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GENDER, METHODOLOGY AND PEOPLE'S WAYS OF KNOWING: SOME PROBLEMS WITH FEMINISM AND THE PARADIGM DEBATE IN SOCIAL SCIENCE

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Abstract This paper examines the character of the debate about 'quantitative' and 'qualitative' methods in feminist social science. The 'paradigm argument' has been central to feminist social science methodology; the feminist case against 'malestream' methods and in favour of qualitative methods has paralleled other methodological arguments within social science against the unthinking adoption by social science of a natural science model of inquiry. The paper argues in favour of rehabilitating quantitative methods and integrating a range of methods in the task of creating an emancipatory social science. It draws on the history of social and natural science, suggesting that a social and historical understanding of ways of knowing gives us the problem not of gender and methodology, but of the gendering of methodology as itself a social construction.

Key words: Feminism, gender, methodology, research.

Since the 1960s, feminist[1] social scientists[2] have had a great deal to say about methodology. The case that has been mounted against mainstream/'malestream' social research has been an important part of the project of women's studies; and within this, the dualism of 'quantitative' and 'qualitative' methods has played a central role. The use of 'qualitative' research methods has been aligned with a feminist perspective, while 'quantitative' methods have been seen as implicitly or explicitly defensive of the (masculinist) status quo. The feminist critique has joined other modern approaches to knowledge which dispute the appropriateness of a 'natural science model' to the social sciences (Bryman 1988; Hammersley 1989; Phillips 1992).

This paper takes a critical look at what has been called a 'paradigm dialog' (Guba 1990) – although the character of the discussion is often more oppositional than this phrase would suggest. The paper argues that the debate about research methods is more than a dialogue/argument concerning the best research technique to use in which circumstances; it offers a narrative which is about the relations between the social and scientific division of labour, the cultural production of masculinities and femininities, and the processes used to establish an understanding of the social and material world. Seen from this viewpoint, *methodology is itself gendered*; and one of the chief functions of the quantitative/qualitative dichotomy is as an *ideological representation*. The paper

suggests that, in order to understand the relationship between methodology and gender, we need to consider the intellectual and social origins of both social and natural science, and the history of the relationship between them. This history indicates that the 'feminist methodological case' has been made largely in ignorance of the way in which different approaches to knowledge have historically been sited within social and natural science and have been used by social reformers, including feminists. One explanation of the feminist dispute with the value of quantitative methods and the claim to own qualitative ones is that this methodological position contributes an important part of the 'professionalisation project' of feminist social science. The paper ends by suggesting that maintaining the division between 'quantitative' and 'qualitative' methods and the feminist case against quantification is ultimately unhelpful to the goal of an emancipatory social science.

The first section of the paper examines in more detail the nature of the feminist criticism of quantitative methods.[3] Secondly, some issues to do with truth-claims in qualitative work are briefly considered. This is followed by a historical outline of how the paradigm argument is located in the historical identities of both natural and social science. Lastly, an argument is put for dissolving the dualism of quantitative and qualitative methods and adopting a feminist empiricist approach which is more likely to promote policy-relevant research 'for' women. The focus of the paper is on feminist arguments about methodology as the main 'problematic', but the argument is relevant to methodological debates more generally, because embedded in the feminist 'case' against quantitative methodologies are other epistemological positions contending the superiority of 'qualitative' methods.

Hearing the Silent

When academia was first challenged by feminism in the late 1960s and early 1970s, and the biases of 'masculine' knowledge and women's invisibility were revealed, the argument quickly developed that 'positivist, quantitative research methodology' (Mies 1983:120) cannot be used uncritically to further the political goals of academic women's studies, because the voices of women as an oppressed social group are unlikely to be heard using such an approach. Accordingly, the early feminist methodology texts all celebrated qualitative methods as best suited to the project of hearing women's accounts of their experiences (see, for example, Bowles and Duelli-Klein 1983; Roberts 1981; Stanley and Wise 1983).

