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On the Failings of Qualitative Inquiry

My focus in this first chapter is on how qualitative research has developed over the past forty years. I examine what many of us claimed for it, and how far it has lived up to these claims. In effect, then, what is offered is an internal critique of qualitative research, one which assesses it according to the clarion calls sounded in the 'paradigm wars' of the 1960s, 1970s, and 1980s. It is important to remember that at that time there was a great deal more consensus about the nature of qualitative research, and why it was desirable, than there is today. While I write as someone who has worked in a particular country (the UK), a particular discipline (sociology), and for the most part in a particular substantive area (education) these were, to a considerable extent, in the vanguard of the shift towards qualitative method in the second half of the twentieth century (Atkinson et al. 1993). So I believe that the arguments I present here have wide relevance. Readers can, of course, judge this for themselves.

The emergence of qualitative research as a distinct approach to social science was often portrayed by its advocates as a 'paradigm change' or 'scientific revolution' of the kind that Thomas Kuhn had outlined in his historical and philosophical work on the development of natural science (Kuhn 1970). Kuhn described scientific revolutions as historical moments when it was widely recognised by researchers in the relevant field that a previously dominant paradigm had severe problems, and a new paradigm had appeared on the horizon that some believed to be superior. He argued that there was little likelihood that the disagreement between defenders of the old paradigm and advocates of the new one could be resolved through discussion. This was not just because there is never any *logical* or *empirical* means whereby the superiority of one paradigm over another can be demonstrated, but also because at the time of a scientific revolution insufficient intellectual resources are available even for effective reason-based persuasion. Instead, necessarily risky assessments have to be made; and, where a revolution succeeds, the outcome is partly determined by defenders of the old paradigm dying off and the emerging generation of researchers taking over. So, Kuhn argued that, during the throes of a scientific

revolution, there can be reasonable disagreement among scientists in judge-ments about the potential of the new paradigm. Achieving a reason-based con-sensus is only possible once further work has been done within the framework of the new paradigm, so that its potential can be properly assessed. (Even then, agreement is not guaranteed – the superiority of one paradigm over another still cannot be *demonstrated* by either logic or empirical evidence.) What is involved in this process of assessment is discovering both whether the new par-adigm resolves the anomalies that had emerged in the old one, and whether it opens up new fields of productive puzzles for normal science to pursue.

Kuhn's work had a huge influence on qualitative researchers in the third quarter of the twentieth century, despite the fact that he explicitly excluded the social sciences from his account, viewing them as pre-paradigmatic at best. Moreover, his work was often misinterpreted.[1] For example, it was treated as showing that even the truth of a natural scientific theory is relative to the par-adigm in which it was developed, so that it may be false from the point of view of a different paradigm. And it was taken to follow from this that the decision to adopt a particular paradigm is necessarily arbitrary: a matter of non-rational commitment, since what is and is not rational is always defined *within* a paradigm. Furthermore, the meaning of the term 'paradigm' was frequently extended by qualitative researchers to include political and ethical assumptions, not just theoretical and methodological ones.

While Kuhn's perspective was not intended to apply to social science, and was often misinterpreted, it is still worth looking back on forty years or so of qualitative research to consider how far it has succeeded in resolving the prob-lems that it initially identified as intrinsic to 'the quantitative paradigm'. As I have already indicated, Kuhn did not deny that paradigms could be evaluated, only that this could not be done in a presuppositionless way – especially not at the height of a scientific revolution. Others too have argued that, while it may not be possible to carry out a point-by-point comparison and assessment of competing paradigms, it is nevertheless feasible to make rational judgements about their relative potential. An influential example is Alasdair MacIntyre's discussion of 'three rival versions of moral inquiry' (MacIntyre 1990). For MacIntyre, assessment can proceed by examining how well a paradigm suc-ceeds in its own terms, and then by looking at how well it comprehends the problems that competing paradigms encounter.

The rise in influence of qualitative inquiry, whereby it came to be seen by many as a separate and superior approach, certainly resulted in part from increasing recognition of the failures of various kinds of quantitative work.

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1 It must be said that Kuhn's message was not always clear, and he changed his views somewhat over time, partly in reaction against the reception of his work. For illuminating accounts of Kuhn's argument, see Bird (2000) and Sharrock and Read (2002).

Whereas these failures were generally regarded by quantitative researchers themselves as technical problems – in other words, as manageable, if not resolvable – many advocates of qualitative inquiry came to see them as more fundamental in character, as pointing to basic flaws in the ideas behind quantitative method, these often being dismissed as 'positivist'. In part what was being rejected here was the very image of science that Kuhn's work had undermined. Moreover, in classic Kuhnian terms, there was a sizeable revolt on the part of a new generation, who saw qualitative research as based on fundamentally different assumptions from quantitative work; plus the defection of some more established scholars from that dominant paradigm.[2] In the process, the attitude of most quantitative researchers towards qualitative work, at least in public, came to be one of toleration and even appreciation. It is only quite recently that there have been signs of a change back to more severe assessments.

