METHODOLOGICAL DISAGREEMENT AND THE PROBLEM OF QUALITY

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[Let me begin with an apology. In the two papers provided for the seminar, I have ignored case study and have focused entirely on qualitative research. The reason for this is that the term ‘case study’ covers such a range of different kinds of work that it would be difficult to provide any account of how it should be assessed as a method, even less of how the findings produced ought to be evaluated. With some colleagues I have elaborated on these problems elsewhere (Gomm et al 2000: Introduction). In my view ‘case study’ – or, better, ‘individual case study’ - is best defined specifically as a strategy for the selection of cases for investigation, rather than as also covering sources of data and forms of analysis (see Hammersley 1992: ch11).]

The issue of the criteria by which qualitative research should be judged, if indeed it should be judged by any, has been one about which qualitative researchers have been unable to agree. There have been many attempts to identify such criteria (lucidly reviewed and consolidated by Spencer et al 2003; see also Altheide and Johnson 1994; and also the recent discussion by Furlong and Oancea 2005 of criteria for assessing applied and practice-based research). Some writers have tried to apply what they see as traditional quantitative criteria to qualitative research. Others have sought to reformulate these epistemic criteria and/or to add extra non-epistemic criteria, whether in terms of ‘giving voice’ to the marginalised or bringing about practical or political effects of some kind. In addition, there are one or two writers who appear to reject the very possibility of criteria, at least as conventionally understood. Smith and Deemer (2000), for instance, seem to offer a complex mixture of the last two options, arguing that ‘the issue of criteria for judging inquiry is a practical and moral affair, not an epistemological one’. Moreover, they insist that ‘criteria should not be thought of in abstraction, but as a list of features that we think, or more or less agree at any given time, and place, characterize good versus bad inquiry. This is a list that can be challenged, added to, subtracted from, modified, and so on, as it is applied in actual practice – in its actual application to actual inquiries’ (p894). There seem to be two points being made here. First, criteria of evaluation cannot be about validity, since to assume this is to adopt a foundationalist or realist position, which these authors reject. Furthermore, at most all there can be is a list of considerations that it is agreed in local circumstances should be taken into account in judging qualitative work, a list that can serve as no more than a reminder and that is always open to revision in the process of being used – indeed, which only gains any meaning in that context.]

Now, there are many people who are impatient with these debates, stressing what they see as the obvious requirement for qualitative researchers to make clear the criteria by which their work should be judged. Indeed, the fact that there is such disagreement is sometimes taken to indicate that there is something fundamentally wrong with qualitative research. While I have some sympathy with this reaction, in my view it is mistaken. I believe that there can be no resolution to the problem of how we assess quality in qualitative research without engaging with the diverse approaches to be found in that field, since it is these that have generated the debates and differences in evaluative practice. Aside from anything else, there is little point in seeking to impose a set of criteria on a research community that will simply reject them. But, even more importantly, there may also be something to learn from the attitudes of other qualitative researchers about this issue even when we do not agree with them.

I suggest that the disputes over the problem of criteria reflect some fundamental divisions to be found within social science research communities, ones that differentiate quantitative from qualitative approaches, in broad terms, as well as dividing some sets of qualitative researchers from others. These divisions are often formulated by means of the concept of paradigm, in the meaning (or, rather, one of the range of meanings) given to that term by the historian and philosopher of science Thomas Kuhn. There has been much discussion of what Kuhn meant by ‘paradigm’, and it is clear that he used the term in various ways. However, his core argument was

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1 For discussion of these various positions on qualitative criteria, see Hammersley 1992: ch4.
2 For excellent recent discussions of Kuhn’s work, see Hoyningen-Huene 1993, Bird 2000, and Sharrock and Read 2002.
If, in order to identify phenomena that are important to study, we necessarily draw on some set of values, extracted from a range of other values that could have been employed; and if, as seems likely, what are involved here are not simply values on their own but values bound up with various factual assumptions about the nature of the phenomena of interest and why they are important, then we have lines of division within educational research that are much more fundamental than those which Kuhn identified in natural science.