'Qualitative' methods include participant observation, unstructured/semi-structured interviewing, (some) life history methods and focus groups. These came to be seen as epistemologically distinct from the 'quantitative' methods of surveys, experiments, statistical records, structured observations and

content analysis, although, in practice, the feminist critique mainly equated qualitative methods with indepth face-to-face interviews (Stanley and Wise 1993:3), and quantitative methods with enumeration in some form or other, and with the epistemological/philosophical position underlying the use of statistical techniques (Mies 1991:67). As Bryman (1988) has noted of the methodological literature generally, quantitative and qualitative methods tend to be portrayed as mutually antagonistic ideal types, and even as representing two different 'paradigms' of social science itself. The contemporary opposition dates from the 1960s, when Glaser and Strauss's *The Discovery of Grounded Theory* (1967) was followed by a spate of books on qualitative methods (see, for example, Fletcher 1974; Lofland 1971; Schatzman and Strauss 1973).

While the 'paradigm argument' pre-dated the feminist critique of methodology, the arrival of feminist scholarship introduced new themes. It became clear that the dualism of quantitative and qualitative is paralleled by others: hard/soft; masculine/feminine; public/private; rational/intuitive; intellect/feeling; scientific/artistic; social/natural; control/understanding; experiment/observation; objective/subjective; separation/fusion; repression/expression; autonomy/dependence; voice/silence (Belenky *et al.* 1986; Gilligan 1982; Millman and Kanter 1987; Reinharz 1984). The dualism of 'quantitative' and 'qualitative' methods became inextricably bound up with the central contentions of women's studies – that traditional social science ignores or marginalises women, that all the major social theories explain the public world of labour but not the private world of work and the home (Elshtain 1981; Stacey 1981), and that the areas of social life which have particularly concerned women – caring, bodies, emotions (Rose 1994; Martin 1987; Williams and Bendelow 1996) – have hardly been part of the sociological landscape at all.

The feminist critique contests quantitative research on a number of grounds: that the choice of topics often implicitly supports sexist values; female subjects are excluded or marginalised; relations between researcher and researched are intrinsically exploitative; the resulting data are superficial and overgeneralised; and quantitative research is generally not used to overcome social problems (Jayaratne and Stewart 1991:86; Jayaratne 1983:145–6). Significantly, such criticisms of quantitative research overlap with general feminist critiques of mainstream/'malestream' social research (see, for example, Eichler 1988). 'Normal' social science research has been equated by feminist critics with 'rape': 'Research is frequently conducted on rape model: the researchers take, hit and run' (Reinharz 1984:95).[4]

The Three Ps

Underlying the various arguments in the feminist case against quantitative methods are three fundamental objections: the case against *positivism*; the case

against *power*; and the case against *p values*, or against the use of statistical techniques as a means of establishing the validity of research findings.

(a) The reality of positivism

Positivism is an approach to knowledge which sees material and social worlds as equivalent, and which limits knowledge to 'facts' knowable through human experience (Kolakowski 1972; see Bryant 1985). A positivist social science is primarily a search for social 'facts' and for social laws which will predict behaviour. The adequacy of a scientific theory within positivism is guaranteed by its 'objectivity' or lack of bias; following rules of valid inference means that knowledge claims should be verifiable by anyone. Removal from the research process of researchers' own values and experiences is essential to this requirement of verifiability (Jaggar 1983). A basic precept is the 'subject/object' dichotomy: what is studied is an 'object', which the knower/researcher can look at in a value-free and neutral way. All these constructions are problematic within feminist and other modern critiques of knowledge. For this reason, positivism has effectively become a 'term of abuse' (Giddens 1978:237). In the most extreme versions of feminist anti-positivism, the difference between 'fact' and 'fiction' disappears completely, and 'truth' and 'objectivity' are regarded as synonymous with 'lies' and 'subjectivity' (Stanley and Wise 1993:171). 'Objectivity' is reframed as 'male subjectivity' (Caplan 1988).