While I still believe that the criticisms qualitative researchers made of the conventional methodological wisdom of the 1950s were largely correct, and that some of the problems with quantitative work are intractable, here I want to focus on assessing the track record of qualitative work.[3] I will suggest that there is much to criticise in the way that it has developed over the past few decades. In particular, there are respects in which it has not achieved what was promised. I am not denying the major contribution it has made to many substantive fields; though I think that this is rather less than is often claimed. My focus is solely on respects in which it has failed to live up to expectations or to meet the challenges it faced.

The failings of qualitative research can be divided into two categories. The first relates to criticisms that qualitative researchers made, and continue to make, of quantitative work, thereby claiming superiority. I will call these 'offensive failings'. The second sort of defect concerns responses to criticisms that quantitative researchers have made of qualitative research. I will call these 'defensive failings'.

Offensive failings

In the late 1960s and early 1970s, there were a number of grounds on which qualitative research was widely advocated.[4] I will pick out just two main ones:

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2 For discussion of an interesting example of this defection, that of Egon Guba, and the effect on those he taught, see Lather (2007:21–2).

3 I have discussed the problems facing quantitative work elsewhere (Hammersley 2007a).

4 Here, I am taking what Hargreaves (1978) called the symbolic interactionist/phenomenological tradition as providing the theoretical underpinning for the turn towards qualitative method in social research during the late 1960s and early 1970s. This was, of course, not the only strand. There were other influences, some of which were at odds with symbolic interactionism and phenomenology in key respects. For an account of the different varieties of British qualitative research, in education, at that time, see Atkinson et al. (1993).

first, the need to *understand people's perspectives* if their actions are to be explained; and, secondly, recognition of the extent to which social life is a *contingent, and even emergent, process* – rather than involving the repetition of law-like patterns. It was argued that, because of its reliance upon pre-structured data, quantitative research was not able to provide adequate understanding of people's perspectives. Similarly, as a result of its focus on variable analysis, it ignored the processual character of human sociation. Qualitative research was put forward as better able to meet these requirements. Here I want to examine the extent to which it has done so.

Understanding

The first argument was that in order to be able to explain – in fact, even to *describe* – people's behaviour it is necessary to understand how they view the world, and themselves. What the term 'understanding' meant here was for the researcher to learn to be able to see things in the same terms as participants, and thereby to recognise and document the internal rationality or logic of their perspectives. Quantitative research was criticised for either ignoring these perspectives completely, focusing on external behaviour, or eliciting attitudes by structured means, which were believed by qualitative critics to introduce serious distortions. These arose, they argued, from the preoccupation of quantitative researchers with measurement. This involved too many assumptions being built into the structure of the questionnaires or interview schedules employed, assumptions which were likely to reflect the cultural background of the researcher rather than the interpretative assumptions of the people being studied. In other words, much quantitative research was criticised for being framed in terms of conventional wisdom about the field being investigated, whereas (it was suggested) proper understanding requires that prior assumptions be suspended, in order to open them up to challenge.

The danger of misunderstanding had always been obvious in anthropological and other cross-cultural research, since there were many occasions when people's behaviour was not immediately intelligible. However, it came to be argued that the risks of distortion operated even when Western researchers studied their own societies: because of the high level of cultural differentiation within them, and also because of the creative and contextually sensitive ways in which people make sense of the world. It was pointed out that much survey research tends to assume that the dimensions of attitudinal variation are already known, and that the task is to document the number of people who see things in each of the various anticipated ways, and to explain the distribution of attitudes discovered. More fundamentally, there was a tendency to assume the existence of a cognitive consensus: that the world appeared the same way to everyone, or should do so (see Wilson 1971). Qualitative researchers insisted that, by reducing perspectives to positions on an attitude scale, a great deal could be lost that might be significant

if we want to understand people's behaviour. They argued that what is required, instead, is the use of less-structured methods, such as informal and/or open-ended interviews, since these allow people to talk freely in ways that can reveal the distinctiveness and complexity of their perspectives. Only in this way, it was suggested, could genuine variation in orientation be understood.[5]

For example, in the field of educational research, to take an area where qualitative work gained influence very quickly in the UK during the 1970s, this resulted in detailed investigations of students' perspectives, particularly in secondary schools. It was argued that quantitative research either tended to assume that all students went to school simply in order to be educated, in the sense of wanting to learn the official curriculum, or it treated any deviance from this as abnormal, as a form of pathology that had to be causally explained. By contrast, qualitative researchers stressed the importance of exploring students' perspectives and actions without prejudging the parameters of these from the outset, or using evaluative categorisations. The aim was to seek to understand students in their own terms, treating what they said and did as rational in context, and as requiring explication. This was especially important, it was argued, in the case of those who were treated as recalcitrant or deviant by the school system. The task was to discover the rationality of these students' responses to schooling.[6] One general way of formulating this contrast – used at the time, and deriving from the sociology of deviance – was between a correctionalist and an appreciative stance (Matza 1969).

