This contribution is one of an intermittent series of invited pieces from long standing and well known researchers in their field, personally reflecting on methodological developments and other research issues over the span of their careers.

Paradigm wars: some thoughts on a personal and public trajectory

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It is often said that research methods are a matter of rational choice: it is the research question, or problem, which ought to dictate the method to be used. This may be true to some extent, but there are other more powerful reasons why social scientists choose the research methods they do. Underlying philosophies of social science and long-held and much cherished tenets about epistemology are prime among these. Moreover, such an interplay between epistemological position and methodological decision is enormously affected by social context. We pay attention to what other social scientists are doing, to fashions in both methodology and topic—the things it is considered proper for social scientists to study; we are affected by research funding and publishing opportunities, by the material resources available to support our work, by intraprofessional rivalries and difference, and by politics—both in its commonly understood sense and as applied to power relations between academics and those who take part in research. Apart from what we do, there is the whole issue of how others construct our work. And this is only some of what goes on. In short, it is a very complicated business.

Arguing about method: introduction

In his classic *The Sociological Imagination* (1959), C. Wright Mills takes a fairly dim view of method as self-conscious procedure. ‘Serious attention should be paid to general discussions of methodology only when they are in direct reference to actual work’, he instructed (p. 122). *The Sociological Imagination* is famous for its attack on the twin evils of grand theory and abstracted empiricism. Wright Mills’ passion for a creative, lateral-thinking, problem-oriented social science did not go down well among some of his methodological colleagues. In a dialogue with Paul Lazarfeld, Wright Mills reputedly opened the conversation by quoting the first sentence of the book (p. 3): ‘Nowadays men often feel that their private lives are a series of traps’. Lazarfeld’s response was: ‘How many men, which men, how long have they felt this way, which aspects of their private lives bother them, when do they feel free rather
than trapped, what kinds of traps do they experience, etc etc?’ (cited in Elcock 1976: 13).

This exchange sums up different positions in the long-running argument between so-called ‘quantitative’ and ‘qualitative’ methods. It is hard to say quite when this battle got off the ground, but there is little sign of it in the general methodological and professional literature before the 1960s. After that time, and partly infused by radical critiques of science, dissatisfaction with ‘quantitative’ methods generated increasing appeal to other forms and methods of collecting social science data (Bryman 1988, Cicourel 1964, Rose and Rose 1976). These built, of course, on earlier work, including that of Weber (1947, see Platt 1985), Schutz (1932) and Blumer (see Hammersley 1989). But one highly significant driving force behind the paradigm war from the 1970s on was nothing to do directly with developments in social science. The arrival of feminism as a political and social movement underscored the importance for political reasons of using ‘qualitative’ research methods, and gave an altogether new gloss to anti-science critiques of quantification.

The old and new Oakleys

My own early work developed a reputation for ‘qualitative’ research (see e.g. Reid 1983, Spender 1985). More recently, however, much of my work has been seen as more easily fitting the ‘quantitative’ mould, with a particular stress on methodologies of rigorous evaluation. This has gained me another kind of reputation—what the informants in one recent case-study called ‘this Oakleyesque view’ of ‘gold-standard-copper-bottomed-man(sic)-in-a-white-coat-randomized-control-trial evaluations’ (Bonell 1999). These contrasting reputations suggest a shift of position, which is the source of a certain amount of puzzlement. In a seminar I gave in a Swedish department of sociology in 1997, the commentator on my talk desperately produced evidence from my previous writings of ‘the old Oakley’—a persona she much preferred—which she then contrasted with ‘the new Oakley’, asking me to account for the difference. I have been accused of some sort of strange conversion experience, of being brainwashed by medics, of letting the ‘qualitative’ and feminist sides down; at the very least it has been important for people to stress that, while my methodological repertoire has seemingly expanded, a primary allegiance to the ‘qualitative’ tradition must remain (see e.g. Brannen 1992, Leavey 1997).

This paper provides a brief account of my personal methodological trajectory. But what it emphasizes are the misconstructions involved in the asking of the question—why the change in methodological position? I argue, instead, that there has been little ‘real’ change. Rather, what my ‘case’ illustrates is the co-option of individual methodological positions by prevailing paradigm arguments. The fundamental question is one about why social scientists (and others) conceive of different research methods as opposed in the first place.
Interviewing and other trials