Supporting the rationale for the rejection of positivism is the association between it and 'the scientific method' which is condemned on political grounds. Science props up 'the fantasy of a calculable universe where everything operates according to quantifiable laws', a fantasy which is 'central to the calculating project of bourgeois democracy and the strategies of global domination' (Walkerdine 1989:208).

(b) The power of knowing

When social science imitates natural science, the resulting delusion is that the barriers between researcher and researched keep researchers 'safe from involvement or risk' (Reinharz 1984:368). The idea of a social world to be known about implies a knower; the knower is the expert, and the known are the objects of someone else's knowledge, not, most importantly, of their own. But feminist knowers must reject 'any mode of explanation which requires or sanctions the imposition upon the female subject of the theorist's own views as to who she is, what she wants, and what she should have'. This is because, 'One cannot survey the human race from a great and learned distance, proclaiming loudly that one has found the truth for the good of all the nameless, faceless abstractions one has never really bothered to take seriously' (Elshtain 1981:303). The hierarchical situation – the position of the researcher as expert knower – invalidates any data that come out of the

research process (Mies 1983:123). Just as hierarchy produces data that are by definition *invalid*, so it is contended that where non-hierarchical relationships between researcher and researched exist, the resulting data are intrinsically *more valid* (Acker, Barry and Esseveld 1991:146).

There may be practical reasons why hierarchical research methods do not work, for example because research participants treated in this way respond to their objectification by not trusting researchers, and therefore by lying to them (Edwards 1990). But this is not the essential objection. The essential objection is that the unequal power relationship between the knower and the known conflicts with the *moral obligation* at the heart of feminism to treat other women as you would yourself wish to be treated, and in this sense is seen to be at odds with feminism's emancipatory ideal. The notion of 'objectivity' calls up the ideological screen of 'objectification' and the adjective 'objectionable'. It has been argued that objectivity in research methodology is nothing more or less than an excuse for the same sort of obscene power relationship which leads women to be sexually assaulted and murdered (Stanley and Wise 1983:169).

(c) The sin of number-crunching: p values and all that

The tendency of positivism towards enumeration and the use of statistical techniques has not helped its credibility to the feminist cause, or, to the whole postmodern project of conceiving knowledge in essentially relativist terms. As Silverman says in his *Qualitative Methodology and Sociology*, 'Since the 1960s, a story has got about that no good sociologist should dirty his [sic] hands with numbers' (Silverman 1985:138). While the feminist literature accepts that some numbers can be used in qualitative studies for 'directional orientation', it is argued that statistical formulae and techniques can only serve to obscure qualitative meaning, and therefore are not part of the practices of a feminist social science (Leininger 1994:103). Here we have the famous dispute between 'number-crunchers' and 'navel-gazers' (Swanson and Chapman 1994:89). Feminist qualitative researchers argue that the *p*s they are interested in do not concern the probabilistic logic of statistical *p* values, but the value of *people*, and this can only be deduced by constructing a qualitative knowledge about them (Sidell 1993:107).[5]

The use of numbers is seen by many feminists as a 'machismo' element closely tied to the habit of manipulating variables and thus creating artificially 'controlled' realities (Bernard 1973) in an 'unpleasantly exaggerated masculine style of control' (Millman and Kanter 1987:35). 'Patriarchal' measurements are 'artificial' and 'abstract' (Griffin 1980:107); the urge to predict and control as the underlying drive of the quantitative method is ideologically linked with men's desire to dominate, to exert power over people as well as nature, in other words it is a veritable 'exercise in masculinity' (McCormack 1981:3).

Experimenting on Women?