Of course, as it emerged within sociology in the 1960s and 1970s, qualitative research was not simply a reaction against quantitative method. It was also opposed to theoretical approaches that evaluated people's perspectives as rational or irrational against the standard of some scientific body of knowledge or purportedly rational mode of thought. Thus, use of the concept of ideology – whether by structural functionalists or by Marxists – was often rejected as simply explaining *away* whatever was difficult to understand from the researcher's chosen theoretical perspective, or on the basis of her or his background assumptions. Also, qualitative researchers criticised the common dismissal of some forms of behaviour as 'mindless', such as 'vandalism' or 'hooliganism', and challenged explanations of the lifestyles of some groups, notably the poor, as the product of 'cultural deprivation' or a 'culture of poverty' (Cohen 1971; Keddie 1975). Such theoretical approaches were rejected as an obstacle to genuine understanding of the lives of the people being studied.

5 In effect, this represented an attempt to reverse a major shift that had taken place from the 1930s onwards in the study of attitudes. Stouffer (1930) had claimed to show that life history interviews do not provide better data than attitude questionnaires; and this, among other things, led to a decline in the use of life histories and open-ended interviews within US sociology.

6 For examples of work in this genre, see Willis (1977), Marsh et al. (1978), and the articles in Woods (1980) and Hammersley and Woods (1984).

In summary, then, the difficulty of understanding other people's points of view was emphasised by qualitative researchers, this difficulty being believed to arise both from the complexity of people's perspectives, and from the barriers to understanding created by those differences in assumption and orientation that frequently exist between researchers and researched. Central here was a rejection of the idea that societies operate on the basis of a widespread consensus – about either values or facts – in favour of an openness to at least the possibility that there can be heterogeneous (or even 'incommensurable') perspectives within a single society, organisation, or local community. Following from this, what was recommended in methodological terms was an attitude that allowed researchers to *learn* the cultural perspectives of the people being studied, this requiring quite lengthy contact and a relatively unstructured approach to data collection, along with forms of analysis that minimised researchers' prior assumptions and maximised their interpretative capacities.

Now, of course, a great deal of qualitative work has indeed sought to understand, or appreciate, perspectives and actions in this manner. And its contribution in this respect has undoubtedly been substantial in many fields. However, there are some respects, both methodological and theoretical, in which it has frequently failed to live up to this appreciative commitment.

One is that qualitative researchers have often been *selective* in seeking to understand the perspectives of the people they study. It is true that they have often attempted to understand the views of people with whom they sympathised, for political or ethical reasons; and, laudably, these have often been those subordinated, devalued, discriminated against or oppressed by the wider society. However, qualitative researchers have been less ready to seek to understand, and to represent in their own terms, the perspectives of those they regard as playing a more central or dominant social role, and/or those with whom they have little sympathy. In this way, a radical methodological principle of early qualitative research – the commitment to understanding or appreciation – became compromised. In fact, what has resulted here is a process of distortion not unlike that which qualitative researchers claimed to detect in quantitative work; in this case the attitudes of people 'in power', or those judged to support the status quo or to be politically correct, came to be treated as pathological.

This failing is exemplified by the common misinterpretation of Becker's influential article 'Whose side are we on?' (Becker 1967). This has frequently been treated as a call for partisanship, yet it was actually a demand for full commitment to objectivity, in the specific sense of being prepared to question dominant views when these are false (Hammersley 2000:ch 3 and 2004a). Becker argued that researchers must suspend the hierarchy of credibility, in terms of which those at the top of power and status structures are assumed to know more and better than those at the bottom. However, what many qualitative researchers have done is to *invert* this credibility hierarchy. While they have sought to appreciate the perspectives and actions of many people at the

bottom of the heap, they have generally adopted a correctionalist stance towards those judged to be in power or in a privileged position. In relation to *them*, the usual devices of ideological analysis have been deployed.[7]

Sometimes, a more sophisticated version of this politically discriminating approach has been adopted, with elements of the perspectives of the marginalised being treated differently, depending upon whether these are deemed rational or irrational by the – usually implicit – evaluative criteria of the analyst. An exemplar here, still much cited, is Willis's *Learning to Labour*, where he carefully selects out those aspects of the Lads' perspective which offend his political sensibilities and explains them away as ideological (Willis 1977). Subsequently, others have done much the same, for instance in interpreting the perspectives of black students (Mac an Ghaill 1988; see Hammersley 1998b).

What this reflects, I suggest, is a failure fully to grasp the nature of a commitment to appreciation. Setting out to understand the rationality of someone's beliefs and actions does not require accepting the validity or legitimacy of those beliefs and actions. All it requires is for us to find the conditions (for example, particular assumptions about the nature of the world or the situation faced) that made it rational for these people to come to the conclusions they did, or to engage in the kind of action they performed.[8] The analyst does not need to accept that the assumptions made were true, the conclusions correct, and so on. Indeed, whether or not the analyst does so is irrelevant to the process of understanding, except as a potential source of bias.

This error also explains an obverse failing. This is the tendency to protect at least *some* of the views of *some* people from assessment when they are being used as a source of information about the world. This is occasionally legitimated on the grounds that these people are in a privileged cognitive position, for example in terms of standpoint epistemology; or that questioning their views would be disrespectful. An example is the ruling out of any questioning of claims by black children, black parents or black teachers about the extent of racism in the British education system (see, for example, Connolly 1992). It is argued on epistemological or ethical grounds that their claims must be accepted at face value, and that not to do this is itself racist (see, for example, Gillborn 1995:ch 3). Here we can see inversion of the conventional credibility hierarchy in full-blown form. And it amounts to what Becker labelled as 'sentimentalism' in his article.

