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Social Science Methodology Today: Diversity or Anarchy?

A striking feature of the methodological literature in the social sciences is its sheer size, and the fact that it continues to increase at a rapid rate. Another is the diverse, not to say contradictory, character of this body of writing. Forty years ago, there was considerable consensus about the nature of social and educational inquiry, how it could be pursued, and what it should aim at producing. Today, there is little wide agreement. In this talk I will discuss some specific issues about which there is currently sharp dispute. These include the relationship between quantitative and qualitative methods, the role of interviews, and the nature of the products of research or the uses to which they can be put. These by no means exhaust the disputes to be found in the methodological field, but they do provide us with some insight into the tensions and conflicts that now operate within that field. In relation to the first issue, while in some quarters there is strong advocacy of 'mixed method' research, other influential voices challenge any attempt to combine quantitative with qualitative strategies, on the grounds that this ignores fundamental differences in philosophical assumption and/or political orientation. As regards the second issue, in the past, debates over the role of interviews had focused to a large extent on the relative advantages of structured versus relatively unstructured forms of questioning. However, today, some qualitative researchers express doubt about whether interview data can tell us anything about what people think, say, or do outside of interview situations. Finally, there are important debates surrounding the idea that research can and should contribute to evidence-based policymaking or practice. Having outlined some of the arguments in these areas, I will conclude by reflecting on whether what is currently displayed in the field of methodology is a refreshing diversity of approaches or a dangerous anarchy.

I should begin by confessing that I was initially rather reluctant to talk at this conference. I didn't think I was an appropriate person. There is a story, perhaps apocryphal, about an embarrassing case of mistaken identity at a conference. There are two educational researchers with the same name, and the organisers of the conference invited the wrong one. Unfortunately, this was only discovered when the speaker actually turned up! As far as I know, there isn't anyone-else with the same name as me, but I have two characteristics which do not fit me well for this conference. First, I am the world's worst second language learner. I remain at what Wilfried Decoo has described as 'the level of frustrating inadequacy' (Decoo 2001). I have managed largely to forget much of what little I was once capable of in both French and Russian. And the second disqualification is that I know little about computer hardware or software, or about computer-assisted language learning programs. Neither of these characteristics is anything to boast about - I mention them solely in mitigation.

What I *do* know something about is the field of methodology in social and educational research, and - to be fair to the organisers - this was precisely what I was asked to talk about. Perhaps I should just give you a brief sketch of my background. I began my career as a qualitative researcher or ethnographer, investigating patterns of

classroom interaction and the perspectives of teachers and pupils in secondary schools. This was at a time when a revolution was taking place in British research on education, marked by a shift away from quantitative approaches towards qualitative method. Since that time I have continued to work primarily with qualitative data, though I've sometimes sought to combine it with quantitative data. And, over the years, I've become increasingly preoccupied with methodological issues, so that the bulk of my writing in recent times has been in the field of methodology.

The issue I want to focus on today is the methodological diversification of social and educational research over the past few decades, and what our attitude should be towards this. Before looking at specific issues, let me sketch some general trends in very crude terms.

From the 1950s through to today, in Western societies, there has been an enormous increase in the amount of social and educational research, and in the number of social and educational researchers. And there has probably been an even greater increase in the amount of methodological writing. Indeed, the methodological literature in the social sciences is now so large that I doubt anyone could be familiar with all of it, or even keep up with new publications. Moreover, one result of this expansion has been a process of fragmentation, so that developments in particular fields have taken place with relatively little influence from what is happening in other areas; despite some cases where cross-over effects have been quite significant. And this fragmentation has increasingly generated quite fundamental philosophical and political disagreements not just about how research should be carried out but even about what its purpose and products should be.[1]

Whereas for many quantitative researchers and some early qualitative researchers the guiding model was that of natural science, at the very least in rhetorical terms, qualitative researchers have increasingly turned away from this. Some rejected this model in favour of a different form of science that they believed was properly suited to studying human social life, one that took account of the need to *understand* the perspectives of the people being studied, to recognise that what they respond to is not the world as described by the researcher but the world as they perceive it. More recently, however, the tendency has been to reject the very idea that social and educational inquiry can or should aim to be scientific in any sense. There's been a strong inclination on the part of some qualitative researchers to turn to philosophy, literature, literary theory, or even art as alternative models to that of natural science.