Now, the reason why I employed the work of Weber is because he did not believe that social science research is necessarily, even less that it should be, political; that natural scientists working within a mature field operate with a set of assumptions, in the sense of being geared towards political goals. There are quite a lot of qualitative researchers who insist that research ought to be political in this sense: that it should aim at the eradication of social inequality of various kinds, that it should improve the lives of some group of people (children, for example), that it should arise that can take over the field, because it is able to explain all of what the previous paradigm covered and resolves the anomalies.

Now there have been many disputes about the accuracy of Kuhn’s account of natural science, but the task of applying it to social science is even more complex and uncertain. Kuhn regarded the social sciences as pre-paradigmatic, at best, and therefore as not characterised by competing paradigms, in his sense of the word. And, even were we to ignore this, there is very little social research that approximates to his notion of normal science; instead, much of it over the past three decades seems to have been in a state of continual revolution, albeit one that is not recognisably scientific in Kuhnian terms. At the same time, it probably is the case that social scientists, including educational researchers, do operate on the basis of exemplar studies and models, and that this is the reason why they often adopt rather different approaches to the same topic, and why they sometimes disagree so sharply. This idea of the development of a variety of kinds of work occupying different niches in the field may go some way in helping us to understand the diversity in approach within social science. And what I want to suggest is that what is involved here is much more complex than in the case of natural science.

I can develop this point by drawing on one of the arguments of Max Weber. He inherited the view, from neo-Kantianism, that social science, by contrast with natural science, must be idiosyncratic rather than nomothetic in orientation. What this means is that social scientists are primarily interested in understanding particular social phenomena in their socio-historical contexts, rather than in discovering universal scientific laws. Moreover, by contrast with the neo-Kantians, Weber believed that in identifying and conceptualising particular phenomena for study, social scientists could not draw on objective or eternal values to define what is worth investigation. Rather, value-relevance is perspectival: there are different value perspectives, which highlight different aspects of the social world as more and less important, and among which there is no rational basis for choice.

This is a rather loose interpretation of Weber, or a kind of extrapolation of his views, but it may help us to understand the situation in educational research today. If, in order to identify phenomena that are important to study, we necessarily draw on some set of values, extracted from a range of other values that could have been employed; and if, as seems likely, what are involved here are not simply values on their own but values bound up with various factual assumptions about the nature of the phenomena of interest and why they are important, then we have lines of division within educational research that are much more fundamental than those which Kuhn identified in natural science.

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Weber thought that value relevance is not a matter of partisan definition, that those who fundamentally disagree about an issue could nevertheless agree about what is relevant to that issue, so that research could play a role in facilitating resolution of disagreements through both providing factual evidence and clarifying the implications of different value positions; even though there was no guarantee that it could completely resolve the disagreement, since he believed that there are ineradicable conflicts among fundamental values. However, while it is true that conflicting views necessarily share at least some assumptions about what is relevant, it is also the case that these value and factual assumptions may not be held by other people. As a result that there may be kinds of research that are believed to be important by some but
regarded as pointless or as positively harmful by others. Let me try to illustrate the point with an example:

There is a body of research on gender inequalities in children’s participation in classrooms. In order to accept the value relevance of this, one must, first of all, believe that there should be equality between the sexes in some sense of that term (and this might not be accepted by the radical religious right or by some radical feminists, both of whom believe that there are fundamental differences in orientation between the sexes, though of course they conceive of these in discrepant ways). One also has to believe that particular kinds of classroom inequality are consequential for educational achievement; and, in addition, one must value educational achievement, defined for example in terms of examination success. Yet, there are those who question whether gender or other inequalities in the classroom have determinate and consequential effects, and others who deny that examinations measure education and/or insist that examinations are at odds with true education. What I’m trying to show, then, is that research in this field is framed by a set of both value and factual assumptions that are not necessarily a matter of consensus. What is involved here is not an instance of absolute ‘incommensurability’ (a problematic term even in the context of Kuhn’s work), but rather that the fewer of the assumptions built into a field of research people share the more difficult it will be for them to understand the point of the research and the less inclined they will be to see it as of value. Moreover, while there is scope for persuasion, so that one might come to see the point of a particular form of research one did not previously value, this is likely to require arguments about values not just about facts. And, as a result, there is little guarantee that even lengthy rational discussion will produce a consensus.³