The apex of the feminist critique of quantitative methods is reached in relation to experimental studies. The core of natural science, and therefore the essential reason for its rejection by advocates of a humane *social* science, is held to lie in the experimental method (Du Bois 1983:107).[6] Feminist objections to experimental work can be read from silences, and from the forms of language used to discuss quantitative methods, as well as from what is specifically said. 'Experiment' is not a word which occurs in the index of most of the key texts (see, for example, Bowles and Duelli Klein 1983; Roberts 1981; Stanley and Wise 1993). The chapter on feminist experimental research in Shulamit Reinharz's *Feminist Methods in Social Research* (1992) begins with the impossibility of hearing feminist 'voices' in experimental research; like others, Reinharz deems the 'rigid control' and 'manipulation' of variables demanded by experiments as a 'male' and 'agentic' style of research. In her *What Can She Know? Feminist Theory and the Construction of Knowledge*, Lorraine Code puts another common argument against the experimental method, that it 'produces explanations of personality and of social structures that take into account neither the consciousness of the subjects studied nor the meanings, and interpretations of their experiences for these subjects' (Code 1991:34). Like science and social science, the experimental method is also condemned on political grounds, as an ideological form rooted in the material conditions of patriarchal domination (see, for example Rose 1994). The most explicit discussion of experimentation occurs in the context of work on feminist science, where it is argued that 'the scientific or experimental method' converts reality into 'mathematical entities modelled on the physical universe'; and all bodies, both human and animals, are seen as machines (Donovan 1990:361). The constant slippage in such texts between the terms 'scientific' 'experimental' and 'mathematical' is significant, suggesting that what is at issue here is an essentially *political* objection to what it is these terms are thought to signify, rather than the usefulness in practice of different research approaches to the feminist project.

The feminist case against quantification produces a seemingly unbridgeable chasm between different ways of knowing; there is a contradiction within the phrase 'feminist social science', because a science, if it is *feminist*, cannot be *scientific*: science is the enemy (Stanley and Wise 1993:6). At this point, three questions suggest themselves. Firstly, there is the satisfactoriness or otherwise of what we might term 'the feminist solution' to the problem of quantification/experimentation in social science research. What are the implications of the hegemony of qualitative methods for what we are consequently enabled to know about women, and about the social world more generally? Leading on from this, the second question is why feminist social scientists have so strongly defended 'qualitative' methods against any counter-evidence that other ways of knowing may be important to, and for, women. Thirdly, there is the

historical question: where did the dichotomised relationship of 'quantitative' and 'qualitative' come from in the first place?

The 'Qualitative' Solution

In so far as there are distinctly feminist answers to the problems of 'quantitative' methods, these lie mainly in the direction of acknowledging the authenticity of multiple viewpoints, the role of values, and the subjectivities of both researcher and researched (Du Bois 1983). This position produces feminist research as research advocating 'an integrative, trans-disciplinary approach to knowledge which grounds theory contextually in the concrete reality of women's everyday lives' (Stacey 1988:212). The resulting solutions include such devices as the theory of *passionate scholarship* – a heretical challenge to the objectification of experience (Du Bois 1983); and the postulate of *conscious partiality* as a substitute for the rule of value-neutral research (Mies 1983). More sustained versions of this argument develop a case for using sociology in an emancipatory way, to further the political goals of feminism. The best-known statement of the difference between a sociology 'of' and a sociology 'for' women is that of the Canadian sociologist Dorothy Smith. Smith suggests the notion of a feminist 'standpoint' as a place outside the dominant frame of organized social science knowledge from which it is possible to construct a sociology respectful of women's subjectivity. Everything begins with everyday life; all concrete experience, and all abstract knowledge. In the case of women, situated as they are in a nexus of domestic labour and emotional work for others, the resultant 'thinking from caring' produces different versions of both social and natural science from the ones that have dominated most intellectual discourse and knowledge-production (Rose 1994). Smith recommends something she calls 'institutional ethnography' as the core of a feminist methodology. Such an ethnography means a commitment to investigating and explaining 'actual practices and relations' (Smith 1988:160). The use of research methods based on 'indepth' interviewing with selected samples of women, a meticulous, iterative attention to the details of what women say, and forms of analysis dedicated to reproducing all of this as 'faithfully' as possible, construct an alternative feminist scholarship in which the enemy of 'the scientific method' appears to lack even a foot in the door.