Now, rather than discussing these trends in general terms, what I want to do here is to focus on three issues that illustrate some of the diversity in orientation that now exists in the methodological field, and the fundamental debates that are taking place.

The first issue I'll discuss is the relationship between quantitative and qualitative methods. We can identify a number of different positions here:

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[1] Some recent publications give a sense of the diverse orientations to be found within qualitative research in particular: Denzin and Lincoln 1994 and 2000, Atkinson et al 2001.

1) Quantitative methods are superior, being essential to a scientific approach.

2) Qualitative methods are superior, because they are specially attuned to understand human social life.

3) Quantitative and qualitative methods are *complementary*: if they are combined their contrasting strengths can be maintained and their weaknesses minimised.

4) Quantitative and qualitative approaches derive from incompatible philosophical traditions that are true in their own terms. Therefore, as researchers we must simply make a personal commitment to one or the other; the two approaches should not be mixed or combined.

5) The distinction between quantitative and qualitative approaches is fundamentally mistaken: a more subtle set of distinctions is required, capturing the options available in dealing with various aspects of the research process – from formulating research questions, through selecting cases and choosing data collection methods, to analysis and writing up.

The idea that quantitative method is essential to natural science, and therefore is superior to qualitative method, was dominant in much of social science around the middle of the twentieth century. Since that time this idea has increasingly been challenged, on the grounds that a distinctive approach is required to understand human actions, and as a result of changes in philosophical ideas about natural science. In particular, the idea that natural science is carried out on the basis of some standard, and fully explicit, method has been undermined by a number of writers, perhaps most influentially Michael Polanyi and Thomas Kuhn.[2]

In the early stages of the rise in influence of qualitative method its advocates often put forward the idea that it was complementary to quantitative research, that it could provide types of information or data about particular kinds of people and situations that were not accessible to experimental or social survey method. However, later, qualitative researchers often came to treat their approach as superior. Paradoxically, at the same time, many of those engaged in quantitative research moved from a position of largely rejecting qualitative method as unscientific towards recognising that the two approaches could complement one another. Indeed, a few quantitative researchers in education switched completely to qualitative method.

Towards the end of the twentieth century two further shifts seem to have occurred, at least in some areas of inquiry. First, some qualitative researchers became more cautious about claiming the outright superiority of their approach. They did not do this because they had doubts about its value but because, while rejecting quantitative method as positivist, they saw any claim to methodological superiority as itself positivist; in the sense of implying a commitment to absolute truths that only qualitative method could gain access to, and they rejected the very notion of truth. In light of this there was a tendency to treat quantitative and qualitative traditions of

[2] See Polanyi 1959, 1962 and 1966, and Kuhn 1970. For excellent commentaries on the work of Kuhn, see Hoyningen-Huene 1993, Bird 2000, and Sharrock and Read 2002.

inquiry as incommensurable paradigms, borrowing that idea from a relativistic interpretation of Kuhn's philosophy of science.[3]

A second development at the end of the twentieth century and into the twenty-first was a revival in the fortunes of quantitative method, stimulated by the demand from governments and others that social research serve evidence-based policymaking and practice. The evidence-based practice movement began in medicine, and because this treated the randomised controlled trial as the gold standard, there has been increasing pressure to employ this method in the study of social and educational policy and practice. Furthermore, this has led to a broader re-valuation of quantitative method, albeit sometimes with a parallel emphasis on the complementary role that qualitative work can play. Thus there has been growing advocacy of so-called mixed methods research, though this is very often conceptualised in terms that many qualitative researchers regard as modelled on the inappropriate logic of quantitative method.[4]

The final view I listed about the relationship between quantitative and qualitative approaches, the one that challenges the very distinction between them, has never been very influential. I mention it only because it happens to be my own position! From this point of view we need a more subtle set of distinctions, relating to particular aspects of the research process, rather than a crude global dichotomy.[5]

Those advocating these different views of the relationship between quantitative and qualitative methods have drawn on a range of philosophical resources. As already hinted, the superiority of quantitative method was generally advocated on the basis of some form of positivism. By contrast, advocacy of qualitative work has appealed to rather different philosophical sources, including American pragmatism, phenomenology, the so-called linguistic philosophy of Austin, Ryle, and Wittgenstein, hermeneutics, structuralism, post-structuralism and postmodernism.[6] These philosophical ideas have been drawn on selectively, and have often been blended together to justify particular qualitative approaches, whether participant observation, in-depth interviewing, or discourse and narrative analysis.