In the late 1960s and early 1970s I interviewed women about housework (Oakley 1974). The housework research, in the course of which I myself became a feminist, was followed by another project on women’s transition to motherhood (Oakley 1979, 1980). Both projects employed in-depth interviewing as a way of generating personal narratives about experiences which, at the time, were viewed within mainstream social science (and society more generally) as unimportant, because they are private, domestic and belong to women’s lives. One of my most quoted publications, ‘Interviewing women: a contradiction in terms?’ (Oakley 1981) came directly out of the transition to motherhood research. This took the textbook model of social science interviewing and showed how interviewing in practice, especially a woman interviewing other women, did not easily fit the uncomfortable ideal-type mould of the interviewer as impersonal data-collector, and the interviewee as subservient data-provider. All this work fed into an emerging and highly vocal literature on social science and women, within which ‘qualitative’ research came to be highlighted quite unambiguously as the preferred paradigm, with ‘quantitative’ research being earmarked as the work of the patriarchal devil (see e.g. Mies 1983, Stanley and Wise 1983). The feminist case against quantification focused on ‘the three ps’—positivism, power and p values (see Oakley 1998a). These were taken as countermanding the feminist case for a social science based on everyday experience as it is lived even (and especially) by the powerless. Experimental research methods represented the apex of the quantitative paradigm as criticized by many feminists, who saw them as synonymous with an approach to knowledge which intrinsically violates the agency and autonomy of those who are known about (see e.g. Donovan 1990, Reinharz 1992).

In 1979 I moved from a department of sociology to a health care research unit specializing in maternity services research. Social science was in a minority position in this unit, and I found myself exposed there to a different research tradition in which quantification, and especially prospective experimental studies, dominated the scene. A main driving force behind the Unit’s work was evidence that health care practices are more often based on guesswork, personal preference, tradition, professional modelling and fear of litigation than on convincing and reliable data about their appropriateness, effectiveness and safety. As a consequence, many lives are damaged. Exposure to this evidence drew my attention to the difference between ‘controlled’ and ‘uncontrolled’ experimentation (see Chalmers 1986). Uncontrolled experimentation is what usually happens in professional practice. Controlled experimentation, on the other hand, happens within the context of a formal research study, whose design is capable of providing an answer to the question: does this treatment/intervention work or not?

The point was/is, that what applies to the practices of doctors and other health care professionals also applies to any social group which sets itself up as possessing expert knowledge about the best way to intervene in other people’s lives. Teachers, social workers, criminologists, volunteer ‘do-gooders’, politicians and other promulgators of public policy are all guilty
of choosing to do what they believe in, rather than what has been demonstrated to be the best thing to do. Medicine’s prime way of knowing, the randomised controlled trial (RCT), evolved as an alternative to the uncontrolled experimentation of ‘normal practice’. In this sense, it is just as relevant to the evaluation of social interventions. Indeed, and for this reason, early developments of the method took place in social science not, as is usually believed, only in medicine (see Oakley 1998b).

By 1984 I had started work on just such a study, an RCT of social support and pregnancy. The study used socially supportive interviewing as an intervention, and evaluated the effects of this on women and babies by having a ‘control’ group of women who were not interviewed (see Oakley 1992). Since then I have also become involved in RCTs of other social interventions—peer-delivered sex education for young people (Stephenson et al. 1997), out-of-home daycare for preschool children (Roberts et al. 1997), and social support strategies for disadvantaged mothers (Oakley et al. 1997). In the early 1990s we also embarked on another programme of work which set out to look critically at the evidence base of health promotion, as a particular kind of social intervention. In a series of systematic reviews (Oakley et al. 1995a, 1995b, 1996, Oakley and Fullerton 1995, Peersman et al. 1998), we were surprised to discover how inadequately the effects of many of these interventions have been assessed, and how often their proponents argue that they work, when the evidence is either non-existent or consistent with the opposite being the case. For example, an intensive health visitor intervention aimed at decreasing falls among older people actually increased them (Vetter 1992); another, of social work services for older people, succeeded in raising mortality and institutionalization rates (Blenkner et al. 1974); social work counselling for boys at risk of becoming ‘delinquent’ made this more, not less, likely (Powers and Witmer 1951, McCord 1981); and advising parents about the dangers to children’s health of passive smoking made them less likely to stop smoking (Irvine et al. 1999).

Mixing methods

What I have described is undoubtedly a process of methodological development, but it is not a revolution. For example, in the housework study, the women’s accounts of their lives jostle for attention with 2 × 2 tables and tests of statistical significance. An important motive was an attempt to calculate the length of housewives’ working week in order to put domestic labour in the language of the paid labour market and the economics of the Gross Domestic Product: this required much painstaking counting. When I published the study, I was slated in certain quarters both for using tests of significance and percentages and for being a feminist, although the same people who made one sort of criticism usually did not make the other. One of the volumes which reported the transition to motherhood study (Oakley 1980) also made much use of statistical tests as a way of arriving at a model of childbirth as a human experience—one which challenges the human capacity to respond unprepared, immediately and
constructively to new and often painful experiences. In this book I argued that the opposite of what people probably expected me to argue—I suggested that women’s reactions to childbirth could best be understood by seeing them as people rather than as women. As this is my most neglected book, I have often speculated that my use of the ‘quantitative’ mode was what put people off. Many simply found it jarred with the ‘softer’ style they expected from a feminist social scientist.