So I am arguing that some of the variation to be found within educational research derives from the fact that it operates in diverse value-relevance niches. And I believe that these niches have consequences not just for judgments about what sorts of work are and are not worthwhile in terms of relevance, but even for judgments of validity. This is because the threshold of validity is determined by relevant research communities - it is not something that can be laid down by the philosophy of science or by any other central authority.

³ For a more detailed discussion of examples of work in this area, see Hammersley 1990. I have specifically avoided the examples that have become almost standard ones: the case of research on racial differences in intelligence and that of research on ‘effective schooling’. For a very recent discussion of the latter that illustrates my point, see Clark 2005.

Besides this variation in orientation arising from operating in different value-relevance niches, there are also some more abstract sources of paradigm differentiation within educational research. These two things are not, of course, completely independent: those working within particular niches draw on, modify, develop, and perhaps even misuse, more general ideas for their own purposes, and then often portray educational research in general as if it were simply their own work writ large, thereby contributing to the intellectual resources that others may draw on in facilitating and justifying work in other niches.

Anyway, what I want to do in the rest of this paper is to identify three general areas where one can find sharp disagreements within the literature of qualitative research methodology – in social research generally not just in education – these having important implications for judgments about quality. They relate: first, to different conceptions of scientific method and rigour; secondly, to the conflict between constructionism and realism; and, finally, to the relationship between research and various other kinds of practical activity, including politics.

Scientific method and rigour

The natural sciences were the model of inquiry for most social researchers for much of the twentieth century. However, this did not prevent considerable disagreement about how social research ought to be pursued. There are three, interrelated, reasons for this:

First, different natural sciences were sometimes taken as exemplifying scientific method: for example, some researchers treated physics as the premier science, others treated nineteenth-century biology (in particular, botany) as a more appropriate guide. Furthermore, there were also disciplines broadly within the social sciences that were taken by those working in other fields as exemplifying scientific method. Key examples here are psychology, economics, and structuralist linguistics. The point is, of course, that what is taken as the key exemplar can lead to very different prescriptions.

Secondly, there were different philosophical interpretations of scientific method. Broadly speaking, moving from the nineteenth into the twentieth century there was a shift from an inductivist conception of inquiry, in which scientific laws were logically derived from observation of repeated patterns of occurrence, towards one which stressed the testing of hypotheses deduced from theories that were necessarily a product of speculative thought and that perhaps could never be proven to be true, only falsified. Quite a lot of the conflict between social scientists
promoting quantitative and those emphasising qualitative methods in the first seventy years of the twentieth century stemmed from commitment to different views of scientific method along these lines, with qualitative researchers tending to retain a more inductivist approach.

Finally, there was variation in views about the differences between social and physical phenomena, and what the implications of any differences were for how they should be investigated. At one extreme, some believed that there were no distinctive features of social phenomena, or that if there were these did not stand in the way of rigorous measurement and control of variables, taking physics as exemplifying scientific method. By contrast, others insisted that social phenomena had to be approached quite differently from physical phenomena. Indeed, it was sometimes argued that whereas physical phenomena could only be studied from outside, social phenomena could and should be understood from within, so that the kind of knowledge available was deeper. And it was claimed that this inner understanding required the researcher to draw upon his or her psychological and/or cultural resources to grasp the meanings that informed the actions of the people being studied, since these meanings are crucial for what it is that people set out to do and why. In particular, it was insisted that these meanings cannot be inferred from external behaviour. To use Clifford Geertz’s example: there are significant differences between a facial tic that forces the closure of one eye, a wink, someone pretending to wink, and someone practising winking; and it is not possible to infer from physical behaviour alone which of these is taking place (Geertz 1973:6). Given this, some other, or additional, means of access must be secured to the cultural meanings that inform people’s behaviour if we are to be able even to describe it accurately, let alone to explain it.