In fact, as previously noted, the trend has been towards a fragmentation of qualitative method in which particular methods of qualitative data collection and analysis are championed against others. This has gone along with a more general trend towards increasingly micro analysis, with reliance placed for data on transcriptions of audio- or video-recordings. This is characteristic, for instance, of discourse analysis, which has become very influential in some fields. Here, discourse is investigated as a phenomenon in its own right: the task is to identify discursive resources and strategies, without ascribing intentions to the people whose discourse is

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[3] An example of this tendency is the methodological writing of John K. Smith: Smith and Heshusius 1986, Smith 1989, 1993, and 1997.

[4] For useful discussions of the relationship between quantitative and qualitative method, and the possibilities of combining them, see Bryman (1988) and Brannen (1992). For an argument to the effect that mixed methods research is a new third paradigm, transcending the two earlier approaches, see Tashakkori and Teddlie (2003).

[5] For an initial effort to develop such distinctions, in relation to the selection of cases, see Hammersley 1992:ch11. On the diversity of views about case study, see Gomm et al 2000.

[6] For useful introductions to some of these strands of philosophical thinking and their implications for social research, see Anderson et al 1986, Outhwaite 1987, Hughes 1990, and Crotty 1998.

being studied, and without seeking to explain the use of particular discursive techniques in terms of individual motivation. It is symptomatic, however, that there are very different versions even of this approach. Perhaps the most influential kind of discourse analysis is that pioneered by Jonathan Potter and Margaret Wetherell, but even Potter and Wetherell now differ from each other in the kind of analysis they practise.[7]

It is perhaps worth pointing out that there are those who argue that a further turn has taken place, beyond and in some respects reversing the discursive turn, this time a turn towards practice, towards focusing on the practical, situated character of human action. And some of the work under this heading has been concerned with interaction between humans and computers.[8]

In summary, then, there is continuing diversification in qualitative approaches generating increasing fragmentation in the field of research methodology. And as a result there's a range of discrepant views about whether and how quantitative and qualitative methods can and should be combined.

At this point, let me move to the second issue, which follows on directly and illustrates the trend I have just been discussing. While early qualitative researchers set out from criticisms of survey researchers' reliance on structured interviews and questionnaires, proposing the superiority of participant observation, they usually complemented this kind observation with in-depth interviews, and over time an increasing amount of qualitative research has come to rely *exclusively* on interview data. One reason for this has been an influential philosophical strand in the thinking of qualitative researchers which stressed the importance of capturing the voices of the people being studied, trying to understand their experience in its own terms; rather than interpreting or judging it in external terms. I suspect that there were also practical reasons for the increasing use of interview data, following from the fact that it is often easier to arrange a few interviews than to negotiate access to a setting in order to engage in long-term fieldwork. It's also easier to fit data collection via interviews into busy academic lives than it is to carry out time-consuming participant observation. But perhaps I'm being too cynical.

Anyway, this increased reliance on interviews has stimulated severe criticism on the part of some qualitative researchers. This is what has been referred to by Elizabeth Murphy and her colleagues as the radical critique of interviews.[9] The critics have not, for the most part, been against the use of interview data per se as much as against its use to draw the sort of conclusions that are routinely drawn from it. They question two standard uses of interview data, employed by both quantitative and qualitative researchers.

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[7] For overviews of different forms of discourse analysis that involve little overlap, see Schiffrin 1994 and Howarth 2000. For a bibliographical guide, see Hammersley 2003a. The core text for Potter and Wetherell's work is Potter and Wetherell 1987. For a distinctive approach to discourse analysis applied to education, see Maclure 2003. For a discussion of some of the disagreements and problems within discourse analysis see Hammersley 2003b and the references given there.

[8] On the turn to practice, see Schatzki et al 2001 and Stern 2002. For some of the work focusing on practice concerned with humans and computers see Suchman 1986 and Button and Sharrock 1996.

