what the term ‘criterion’ means, and what role criteria could play in the context of qualitative inquiry. I suggest that they cannot amount to ‘transparent’ procedures which can be simply applied, and that a distinction must be drawn between the standards according to which research should be assessed and the considerations that need to be taken into account in assessing it. Furthermore, attention ought to be given to the different aims that assessing research can have: whether the focus is on the validity of the findings, the adequacy with which the research is presented, the value of the methods used, the competence of the researcher, and so on. Different standards will apply, and different considerations will be relevant, in each case. Equally important, though, is the question of whether a single set of criteria, guidelines, or principles is possible across qualitative research – given the methodological pluralism currently found within it and the fundamental areas of disagreement this reflects. Divergent approaches are framed, in part, by value assumptions about what is and is not worth investigation. In addition, there are differences in general methodological orientation: over what counts as rigorous inquiry; between realist and constructionist assumptions about the nature of social phenomena; and over whether the goal of research is to produce knowledge or to serve other purposes. The chapter concludes with a brief discussion of responses to this methodological pluralism, and their implications.

The book ends with a short epilogue addressing the conclusions that might be drawn from questioning qualitative research in the ways that have been done here. I suggest that there is a need for moderation in the claims made for

qualitative inquiry, and for social science more generally, that tough decisions need to be made about what it can and cannot achieve, and that there must be sustained attempts to resolve the difficult methodological problems it faces. It seems to me that many qualitative researchers overestimate the success of their enterprise, and underestimate the seriousness of its problems. The main purpose of this book is to raise some of these problems and to assess the attempts that have been made to deal with them.