This leads me to another, closely related, point. In the past, it was frequently claimed that qualitative research would overcome reliance on the taken-for-granted assumptions that much discussion of social issues, and much quantitative research, had previously involved. Thus, the new approach was believed to

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7 For one example see Ball (1990).

8 And here 'rational' does not mean 'logical' in the sense that there could only be one reasonable conclusion.

entail an explicit distancing from conventional assumptions: a making of the familiar strange. And this required a suspension of the researcher's political and ethical assumptions. Yet, in some ways, qualitative research has come to be more embroiled in political and ethical constraints than most quantitative research ever was. Indeed, some qualitative researchers argue that its very *purpose* is political or ethical, in the sense that this extends beyond a concern with the production of knowledge (see Denzin and Lincoln 2005). What this means is that, in many cases, the shift to qualitative research has amounted primarily to a change in political assumptions, rather than to an abandonment of correctionalism in favour of appreciation.

There are several reasons why this has happened. One is the close association between the emergence of qualitative research as a viable challenger to quantitative method, in the late 1960s and 1970s, and the rise of what has come to be called 'critical' research. The labelling theory of deviance was, in part, a political as well as a theoretical challenge to established views in criminology, one that was motivated by a radical liberalism (Pearson 1975; Downes and Rock 1979). On top of this, the emergence of student radicalism, in the late 1960s, shaped the development of sociology and of other social sciences. One aspect of this was a reinvigoration of Marxism after widespread disillusionment with it in the late 1950s. Moreover, Marxism was now seen by many very much in terms of the work of the young Marx, which was regarded as placing greater emphasis on agency: the active role that people can play in structuring social relations. The influence of Western Marxism (Merquior 1986a) was central here, and particularly the Critical Theory of the Frankfurt School. There were important parallels, as well as conflicts, between this and the interactionist and phenomenological ideas that surrounded qualitative research; and in a number of fields, notably deviance and education, there was a fusion of these different trends.[9] This was later reinforced by the influence of feminism and anti-racism, which introduced new political criteria to be used in critically evaluating the social phenomena under study. Later still, similar orientations developed around disability and sexual orientation.

From this 'critical' point of view, older forms of qualitative research came to be judged as insufficiently radical in political terms, on the alleged grounds that they neglected power differences and failed to recognise the researcher's responsibility to challenge dominant groups.[10] Yet, to repeat the point, what is involved here is a relapse from an early methodological radicalism centred on a commitment to appreciation back to a form of correctionalism; a reversion to the old evaluative orientation, albeit with changed political criteria.

It is worth noting that another important influence on qualitative inquiry in recent years, what has come to be put under the heading of 'postmodernism',

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9 But not without dispute. See, for example, Downes and Rock (1979) and Hargreaves (1982).

10 For an early response to these criticisms, see Hargreaves (1978).

challenges the ethnographic commitment to understanding in an even more fundamental way. Several influences that strongly affected younger French intellectuals in the 1950s and 1960s, including Derrida and Foucault, played down or undercut the sort of interpretivism and hermeneutics that had influenced early qualitative research. Behind this post-structuralism lay the Nietzsche- and Durkheim-inspired ideas of Bataille and others, the resurgence of interest in psycho-analysis and Hegelian Marxism, as well as the influence of Russian formalism and structuralism in linguistics and literary study. These perspectives all focus, in one way or another, on the role of the unconscious: on how people's experience and actions are shaped by factors of which they are (perhaps necessarily) unaware. This has undercut the idea that in order to explain social actions and institutions a primary requirement is to understand the perspectives of those involved. Indeed, the very possibility of understanding others, and perhaps even the desirability of attempting to do so, is questioned. Thus, the emphasis on difference, on the Otherness of others, characteristic of much writing that is labelled as postmodernist, stresses the importance of what escapes any understanding. Another challenge derives from rejection of the notion of unitary selves: it is argued that the commitment to understanding others assumes a unitary self that can be comprehended, and also one that is actually doing the understanding.

However, rather than these ideas being subjected to critical evaluation, in the way that they are in the large secondary literature about post-structuralism, qualitative researchers have often used them in *ad hoc* ways. The fact that they conflict, not just with the initial commitments of qualitative inquiry but also with the notion of 'voice' and even with key elements of a 'critical' orientation, has usually been overlooked or neglected. In particular, what has *not* been addressed is the way that such 'postmodernism' challenges the constitutive assumptions of research as an activity. But my main point here is that it undermines what was one of the main bases on which the switch to a qualitative orientation was originally proposed.

Process

A second important commitment of qualitative research, when it emerged as a distinctive approach in the 1960s and 1970s, was an emphasis on process: its advocates argued that, rather than patterns of human activity being pre-determined by some set of psychological or social variables, people ongoingly *build* courses of action over time. They do this on the basis of their goals and concerns, but these are adjusted or even transformed in response to the actions of others. As a result, human action is contextually variable, in terms of both what people say about themselves and their world, and what they do; and it is contingent in character – it can change in orientation, subtly or dramatically, even over relatively short periods of time.