[9] On the radical critique of interviews, see Murphy et al 1998, Dingwall 1997, Gubrium and Holstein 2002, Atkinson and Silverman 1997, and Hammersley 2003c.

First, there is the use of informants as witnesses to provide us with second-hand information about the world: about what informants have themselves observed or heard about from others. An example would be asking a teacher to report on how students use a language learning program. The critics rule this sort of information out on the basis that any accounts informants produce will reflect their social positions, local cultures, or discursive repertoires rather than corresponding in any straightforward way to what actually happened. Indeed, they often question any idea that informants' accounts can simply correspond to reality. They insist that it's always possible to describe the same situation in many different ways. Given this, they argue that we should *not* treat informants' accounts in terms of how accurate they are. Rather, we should study how people construct accounts, what resources they draw on, and perhaps why they portray things in the ways that they do.

The second use of interviews that is challenged by the radical critique is where they are employed to identify stable attitudes or perspectives on the part of informants that govern their behaviour. An example would be interviewing students to try to understand their attitudes towards second language learning, assuming that this can tell us something about how, or how well, they learn. The critics argue that what people say and do is strongly shaped by context, rather than being fixed or general. And, as a result, what people say in interviews largely reflects the peculiar characteristics of the interview situation, rather than telling us what they would say or do in other contexts. In short, the critics argue that interview data can only be used to identify the ways in which meaning is co-constructed in interviews, or the discursive practices employed by both interviewees and interviewers. It cannot be relied upon to tell us anything about what happens in the world outside the interview.

This line of argument reflects what I referred to earlier as the discursive turn in many areas of social and educational research. And implicit in this discursive turn is a way of thinking about social life that's often referred to as constructionism. This is the idea that we must not treat social or psychological phenomena as objects that are simply given in the world and therefore readily available for description and explanation by researchers. Rather, we must recognise and document how these phenomena are ongoingly constructed and sustained through processes of social interaction. Within psychology we can see this trend in notions of situated cognition and discursive psychology.[10]

The radical critique involves an application of this constructionism to the interview. We are called on no longer to treat what is said in interviews as a stable representation of some hidden reality. Rather, we must attend to how what is said is constructed, recognising its situated and contextually variable character. A substantial shift in the focus of inquiry is involved here: from seeking to describe and explain objects out there in the social world to focusing on how people talk about the world and thereby bring it into existence on particular occasions.

Incidentally, some qualitative researchers have extended this constructionism to include consideration of the ways in which researchers themselves construct the phenomena they study, in and through studying those phenomena. Under the

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[10] On situated cognition, see Lave 1988, Lave and Wenger 1991, and Chaiklin and Lave 1993. On discursive psychology, see Edwards 1997.

influence of postmodernism this has led to the adoption of so-called experimental kinds of writing often modelled on literary modernism. These seek to subvert any assumption that a research report simply represents phenomena that exist independently of it, even phenomena like the discursive practices of interviewees.[11]

I perhaps should say that, in my view this radical constructionism, including the critique of interviews that I've outlined, is philosophically misconceived. While it contains some important cautions, I think it is overplayed. Of course, we cannot assume that what informants say in interviews simply displays stable attitudes that govern their general behaviour, nor can we treat their reports of what goes on in their lives as if these were automatically true. However, we can often use interview data to identify relatively stable dispositions linked to types of context and/or use these data as a source of information about what happened in situations which we as researchers did not or could not observe directly. What the radical critique of interviews points up, it seems to me, is the need for cautious interpretation of interview data and for triangulation across methods.

Finally, let me turn to the third issue I want to discuss. This concerns variation in ideas on the part of researchers about the sort of product their work generates and how it can contribute to policymaking and practice. A distinction is sometimes drawn between what are called the engineering and enlightenment models. From the point of view of the engineering model, research develops new policies, techniques, or forms of practice and/or evaluates how well they work; much as engineering research produces tools, technologies or physical structures of various kinds, or methods of testing the performance these.