Similar caveats could be applied to the labelling of my later work as ‘quantitative’. For example, while the prospective experimental design of the social support and pregnancy study was clearly that of a randomised controlled trial with its traditional emphasis on the power of numbers, the study was prompted directly by experiences of interviewing as both a social relationship and form of action. It was ‘qualitative’ in the sense of using indepth interviewing as a means of encouraging narratives from research participants about their experiences, and also because we collected a great deal of information about the processes of developing and implementing such an intervention. The challenge as we saw it—and this applies to RCT’s more generally—is to integrate the collection and analysis of ‘qualitative’ and ‘quantitative’ data so as to arrive at an interpretation which makes productive use of both.

Quantification and experimentation for women (and other people)?

Insofar as I can be said to have undergone any ‘conversion’ experience this has been limited to understanding four things. The first is the requirements imposed on a socially responsible social science by professional arrogance. It is because doctors, teachers, social workers and others are so prone to launch interventions without knowing their effects that social science is obliged to use the best tools at its disposal to scrutinize such activities. Method here is (as Wright Mills advised) properly harnessed to the service of the social problem itself, rather than the other way around.

Secondly, I learnt that well-designed and ethically conducted RCT’s offer an elegantly simple approach to assessing the effects of such arrogance, as well as yielding answers to questions about the impact of all sorts of interventions. If the term ‘randomised controlled trial’ upsets people (which it undoubtedly does), then ‘socially equitable comparison test’ offers an equally truthful but less offensive description (Oakley 1998c). Thirdly, I discovered that in our excitement to dismantle patriarchy I and other feminist social scientists had mistakenly thrown at least part of the baby out with the bathwater. Women and other minority groups, above all, need ‘quantitative’ research, because without this it is difficult to distinguish between personal experience and collective oppression. Only large-scale comparative data can determine to what extent the situations of men and women are structurally differentiated. And as targets of the health care industry in particular, women also need well-designed experimental studies which are capable of reliably evaluating the increasing numbers of medical procedures encountered over a lifetime (Foster 1995, Oakley
Without such studies, many of the harmful effects of such routine procedures as hormone treatment for miscarriage, ultrasound scanning in pregnancy, induction of labour and hormone replacement therapy would remain unknown.

The fourth thing I learnt (and am still learning) is less straightforward. It concerns the question: what are research methods for? In an era dominated by postmodernism, postfeminism and a general acceptance of multiple meanings, it is obviously unfashionable to suggest that the aim of research methods is to provide some sort of approximation to what is ‘really’ going on. Yet this is, I think, what drives and should drive most social scientists, just as most of us live our everyday lives as though reality exists and can be known about. Put the other way round, this concern becomes one about the extent to which different research methods offer protection against bias, against the possibility that we will end up with misleading answers. Much ‘qualitative’ research is simply too unsystematic, too masonic in nature, too cavalier about appeals to ‘triangulation’ and/or analysis using computerized software packages, to establish serious credentials for being trustworthy. For example, in four lists compiled by different researchers of criteria for judging the trustworthiness of qualitative research, there are 46 different criteria, of which only two are common to all four lists; terms capable of varying interpretations such as ‘clear’, ‘adequate’, and ‘careful’ abound (Boulton et al. 1996, Cobb and Hagemaster 1987, Mays and Pope 1995, Medical Sociology Group 1996, see also Oakley 1998a). Of course it is the case that research methods must fit the question being asked and this means that ‘qualitative’ methods are undoubtedly sometimes the most appropriate choice. But all methods must be open, consistently applied and replicable by others.

On labelling

Sociologists are in a better position than most people to understand the social processes involved in labelling. The above brief excursion through one methodological career demonstrates some changes in direction prompted by the arrival of new understandings, but what it illustrates most of all is the way in which the labelling of work as ‘qualitative’ or ‘quantitative’ proceeds from the context and the culture and may have little to do with the intentions of the individual. I have never presented myself as an single-minded advocate either of ‘quantitative’ or ‘qualitative’ methods. I have certainly stressed the need for adopting methods appropriate to research questions, for choosing methods which are sensitive to power relations, and for the ethical conduct of research (which also means well-designed research able to answer the questions it is set up to answer); but all these precepts apply right across the methodological board.

One disadvantage of labelling is that it introduces an artificial problematic which then has to be explained. A more fruitful line of inquiry is to consider the problematic itself. The alignment of feminism with ‘qualitative’ methods, at one extreme, and the association of experimental methods with medical science, at the other, speak to, and of, a long drawn out
process of gendering which has informed the development and use of ways of knowing across the sciences. Semantic attacks and other activities distract attention from what Paul Meehl (1986: 317) has identified as the central question: ‘To what extent does this discipline contain knowledge that brings some sort of credentials with it?’ A secondary question is whether there is any kind of credentialled knowledge that is not, in some sense, ‘scientific’. This takes us back to the very origins of social science, which is where we need to be in order to understand, not just one person’s journey through the space and time of paradigm wars, but their institutionalization in a more global, and thus ultimately more interesting, sense.

Notes

1. For a more detailed account see Oakley (in press).
2. Although this is a personal account, much of my work has been collaborative; the journey described in this paper has been a collective one.

References