However, feminists' use of qualitative methods reveals problems in relation both to the validity of truth-claims and conflict with feminist values. For example, the use of women-only samples can give us exactly the same problem as men-only samples, raising questions about, for example, claims to have established authentically female ways of knowing (Belenky *et al.* 1986; see Crawford 1989) or thinking about moral choice (Gilligan 1982), and ignoring the tendency for sex differences to disappear when direct comparisons are

made between samples of men and women (Walker 1984); methods such as interviewing do not necessarily rid the research situation of hierarchy (Ribbens 1989; Finch 1984; Wise 1987; Edwards 1990; Graham 1984; McKee and O'Brien 1983); appeal to such devices as sharing book royalties with informants (Wolf 1996:25) and discussing data analysis with them (Stacey 1988) may lead researchers away from 'truth-seeking' as a legitimate goal of inquiry. Many of these issues are shared with qualitative research more generally. The 'qualitative' researcher asked to explain the methods used to derive findings from data has been compared to a centipede which is paralysed when asked to think about how it moves all its legs (Sandelowski 1994). Validity and 'bias' are general problems. Although the aims of qualitative research theoretically exclude generalisability, the insights gained are likely to reflect the social world of research participants, so that bias in qualitative research is just as possible as in the quantitative sort, and possibly more so, given the smaller sample sizes (Jayaratne and Stewart 1991), the emphasis on the need for homogeneous samples (Cannon, Higginbotham and Leung 1991), and the absence of 'visible research standards' (Sprague and Zimmerman 1989:73). There are particular problems which can lead to false inferences being made with the lower participation rates in much qualitative research of people from working-class and ethnic minority backgrounds (Cannon, Higginbotham and Leung 1991).

While quantification is a strategy which emphasises explicitness in each stage of research (measurement techniques, data, evaluation processes), there is no accepted model for 'good' qualitative research with agreed criteria for evaluating its truth content (Griffin and Ragin 1994; Jayaratne and Stewart 1991). Many qualitative researchers do not provide adequate and clear justifications for their methods, findings or conclusions (Howe and Eisenhart 1990); indeed, the 'almost mystical advocacy of the virtues of qualitative ethnology' (Shadish *et al.* 1991:397) leads sometimes to missing information about how research was *actually* done.

Some researchers argue that establishing the validity of truth claims made for qualitative data is an activity so different from the parallel exercise with quantitative data that it is unreasonable to think of using the same criteria (Leininger 1994; Muecke 1994). There is very little systematic training in how to *do* qualitative research (Richardson 1996). Those who claim that qualitative techniques protect from research bias ignore the selection process inherent in the collection and reporting of qualitative data which by no means saves it from 'the intrusion of the researcher's values' (Sprague and Zimmerman 1989:74). As Marsh has pointed out, problems of valid data and valid inferences from data run through all social research and 'you do not escape the difficulties by pretending that you can extract unproblematic information yourself in a pub over a pint of beer' (Marsh 1984:96). Reactivity – the influence of researchers on their data – is a problem to be faced in all types of research (Bryman 1988). Even when quoted material is allowed 'to speak for

itself', it is not clear that the unmediated views of research participants provide the best way to understand the social world. A preference for 'multi-level multi-method' studies using triangulated evidence from a variety of sources 'which would be sufficiently convincing for a jury to commit themselves to action' can be a good deal more complicated than it sounds. Triangulating data means mixing interpretations and these may be at odds with one another. Moyra Sidell (1993) gives an example of this in a study of the health status of older women. She used three levels of data ranging from 'very hard' (national statistics on mortality and morbidity) through 'medium' (two large sample surveys) to 'very soft' (biographical interviews with thirty older women). The result was 'a mass of paradox and downright contradictory evidence' (Sidell 1993:111–12). It is not simply a question of checking one dataset against another; different 'types' of data may yield very different conclusions.