Now qualitative researchers today vary in whether they see their work as scientific and, if they do, in what they take this to imply. And, aside from this, there are significant differences in what is seen as possible or legitimate in epistemic terms. To put a little flesh on the bones here, I want to discuss a recent, and ongoing, dispute that illustrates some of the differences: what has been called the ‘radical critique of interviews’ (Murphy et al 1998:120-3).

This critique does not just raise questions about over-reliance on interview data; a common complaint, for example, on the part of ethnographers who, in the past at least, tended to stress the centrality of participant observation. What makes it radical is that it challenges the use of these data as a window into the minds of informants and/or as a window on to the social worlds in which informants live (Dingwall 1997; Silverman 1997; Atkinson and Coffey 2002). In other words, the critics throw doubt on the idea that interviews can tap stable attitudes or perspectives that govern people’s behaviour beyond the interview situation, or that they can be a sound source of witness information about what happened or happens in particular settings, or in the world more generally.

Concern over the status and use of interview data in qualitative research is by no means new. However, the grounds for this concern, and the conclusions drawn on the basis of it, have changed. In the past, the focus of criticism was on issues like:

- ‘how do we know the informant is telling the truth’ (Dean and Whyte 1958);
- the ‘incompleteness’ of interview data as compared with what can be gained from participant observation (Becker and Geer 1957);
- the difference between what people say and what they do (Deutscher 1973).

To a large extent, these issues were practical, methodological ones concerned with how best to conduct interviews, or how to combine them with observation, in order to gain the information required. By contrast, the radical critique involves a more fundamental scepticism about the capacity of interviews to provide the basis for accurate representations of anything beyond the interview situation itself: either of the interviewee’s general orientation, personal experience, etc or of events in the world in which he or she lives. On this basis, some critics insist that researchers should avoid the use of interviews altogether and restrict their analyses to ‘naturally occurring’ data. Others argue that we should only analyse interview data for what they can tell us about interviews as sites for discursive meaning-making.

The rationale for these restrictions draws on one or other, or both, of two main theoretical approaches – ethnomethodology and social constructionism - and on forms of research stemming from these, specifically conversation and discourse analysis. Ethnomethodology analyses the social world as ongoingly constituted in- and-through participants’ displaying what they are doing as intelligible - as orderly - in multifarious ways. It thus involves a re-specification of the traditional sociological concern with the problem of order, patterns of institutional life, and so on; indeed, a fundamental re-specification which transforms what was previously treated as a resource – commonsense methods of making sense of the world – into a topic for analytical inquiry (see Zimmerman and Pollner 1971; Heritage 1984; Lynch 1993). The focus becomes the accounting practices through which people, from one moment
to the next, constitute the social world as an orderly place that has stable and recognisable features.

Constructionism also has radical implications for qualitative research, if applied consistently. As with ethnomethodology, the focus of inquiry can no longer be stable attitudes or patterns of behaviour, institutional structures, and so on - at least not if these are conceived as phenomena existing in the world independently of the discursive activities of the actors involved. Instead, the focus must be on the discourses through which people construct the world – presenting it as taking one form rather than another (see Potter and Wetherell 1987). Indeed, often, the very fact that phenomena are portrayed as having an independent existence is treated as itself a product of the rhetorical strategies employed (see, for example, Potter 1996).