An important methodological implication of this second commitment is recognition that in studying human behaviour there will often be a substantial element of reactivity, with people responding in various ways to the fact that they are being researched (procedural reactivity) and/or to the particular characteristics of the researcher (personal reactivity). The importance of this was highlighted by studies of the role of self-fulfilling and self-defeating expectations in human social life.[11]

On the basis of this commitment to the importance of process, quantitative research was criticised for failing to respect the very nature of human social life. For example, experimental investigations were dismissed as incapable of telling us anything about what goes on in the real world. Attitude surveys were also often rejected, on at least two grounds:

1   They involve the doubtful presupposition that people have stable attitudes which guide behaviour across all contexts as well as over substantial stretches of time; and
2   They assume that variation in attitude can be explained through causal analysis of social and psychological variables, treated as if these all operated independently and at the same point in time.

Against this, qualitative researchers emphasised the need for people's actions – including what people say about themselves and the world – to be *observed in situ*, and indeed across different contexts, as well as over relatively long periods of time. This was the sense in which Becker and Geer argued that participant observation provides the more complete form of data by contrast with interviews (Becker and Geer 1957).[12]

Qualitative researchers also criticised the commitment of most quantitative work to what Blumer referred to as 'variable analysis' (Blumer 1956). This was challenged for two reasons. First, much human behaviour is not sufficiently stable in its course to be explained in terms of a small set of key variables, in the way that the planets' movement in the solar system can (in gross terms at least) be accounted for. Secondly, processual complexity and change mean that, even more than in the exemplary case of evolutionary biology, the very phenomena whose character, causes, and consequences are to be understood do not fall neatly into natural kinds which can be characterised in terms of their essential features. Rather, at best, they form fuzzy sets having family resemblances; in other words, members of these sets do not all share a finite number of clearly demarcated characteristics in common.

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11 See Merton (1948), and also Rosenthal and Jacobson (1968). The core idea is to be found in the dictum of W.I. Thomas to the effect that what people treat as real is real in its consequences (see Merton 1995).

12 For a recent critique of this classic article, see Atkinson et al. (2002).

As with the first commitment I discussed, qualitative researchers have not lived up fully to this second one. Silverman and others have pointed out that a large proportion of qualitative research today relies entirely on interview data, and treats this as a window on people's stable perspectives and/or on their behaviour in other contexts (Silverman 1973 and 1997, 2007; Murphy et al. 1998; Seale 1998). While I would not go as far as some of these critics in challenging the value of interviews, it is not difficult to see that there are grave dangers in exclusive reliance on this source of data (see Chapter 5). Indeed, the older criticisms of interviews apply as much to qualitative researchers' use of them as to their employment in survey research:

- that interviews elicit responses in a distinctive context whose character is shaped through reactivity; and
- that, therefore, they do not provide a reliable basis – on their own – for inferring what people say and do in other contexts.

In other words, reliance on qualitative interviews as an exclusive, or even a primary, means of understanding people's behaviour suffers from some of the same problems as reliance on attitude inventories in survey research. In many cases, it assumes that behaviour is in large part a function of some stable orientation that directly expresses itself in the same way in diverse contexts. Here there is a neglect of contextual variation – of the way in which people respond to variation in socio-cultural context – and of the scope for change in people's orientations over time. Furthermore, there is often a failure to recognise the implications of the contextual sensitivity of what is said and done for what can be inferred from what people say in interviews about their experience of, and actions in, the world.

There are a number of possible reasons for this over-reliance on interviews. One arises from the first commitment I discussed, or rather the kinds of distortion that have been introduced into it. The emphasis that some qualitative researchers have come to place on capturing the 'voices', by implication the 'true' or 'authentic' voices, of the marginalised or oppressed has perhaps tended to privilege the use of interviews. So, too, probably, has the parallel concern with ideology critique of the views of those in dominant positions. Also significant is what has been referred to as the 'discursive turn' in social thought: the increased interest in detailed analysis of what people say or write. In particular, audio-recorded and transcribed data have become the main kind of data employed in much qualitative work. While observation can generate discourse data, interviews are a more accessible source. It is often easier to gain agreement to carry out a few interviews than to negotiate access for a lengthy period of participant observation; and the scope for audio-recording is likely to be greater and the quality of the recording better. Increases in the time pressures under which researchers work may have exacerbated this trend towards

reliance on interview data, since relatively large amounts can be generated quite quickly.

A similar neglect of the significance of process operates at the level of qualitative *analysis*. In practice, much of this is concerned with the ways in which variables, usually treated in a largely static way, shape perspectives and actions; or how particular types of orientation result in particular types of outcome. Moreover, many of the variables employed are those that are central to conventional social theory and/or everyday policy language – for example, social class, gender, and ethnicity; and outcomes such as criminal conviction or educational qualification – rather than variables whose significance has emerged from the particular study being carried out and/or which are distinctive to the local context being investigated. As a result, quite a lot of qualitative inquiry now takes a form that is not far from being old-style quantitative research without the numbers. A range of different attitudes or orientations may be identified, their distribution specified, albeit usually in qualitative rather than quantitative terms; and this is then explained as resulting from the operation of structural variables and/or as producing a certain pattern of outcome (for examples, see Gewirtz et al. 1995; Reay 1995; Reay et al. 2001).