In terms of this engineering model, the findings of social and educational research are to be 'applied' or 'implemented' and the expectation is that they will be used in relatively predictable ways that have identifiable and beneficial results. For the first half of the twentieth century, this model probably shaped the way that most social scientists and others thought about the role of research. And it continues to be very influential in some fields. However, partly as a result of the rise of qualitative research, over the second half of the twentieth century the engineering model has declined somewhat in influence, and what has come to be referred to as the enlightenment model has become *more* influential.

By contrast with the engineering model, the enlightenment model generally treats the impact of research as more diffuse, and therefore as more contingent and uncertain in outcome. In one version, this model portrays research as supplying a new mode of viewing the world that replaces the spontaneous and ideological ways in which policymakers and practitioners are normally inclined to view things. For other advocates of the enlightenment model, however, the contribution of research is seen as much more uncertain and small-scale: it is a matter of providing ideas and information that policymakers and practitioners can make use of as resources; as, when, and how they find this appropriate. In other words, *how* those resources are employed is not built into the research itself, in the way it is supposed to be for the engineering model. From this point of view, the relationship between research and

[11] For the origins of this line of development, see Clifford and Marcus 1986. The most entertaining example of literary 'experimentalism' in social science, in my view, is Ashmore 1989.

policymaking or practice is much more distant and unpredictable than it is for the engineering model.[12]

Interestingly, in the early years of this century, as a result of the influence of the evidence-based practice movement, there has been a revival in the fortunes of the engineering model. It is in terms of that model that many governments now see social and educational research as contributing to their policymaking and serving as a basis for improving educational practice. And some researchers, seeking to close what the believe is a credibility gap between research and practice, are promoting forms of inquiry that embody the engineering model. This is true, for example, of the various kinds of design-based research.[13]

The evidence-based practice movement began in medicine. It involved criticism that many of the remedies that doctors use have not been subjected to rigorous test by means of research, and that where they have been assessed it has often been shown that they don't work and sometimes even have negative effects. So the argument is that clinical practice should become evidence-based, in the sense of being founded on sound research evidence about what does and does not work. And the key scientific tool for carrying out the testing of policies and practices, as I mentioned earlier, is the randomised controlled trial, the kind of experiment that is used in testing new drugs, along with systematic review of results from such trials.[14]

Conclusion

I have identified a number of areas in which the diversity and disagreement currently to be found in the field of social research methodology is displayed at its sharpest. I want to end by raising the question of whether this diversity is a good or a bad thing. Does it amount to a form of pluralism that ought to be applauded, or is it a major barrier to progress in educational research and to its playing a worthwhile role in relation to policymaking and practice. Both answers to this question can be found in the recent literature. Some argue that there's a need to build greater methodological consensus, that only by doing this can social and educational researchers develop a cumulative body of knowledge that can inform policymaking and practice effectively. Others, including many qualitative researchers, argue that attempts to create consensus amount to the re-imposition of a positivist paradigm, and that this will undercut much valuable research.[15]

As is often the case in methodological controversies, I find myself somewhere in the middle on this issue. I don't think that any simple bringing-together of all the various methodological trends that have developed can be engineered, or even

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[12] For further discussion of the engineering and enlightenment models, see Bulmer 1982, Finch 1986, and Hammersley 2002.

[13] On design-based research in education, see Brown (1992), Collins (1992) and *Educational Researcher* 32, 1, January/February, 2003.

[14] For advocacy of the model of evidence-based medicine, the notion of evidence-based policymaking and practice, and of the importance of randomised controlled trials, see Mosteller and Boruch 2002, Oakley 2000 and 2003. For assessments and criticisms of the evidence-based practice movement, see some of the articles in Trinder 2000 and Thomas and Pring 2004.

[15] For the arguments on various sides of this debate, see Feuer et al 2002, Hodkinson 2004, and Hammersley forthcoming.

brought about through enlightenment. Indeed, some of those trends seem to me to be so obviously antithetical to the very nature of research that they can't be tolerated; this being true of extreme positions on both quantitative and qualitative sides. Nevertheless, there is a need for more effort on the part of social and educational researchers to build bridges among different kinds of work and to see how these might usefully be combined or integrated. At the same time, I'm not under any illusion that this can be done rapidly and easily, or that it will ever produce complete consensus or homogeneity. Indeed, complete consensus would not be a good thing; some difference and diversity is always necessary in order to stimulate further development.

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