Drawing on ethnomethodology, conversation analysis has generally ruled out any use of interview data, insisting on the importance of employing data from ‘naturally occurring talk’, in other words talk unaffected by the researcher. By contrast, under the influence of constructionism, discourse analysts have used interviews, but simply as a convenient way of generating data for analysis. Strictly speaking, in neither case is what people say to be used as grounds for inferring what they think or feel, or what they and others do in other contexts. In this way, both approaches rule out what were previously the main uses of interview data among social researchers, including qualitative researchers.

Now, the point I want to emphasise here is that what motivates these radical positions are conceptions of methodological rigour, albeit ones that are rather different from those which are characteristic of quantitative research, and of some other kinds of qualitative work. They rely on one or more of the following arguments:

1) A rejection of the idea that what people say somehow represents, or simply derives from, what is going on inside their heads. This is part of a general philosophical insistence on viewing mind as behaviour, and therefore as publicly available - rather than as a matter of internal, private experience beyond public representation. From this point of view, accounts of ‘subjective’ phenomena - of beliefs, attitudes, past experience, etc – are treated not as more or less accurate representations of cognitive activity or of internal events but instead as public displays through which subjectivities are actively constituted and displayed. As a useful shorthand phrase, we can call this first argument discursive psychology (see Edwards 1997).

2) A second line of argument is scepticism about the idea that accounts can ever represent reality at all, whether this is ‘external’ or ‘internal’ reality. Here, even accounts of what happened in some public setting are treated not as true or false, but rather as constitutive - as themselves producing one of many possible versions of events. Thus, reality is constructed in and through the telling, rather than having characteristics that are independent of this. As with the first argument, such epistemological scepticism often leads to an abandonment of any concern with what information interviews might supply, in favour of a focus on the work that accounts produced in interviews do, and perhaps also on the cultural resources which they employ.

3) A third argument is what we might call severe methodological caution. This relies on a contrast drawn between the results of scientific observation and the ordinary accounts provided by informants in interviews: by contrast with researchers’ own observations, informants’ accounts are not usually based on rigorous data collection - for example they do not involve audio- or video-recording plus full transcription, or even careful note-taking. Nor are they the result of systematic analysis for exclusively scientific purposes. Given this, it is suggested that interview data cannot be used as a substitute for observations by the researcher.

4) Finally, there is the argument that a person’s responses in interviews are so heavily shaped by the context, and especially by the influence of the interviewer, that reliable inferences about their attitudes or behaviour in other situations are impossible. Here the focus is on reactivity, treated as an irremediable matter: interview data are so contaminated by the features of the interview situation as to make them useless for analytic purposes, or at least for studying anything other than behaviour in interviews.

Now, in large part, what the radical critique indicates is the growing influence of discourse and narrative analysis, and the ideas surrounding them, as against older qualitative approaches, such as ethnography and life history. Indeed,

4 What is involved here is behaviourist in one way, but differs from old style behaviourist psychology in not being concerned with causal explanations of behaviour but rather in generating ‘phenomenological’ or ‘structuralist’ accounts of how social phenomena come to be what they are recognised as being.
both the latter are being transformed by some of their exponents into something much closer in orientation to discourse analysis. But it is also important to emphasise that there are some very significant divisions within discourse analysis. Indeed, those who do not already realise it may be horrified to discover that there are, to exaggerate only slightly, 57 varieties, displaying quite fundamental differences from one another; though I will not go into the details of these here.5

It is worth pointing out that what the radical critique is rejecting is not just an approach to the use of interview data that is shared by many qualitative researchers with survey researchers, but also a conception of rigour that is characteristic of some forms of ethnography, for example that exemplified by interpretive anthropology. Many discourse analysts insist on the data being presented to readers so that the latter can assess directly the validity of the inferences made. By contrast, ethnographers often argue that it is not possible for them to make the data available to readers, that the validity of their inferences depends upon the success with which they have learned the culture of the people they are studying and thereby become able to interpret accurately what meanings various phenomena have for them. Moreover, they would question whether discourse analysts are actually presenting all of the resources they are using to readers (given that transcriptions involve theoretical assumptions about language use, that how they are read will depend upon the reader’s cultural background, and so on); just as they question quantitative researchers’ claims that they are giving readers access to their data by including tables (since considerable work has gone into transforming the data that were actually collected into these tables – see, for example, the arguments of Cicourel 1964).