One reason for this move back towards variable analysis is the influence of the 'critical' approaches mentioned earlier. These have generated pressure to 'locate' whatever is being studied in its 'macro context', and most recently in a global context (see, for example, Burawoy 2000). And it is this wider context, often introduced in a heavily pre-theorised fashion, which gives the researcher the structural variables that are to be the main focus. Here the arguments of interactionists and ethnomethodologists about the nature of context have been largely ignored, forgotten, or never properly understood (see Rock 1973; Watson and Seiler 1992). They have certainly not been refuted. More importantly, though, there has been a failure to recognise the importance of generating theory rigorously through the analysis of data, in the manner recommended, for example, by grounded theorising and analytic induction. Instead, both these terms are often used in ways that ignore the considerable demands they make on qualitative analysis. Instead, what seems to take place is a much weaker form of pattern-finding.[13]

Summary

I have argued that in two significant respects – relating to the initial guiding themes of 'understanding' and of 'process' – qualitative research has failed to live up to its own initial commitments. Now, to some degree this may have arisen

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13 I am not suggesting that either grounded theorising or analytic induction are without problems. See Dey (1999) on the first, and Chapter 4 for the second. However, they both represent systematic approaches to the production of theory from qualitative data.

from judgements to the effect that those commitments were ill-conceived or illegitimate. But, in large part, what seems to have occurred is a process of *ad hoc* and largely unconscious drift: back towards orientations that ignore the radical break represented by the principle of appreciation and a concern with taking process seriously.

At the same time, there has been little recognition of the extent to which this leads to a re-emergence of the standard methodological problems that have long preoccupied quantitative researchers. In the second half of the chapter, I want to discuss qualitative researchers' attitudes towards those traditional problems. Here, too, it seems to me that there has been a significant failure.

Defensive failings

In my view, qualitative researchers have failed to defend their work effectively against quantitative criticisms; or, to put it less combatively, they have not addressed effectively the problems to which those criticisms point. In the early battles with quantitative researchers, qualitative inquiry was criticised in three main areas:

1   For failing rigorously to operationalise concepts and thereby to document measurable differences.
2   For being unable to rule out rival explanations through physical or statistical control.
3   For failing to produce generalisable findings.

Qualitative researchers' responses to these criticisms have varied, but the following are the main ones:

1   It is sometimes argued that qualitative work does not need to stand on its own, and if it is combined with quantitative work it can meet the requirements that lie behind these criticisms.
2   Alternatively, it may be argued that qualitative research has its own ways of documenting differences, identifying causal relations, and producing theoretical generalisations. Therefore, the strategies associated with quantitative work are not relevant to it, and the criticisms do not apply.
3   Finally, it is often argued that these criticisms of qualitative inquiry simply misconceive the nature of social research, since they derive from a positivistic paradigm that has been discredited and superseded.

Over the years, the latter two responses, and especially the third, have probably become the most common; though a revival in the fortunes of the first may be

taking place, in the context of calls for mixed methods research. My own view is that there is an element of truth in all of these responses, and that none is adequate. Moreover, these three counter-arguments have tended to be used as shields to fend off criticisms rather than as starting points for the development of a sounder position.

Those putting forward the first defence have often appealed to the concept of triangulation as a way of dealing with the issues of measurement, causal validation, and perhaps even generalisability. Yet, this term is used in a variety of ways, often without much clarity about its meaning. Initially, it was employed to refer to the use of additional types of data in order to check the *validity* of inferences. The idea here is that by drawing data from sources that have divergent threats to validity it is possible to discount error built into the data (see, for example, Denzin 1970). And combining quantitative and qualitative data has frequently been regarded as the paradigm case of such triangulation.

In practice, however, even qualitative researchers who use the term 'triangulation' in this sense often do not apply the strategy in a rigorous fashion: they do not seek to identify the relevant validity threats and assess their impact. Instead, the term has come to refer more loosely to drawing on different sorts of data within the same study, irrespective of how there are used. Moreover, there has been a shift in the qualitative methodological literature away from this original interpretation of the term. Flick reports this move as follows: 'Triangulation was first conceptualized as a strategy for validating results obtained with the individual methods. The focus, however, has shifted increasingly towards further enriching and completing knowledge and towards transgressing the (always limited) epistemological potentials of the individual method'. From this latter point of view, as he remarks, 'triangulation is less a strategy for validating results and procedures than an alternative to validation [...]' (Flick 1998:230). This is probably an accurate account of how views have changed. However, the grounds for this move have not been presented very clearly, nor do they seem convincing (Hammersley 2008a).