The feminist critique puts a new emphasis on the extent to which different methodologies imply different forms of power relations, but much of what is argued is otherwise not decisively new. Similar issues have occupied a central place in many 'isms' and 'ologies', for example poststructuralism, ethnomethodology, phenomenology, postmodernism and hermeneutics (Hammersley 1995). All of these, like feminism, criticise the idea of an objective social science as ignoring the constitution of the social world by agents whose meanings and intentions are essential to our understanding of it. In Britain, sociologists showed more interest in qualitative research from the late 1960s on; the Chicago school was more advanced in this regard in its practices, if not in its theories, during the 1920s and 1930s. Cicourel talked about 'quantofrenia' in 1964 (Desrosieres 1991). Glaser and Strauss's critique of verification theory (1967) is very like the later feminist case. Foucault, Habermas and Heidegger all comment on the dissolution of the subject/object distinction, and the need for a more reflexive, socially-located approach to knowledge.

The Problem of Dichotomy: Weakness or Strength?

Many of the supposed differences between qualitative and quantitative ways of knowing are not a matter of a hard-and-fast distinction, but of a continuum, with points on it where one would find it difficult to say which method was in the ascendant. For example, either 'quantitative' or 'qualitative' research may include the development and progressive (or unprogressive) testing of theory, or it may not. Analysing data as numerical values – seen as an aspect of quantitative research – often requires qualitative judgements about data and the construction of categories for statistical testing. In just the same way, there is a false synonymity between being systematic and being quantitative; one may be an unsystematic quantitative researcher, or a systematic qualitative one, or vice versa, in each case (Jayaratne and Stewart 1991). 'Qualitative'

data can be analysed systematically or formally (Griffin and Ragin 1994). In practice, few such analyses proceed without relying on the occurrence of a theme to derive an interpretation of meaning, and the analysis of open-ended interviews by counting the number of times particular themes occur 'can hardly be called qualitative, especially if the themes are assigned a significance based on the frequency of occurrence' (Dreher 1994:284). As Bryman has argued: 'a good deal of qualitative research shares an empiricist streak with quantitative research; much quantitative research shares a concern for subjects' interpretations, which is supposedly the province of the qualitative researcher' (Bryman 1988:172–3). Many research studies collect both 'objective' and 'subjective' data, and the transformation of such issues into 'dichotomous choices is unnecessary, inaccurate and ultimately counterproductive' (LeCompte and Goetz 1982:54).

Although there are some signs of a new recognition within feminist social science of the usefulness of non-qualitative methods (see, for example, Gorelick 1991; Kelly, Regan and Burton 1992; Oakley 1992b), both feminist methodology and feminist epistemology remain strongly founded on qualitative methods. Applying Kuhn's arguments about cultures of 'normal science', it could be suggested that feminism holds onto qualitative methodology because this has become part of its normal intellectual repertoire. To be a feminist social scientist one must have a certain allegiance to the qualitative paradigm, and a willingness to go along with the habit of dualistic either/or thinking (even if this is, at the same time, the enemy of both an emancipatory social science and an emancipatory social order). Thus, feminists who use quantitative methods may be expected to apologise for so doing (Graham and Rawlings, quoted in Reinharz 1992:87). Drawing on the sociology of professions and professionalisation literature, it can also be argued that the claims of feminist social science preferentially to own the qualitative method are part of its own professionalising agenda (Freidson 1970; Witz 1992). Feminism needed a research method, a distinct methodology, in order to occupy a distinctive place in the academy and acquire social status and moral legitimacy. Opposition to 'traditional' research methods as much as innovation of alternative ones thus provided an organising platform for feminist scholarship. This opposition, and the whole contention of positivism and realism as inherently anti-feminist, were reinforced when postmodernism entered the feminist critique in the 1980s. As Wolf (1996:6) has commented, it is probably no accident that the 'often inaccessible, abstract and hypertheoretical language' of postmodernism gained ascendancy at the same time as women increased their representation within academia.