There are other approaches that the radical critique rules out as well. These include those versions of life history work, or other uses of in-depth interviews, that are committed to documenting in detail the distinctive perspectives and experiences of particular people. Also challenged are approaches to qualitative research which, for political reasons, are committed to ‘giving voice’ to some category of person that has been marginalised. More generally, much traditional psychological and sociological theory, which has shaped qualitative as well as quantitative research, is treated as reifying social phenomena rather than studying them.

In many ways, the conception of inquiry employed by older ethnographers and life history researchers is that of nineteenth-century hermeneutics, which rejected the model of natural science but still insisted that a scientific approach is necessary, albeit one that is attuned to the distinctive features of human social life. However, many qualitative researchers today reject the idea that human social life can be understood by means of any notion of scientific method. Some would resist all injunctions about the proper nature of social inquiry, and perhaps even any commitment to inquiry at all (in favour of making political interventions or producing edifying or shocking aesthetic products). There are those, however, who do not take this argument quite so far, but still far enough to have radical implications for how we should go about social and educational research. For instance, philosophical hermeneutics rejects the idea that the rigour of research lies in its following a method. Instead, it is argued that other people can only be understood through engaging in dialogue with them. Moreover, what is discovered through dialogue is knowledge about important things in life, rather than simply empirical facts about the world. From this point of view, we do not seek to understand other people simply so that we can document their culture, how they view education, or whatever. Rather, we seek to understand others’ views about, say, education so as to deepen our own understanding of what it involves through exploring the tensions among different perspectives. From this position, as from some others, the very possibility of criteria of quality, in any hard and fast sense, is viewed as an expression of a positivism that must be rejected; though philosophical hermeneutics does not deny the importance of judgments of quality. Here social and educational research has been transformed into a philosophical enterprise; and, of course, there are those who would do the same, drawing on other conceptions of philosophy.6 In fact, in the case of philosophical hermeneutics, on the model of Goethe and others (see Heller 1961), the challenge is to natural science as well.

So, without even touching on post-structuralism and postmodernism, the usual bogeymen in this context, I have tried to show that there are very diverse conceptions of the requirements of inquiry to be found amongst qualitative researchers. And these are a major cause of disagreements in judging what is good quality work.

Realism versus constructionism

This second important division has already been introduced. As we saw, constructionism is one source of the arguments that seek to undermine the traditional practice of ethnographers, life history interviewers, and others to use interview data as sources of information about the world and/or as a basis for inferences about perspectives or attitudes on the part of the interviewees (and others like them) that to some degree govern their behaviour in other contexts. The generic move of

5 I have tried to provide a map of these elsewhere: Hammersley 2003b.

constructionism against realism is to insist that social phenomena do not simply exist independently of our attempts to understand them. Of course, many realists would accept, indeed insist, that social phenomena are the product of ‘people acting together’ rather than a product of social forces operating beyond their control. What is distinctive about constructionism is that it takes the argument that people’s actions, and social institutions generally, are culturally constituted and draws from it the conclusion that social phenomena can only be understood by describing the processes by which they are culturally constituted. In other words, a fundamental re-specification of the goal of inquiry is required. The focus becomes, not the phenomena themselves, and certainly not what might have caused them or what effects they have, but rather the processes by which they are identified and in effect created through identification by culture members. For example, rather than studying families as groups interacting within and beyond the context of their homes, the focus becomes how people talk about what families are, both explicitly and perhaps even more importantly implicitly, how they use notions of family life, ideas about kin obligations, etc in the course of their interactions with one another. Moreover, it is emphasised that these notions are not descriptions of patterns of social relations that exist independently of them but rather notions whose social significance lies in their functional uses rather than any representational capacity.