So, in this respect at least, the first of the three responses to quantitative criticisms has not been developed effectively. And neither have the other two. Qualitative researchers' insistence that they operate on the basis of a quite different paradigm, one which either deals with the problems in a distinctive way or avoids those problems altogether, is seriously weakened, as a defence, by the fact that there has been a proliferation of alternative qualitative paradigms, none of which has gained a broad consensus. In the early stages of growth in qualitative inquiry, interpretivism was appealed to as a counter to the positivism that was held to underpin quantitative method. Even this took a number of different forms, captured at the philosophical level by the divergent positions of Schutz and Winch (Winch 1958; Schutz 1962; Thomason 1982). For the former, philosophical understanding was to provide a foundation on which social scientific work of relatively conventional kinds could be carried out. For the

latter, philosophical understanding was a substitute for social science.[14] Later, there was the already-mentioned influence of 'Critical' Theory, which in its early forms insisted on the importance of a philosophical-cum-scientific grasp of the whole process of socio-historical development if the perspectives and actions of any particular group of people were to be properly understood. This then differentiated into forms concerned with other kinds of social inequality besides social class, centring on gender, ethnicity, sexual orientation, and disability. More recently still, all these approaches have been subjected to criticism and modification by post-structuralism and postmodernism, in various ways. The result is that, as noted earlier, the stances taken by qualitative researchers today often involve an unstable blend of different positions, with arguments that have potentially contradictory implications being used in an *ad hoc* fashion to respond to criticism, or to justify particular research strategies.[15]

In general terms, the predominant trend has been to move further and further into what we might call the constructionist cul-de-sac. There are various way-stations on this road. The starting point is a quite reasonable insistence that all accounts of the world are constructions – in the sense that they are produced by particular people in particular places with characteristic interests, attitudes and background assumptions – and that they are always constructed in terms of some set of cultural conventions, since otherwise they would be unintelligible. It may also be recognised that there are different genres in terms of which accounts can be formulated, and that multiple true accounts can be given of the same scene.

However, many are tempted to go beyond this first position to assume, wrongly, that researchers must recognise *contradictory* accounts produced by informants as all true, or at least as no more false than one another. This is often proposed on the grounds that these accounts represent the voices of diverse groups of people, or that the validity of no account can be 'legislated' by the researcher – that the task instead is, for example, to analyse the discursive practices which make them persuasive (see Potter 1996a). It should be noted, though, that this position is rarely applied across the board: there are some views, for example those regarded as sexist or racist, that are almost always subjected to implicit or even explicit critique.

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14 There is some dispute about the relationship Schutz envisaged between his phenomenological investigation of the lifeworld and the conventional practice of social science (see Thomason 1982). Furthermore, Lyas has questioned the common interpretation of Winch's position I have presented here (see Lyas 1999). Nevertheless, my point is that these writers were treated as exemplifying contrasting orientations within the interpretive tradition.

15 In their introductions to successive editions of the *Handbook of Qualitative Research* (1994, 2000, and 2005), Denzin and Lincoln have sought to legitimise this by suggesting that the qualitative researcher should be a '*bricoleur*'. However, here again there has been no proper development of an effective supporting argument. See Chapter 7.

Moreover, a few qualitative researchers go even further into the cul-de-sac, accepting that even their own accounts and those of other social scientists must themselves be treated as simply further 'versions of reality', constructed in particular ways and with no more claim to truth than any other, since all accounts actively constitute the reality they purport to represent. The implication of this radical epistemological constructionism is not simply a rejection of the methodological principles underlying quantitative work, or even of positivism, but an erosion of the fundamental commitments that are preconditions of *all* inquiry. These are replaced by a general scepticism about the very possibility of knowledge, or at least a denial that research knowledge can make any claim to knowledge which is superior to that from other sources. This undermines the viability of inquiry as a specialised activity that warrants effort or funding.[16] Thus, what we have here is not a defence of qualitative research but rather an abandonment of it in favour of some other activity: politics, literature, or art.

Under the influence of constructionism, the purpose of research is sometimes presented as simply raising questions or challenging taken-for-granted assumptions, and thereby unsettling existing positions (see, for example, Lather 1996; MacLure 2003). Yet, it is unclear why research would be necessary in order to achieve this; after all, it seems to come naturally to young children. At the same time, how could blanket questioning of assumptions be justified? Here, genuine recognition of the fallible nature of our knowledge has collapsed into a spurious scepticism. It is spurious, not least, because it cannot be maintained consistently: as has long been recognised, in order to doubt anything we always have to take something else for granted.

My basic point, then, is that no effective response to quantitative criticisms has been developed by qualitative researchers. Triangulation has been appealed to as a basis for combining quantitative and qualitative methods, but without this being properly theorised; and without much evidence in practical terms of the effective combination of different types of data to meet the demands for measurement and causal validation. More usually, qualitative researchers have discounted quantitative criticisms as based on an inappropriate or false paradigm, but they have not developed an effective consensus about the philosophical underpinnings of their work. Indeed, many have drifted into a denial that consensus is necessary or desirable, insisting on the celebration of 'difference'. But there is no such thing as difference in general, and the identification of any particular difference depends upon some assumption of underlying similarity. The situation we are faced with is methodological fragmentation in many fields of research, and this probably should not be tolerated, even less celebrated (Hammersley 2005b). Finally, some of the positions now adopted amount to the abandonment of inquiry, under the influence of constructionism.

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16 At the same time, puzzlingly, this scepticism is often combined with an apparent belief that certain political, ethical or aesthetic values can be treated as absolute.