Constructionism shares this orientation with ethnomethodology, and one way of trying to do research in terms of this line of argument is associated with its founder, Harold Garfinkel, who assumes that there are means whereby the constitutive processes by which social phenomena are ongoingly produced can be uncovered or displayed in a manner that does not involve any cultural interpretation or inference on the part of the researcher. All that is involved, it is claimed, is description, in the sense of explication. The terms of this explication are entirely those of the culture embodied in the actions being explicated. It is worth noting that this position does not involve any fundamental form of epistemological scepticism, in other words there is not necessarily any denial that the actions people produce have a real existence as particulars. What is denied, though, is that these can be grouped into natural kinds in any other way than in terms of the processes that constituted them, and we must remember that there is no one-to-one correspondence between some constitutive cultural notion, like family form, and the actions it is used to produce and how they interrelate with other actions. This is because such notions may be ‘honoured in the breach’, ‘stretched’ in various ways, joked about, and so on.

A quite different way of operating, characteristic of radical forms of constructionism, is to treat the researcher as her or himself necessarily engaging in constituting the social world, or particular social phenomena, rather than in any sense simply describing or displaying how others are doing this, or how members in general do it. This often leads to a blurring of the boundary between social research and imaginative literature. In fact, from this point of view, the whole of social science cannot but be, in effect, simply a form of imaginative literature which is falsely conscious of its own character.

These two approaches, which by no means exhaust the kinds of work constructionism has generated, imply very different modes of assessing work. For the first, the concern is with whether the descriptions of the constitutive processes of social life are accurate, in other words whether what is displayed is indeed these processes. By contrast, for the second position, any idea of validity in this sense is rejected, and the accounts produced by researchers must be judged in non-epistemic terms, according to aesthetic, ethical, and/or political criteria. One way of thinking about the shift from realism to this kind of constructionism is in terms of the replacement of Kuhn by Rorty as the patron philosopher of many qualitative researchers. Whereas Kuhn still sees natural science as engaged in a process of inquiry, in which knowledge is accumulated, albeit in a discontinuous rather than continuous way, Rorty abandons the residual realism to be found in Kuhn’s account. Indeed, in effect, he erases the distinction between inquiry, which is concerned with gaining knowledge, and conversation, conceived as guided by an interest in ‘edification’. Indeed, he treats inquiry, understood in its conventional way, as labouring under a mistaken conception of itself, one that assumes that it is possible (and desirable) to claim superior knowledge of reality.

There are many varieties of both constructionism and realism, but I hope this discussion gives some sense of the implications of such a contrast in orientation for judgments of the value of particular pieces of research.

**Activism: the relationship with politics, policymaking, and practice**

What comes under this heading, like the others, is quite diverse in character. What I want to discuss here is all of those forms of qualitative research which reject the idea that the only immediate goal of inquiry should be the production of knowledge. Some of these see research as properly forming part of a larger problem-solving activity, and believe that it is rendered useless, or at least of much less value, when separated out from this. Indeed, there are those who argue that this process of separating out,

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7 There are many different varieties of constructionism, and the representatives of some of what I have labelled constructionism here would not describe themselves as constructionists. However, my aim has been to give some sense of the implications that a particular theoretical argument can have.
whereby knowledge production is institutionalised in special institutions, notably universities, is now being reversed through the rise of a second mode of knowledge production which occurs in the context of and directly addresses practical problems of various kinds (Gibbons et al 1994; Gibbons 2000). And, in many ways, this is simply an elaboration on some earlier arguments for action research. These approaches introduce extra or alternative considerations in judging the quality of research from the traditional epistemic ones concerned with the production of knowledge. In some versions these include economic criteria, notably whether there is demand for the knowledge being produced, whether it offers value for money, etc.