In my view, the quantitative criticisms of qualitative inquiry have some force and need to be properly addressed:

- *Measurement:* While often denying the need for quantification, qualitative researchers frequently make quantitative judgements about intensity of commitment or belief, about the distribution of perspectives across categories of actor, about the frequency of particular sorts of action or event, and so on. I am not suggesting that these must always be dealt with via counting, even less by formal measurement procedures, but this may sometimes be necessary (see Hammersley 1986). In particular, we need to address the well-known threats to the validity of frequency judgements – both our own as researchers and those of our informants (Phillips 1990:33). There are, of course, arguments to be developed here about what is meant by 'measurement', when it is and is not necessary to measure, what can and cannot be measured, and so on. But these have scarcely begun to be addressed by qualitative researchers. Instead, they have been largely ignored.
- *Causal analysis:* While qualitative researchers sometimes deny that they are engaged in causal analysis, most of their research reports are saturated with causal claims about how X affects, influences or shapes Y, about the consequences of various institutional practices, and so on. Moreover, this cannot be avoided without radically re-specifying the whole nature of the enterprise. There are, of course, examples of such re-specification. One is ethnomethodology, but only a relatively few qualitative researchers adopt this orientation consistently, and there are serious questions to be raised about it (see Chapter 6). Another sort of re-specification is the kind of constructionism, mentioned earlier, that attempts to be fully reflexive, continually undermining its own claims to knowledge. Yet few qualitative researchers adopt this radical re-specification either. Not unreasonably, they want to continue in the business of inquiry. But the price for doing this is finding some effective means of documenting what causes what (see Gomm et al. 2000).
- *Generalisation:* Some qualitative researchers insist that they focus on the uniqueness of the cases they investigate. However, most cases studied by social scientists are of little general interest in themselves, and qualitative researchers, even when they deny that they are generalising, often go on to do just this. Of course, there are important arguments that have been developed about the *kinds* of generalisation that are possible on the basis of qualitative work, though some of these involve major problems. For example, there are writers who insist that they are not engaged in empirical generalisation but rather rely on theoretical generalisation. However, there is rarely much clarity about what is meant by 'theory' here, about how such inference is possible and can be checked, and so on (see Hammersley et al. 2000). Moreover, very little effort has been devoted to the systematic

development and testing of theory by qualitative researchers.[17] Similar issues arise with generalisation from a sample to a finite population. Qualitative researchers often seek to do this, inevitably. It does not require the application of statistical sampling procedures; nor are these unproblematic. However, the task does need to be addressed explicitly, and the strategies used for doing it justified.[18]

So, the various defences that qualitative researchers use against the criticisms made by quantitative researchers have not been properly developed. And the problems these criticisms point to are genuine ones. Instead of addressing the criticisms effectively, stock defences have been treated as if they were magic symbols that can ward off positivist demons.

Conclusion

In this chapter, I have identified two respects in which much recent qualitative research has failed to live up to the promises that were initially made on its behalf. First, there has been failure to satisfy the claims made for its superiority. Qualitative inquiry was supposed to provide genuine understanding of people's perspectives, rather than being biased by methodological preconceptions and conventional wisdom. However, this appreciative approach has been applied only selectively, and has sometimes been distorted into the idea of 'giving voice' or advocacy. Moreover, some influences on qualitative inquiry, notably 'critical' theory, postmodernism and constructionism, challenge the very possibility of, and/or desirability of seeking, such understanding.

Equally important, qualitative inquiry promised to take proper account of the processual character of human social life. Yet much qualitative research today relies primarily on interviews, and implicitly resorts to variable analysis, identifying what it claims are relatively stable orientations on the part of the people studied, explaining these in terms of structural variables, and treating them as having determinate effects. Once again, there seems to have been a collapse back into the sort of approach used by the quantitative work that was heavily criticised in the initial promotion of qualitative inquiry. Moreover, this has not been done in an explicit and reasoned fashion, but rather there seems to be have been an unwitting slide back into old ways under the cover of adopting 'new' approaches.

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17 For discussion of an exception, see Hammersley (1985).

18 For the most illuminating discussion of the problem of generalisation by a qualitative researcher, see Schofield (1989).

Secondly, I discussed what I referred to as defensive failings. I argued that qualitative researchers' various responses to the criticisms that quantitative researchers have made of their work are largely ineffective. Instead of engaging with these criticisms, stock arguments are often used as a way of trying to deflect them. Yet the problems are genuine ones that qualitative researchers cannot avoid. Furthermore, some of the positions taken involve a rejection not just of positivism and quantitative method but of the constitutive assumptions of any research: that there are phenomena that exist independently of our investigations of them, and that pursuing knowledge of these is a viable and worthwhile goal.

I am not arguing here that qualitative researchers must revert either to their original commitments or to positivism.[19] Nor am I suggesting that qualitative research is unsustainable and therefore must be incorporated into a quantitative approach; though I do believe that an integrated approach to social research would be desirable – one that takes proper account of the methodological arguments on both sides of the divide. For the moment, however, it seems to me that qualitative researchers need to become more reflective and open-minded, to recognise the contradictory methodological arguments that now inform their work, and to engage with the serious problems that remain unresolved.

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19 I would not deny that there are aspects of positivism from which much could still be learned (Hammersley 1995:ch 1). But there is also much to be learned from the other perspectives that have shaped qualitative inquiry over the past fifty years, including postmodernism.