Ironically, perhaps, also coming under this heading are those views of social research that are influenced by Marxism where, to paraphrase its founder, the point is not simply to understand the world but to change it. Today, such approaches often adopt a ‘critical’ label, but much the same orientation can also be found under the headings of feminist, anti-racist, and ‘disabilist’ research. A key principle of Marxism was, of course, the unity of theory and practice. Thus, a distinctive feature of Marxism, ‘critical’ research, and these other approaches is that they combine a commitment to the production of knowledge with a commitment to the realisation of certain practical ideals: notably the abolition of inequities relating to social class, sex, race, etc. Unless it is simply assumed that the pursuit of knowledge will always have progressive political consequences, this raises the question of whether additional criteria of assessment are required besides validity. In its original form, Marxism conflated epistemic with other considerations, on the basis of the Hegelian assumption that the development of knowledge and the realisation of other ideals are strongly interrelated in a historical dialectic. More recently, that assumption has been abandoned, and as a result other goals have often been raised above, or have replaced, epistemic ones. Thus, some arguments for interventionist research prioritise political goals or ethical values, arguing that simply to observe the world as a researcher is a form of voyeurism and is not justifiable - that, instead, one must be committed to working for social justice, and that one’s research must be judged in terms of that ideal.

There are also arguments for intervention that are aesthetic rather than either scientific or ethical in character. Here research is to be a form of performance art, indeed it may be argued that it cannot but be this, and the requirement is that it be consciously shaped aesthetically, at most only hiding behind the masks of science or ethics in an ironic, knowing way. It is not difficult to see that the criteria relevant here would be different again.

Conclusion

In this paper, I have sought to give a sense of the variety of approaches to inquiry to be found within qualitative research today, these providing the resources that are used in making judgments about quality, and in specifying criteria of assessment. If my argument is correct - that there are some very deep differences in orientation to be found not just between quantitative and qualitative researchers but also among qualitative researchers - then it is not very surprising that there has been little agreement about quality. The issue of most immediate importance, it seems to me, is whether the various approaches to be found in the field are incompatible, and if they are what the response to this should be. There are at least the following possible views here:

1. The apparent differences are spurious, either because they are not genuine but simply arise from differential emphasis on shared concerns, or because they amount simply to rhetoric, that when it comes to actually doing educational research there is much less variation than the methodological debates suggest. In other words, in practice, in doing research everyone is forced to take account of more or less the same concerns. From this pragmatist position, even the wilder flights of methodological fancy can prove valuable, for example as antidotes to traditional prejudices; though one should not take any form of methodology straight or in large doses. (Seale 1999 provides an example of this sort of position.)

2. There are deep-seated incompatibilities here, but they are not a matter of concern. We must simply recognise that diverse forms of social research will develop, each having its own distinctive conception of what is good quality work, and its own ways of applying this. (Hodkinson 2004 illustrates such a position.) It is worth noting that this does not have to be taken as implying that there are incommensurable paradigms, the exponents of which simply cannot understand one another. An alternative metaphor is different language

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8 For an assessment of the rationale for action research, see Hammersley 2004.

9 I have not been able to find any clear presentation of this argument for intervention, but an example of the approach is Miller and Whalley 2005. I am not suggesting that these authors would accept my description of this argument as an accurate account of their motivation.
communities, where while true translation may not be possible learning the other languages is.  

3. There are serious differences in perspective, but some means needs to be found to at least reduce them, so as to increase the level of agreement in educational researchers’ judgments about what is and is not good quality work. (Feuer et al 2002 exemplify this position.)

Elsewhere, I have made clear that my own position comes under the third heading (Hammersley 2005). In the second paper I will try to outline what I think the implications of this are for the issue of quality criteria.

References


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10 This is an analogy that Kuhn uses in his later work, as against the perceptual analogy on which he relies in The Structure of Scientific Revolutions.

Deutscher, I. (1973) What We Say/What We Do: Sentiments and acts, Glenview IL, Scott, Foresman and Co.