What is Science?

Situating feminist methodologies within their social context may provide a

partial explanation as to their often pronounced anti-quantitative and anti-positivist stance. But there are more fundamental questions about why social science methodology appears to be trapped in an ideological combat between quantitative and qualitative methods. These questions invoke other issues to do with the nature of science, and the relationship between it and social science. Attending to these issues suggests that the *idea* of science was what threw up *social* science as a response; and the feminist critique was, in turn, formulated partly in relation to an *idea* of social science. The methodological debate, the conflict between the two models of the 'quantitative' and the 'qualitative', is then reduced to the dialectic between a series of straw men and women; unrealistically seen, and fictionally counterposed, in an endless loop.

The idea of an 'objective' reality goes back a long way, at least as far back as the divorce of philosophy from science in the eighteenth century, to the radical empiricism of philosopher David Hume and the Kantian proposition that whatever lies beyond experience cannot properly be called science. What we now call science developed out of an ancient tradition of rational enquiry in which reason, experience, theory and argument were conjoined in an effort to know God, the cosmos, and the social and psychological world of human beings. Throughout the seventeenth and eighteenth centuries the term 'natural philosophy' embraced the sciences of mathematics and astronomy. The terms 'philosophy', 'arts' and 'science' were used more-or-less interchangeably. Following the work of Newton in the seventeenth and early eighteenth centuries, a fissure developed within philosophy around the use of the experimental method to study the relationships between externally existing phenomena. This established 'experimental philosophy' as a subdivision of 'natural philosophy'. But it is not until well into the nineteenth century that the term 'scientist' comes to be used in anything like the modern sense, and not until the early years of this century is a scientific method identifiable with the testing of hypotheses clearly visible (Manicas 1987). By the time we get to the 1900s, explicit images of science feature the popular modern notion of the scientist as an efficient man working in a modern laboratory and usually wearing a white coat (Schiebinger 1989:148).

Among the ideas about science that continue to inform social science's ways of thinking and knowing, despite having been long abandoned by physical science are: the verifiability theory of meaning; the analytic/synthetic distinction; the notion of reductionism; the idea that 'laws of nature' can be found and analysed; Humean causality; and the status of scientific theories as axiomatic systems (Manicas 1987). Popular conceptions of science portray scientists as 'reasonable men' searching for causal laws with the goal of predicting and controlling nature, and doing so themselves almost like machines, without reference to values or to their own experience. The scientist 'himself' is context-free, and science itself has a linear and evolutionary shape, according to which its knowledge gets better and better all the time. This idea of science as open-minded, objective practice was mortally wounded by

Kuhn's *The Structure of Scientific Revolutions*, in which the brilliant scientific mind emerged as 'a type of concentration that is loose, intuitive, a bit frivolous, if not wayward' (McCormack 1981:2), and the actual practices of science were revealed as things that depend on experiences, on feelings and intuition and on the operation of ordinary human events. Science is a socially embedded activity, and the most creative theories are imaginative visions produced within the specific cultural contexts in which scientists work (Gould 1981).

Other more fundamental ways in which science is gendered operate at the level of metaphors and representations of private relations in public lives. The idea of an organic female universe prevailed in the Middle Ages and the Renaissance. Human and physical realms were regarded as continuous with one another; neither objectivity nor an autonomous self-identity were conceived as goals (Merchant 1980). As nature was female, so was science/knowledge; the two domains overlapped. This feminine gendering gave rise to the modern lexicon of the knower as masculine. Descartes, whose project separating the knower from the known is one of the most-quoted events in the entire history of science, envisaged the aim of science as domination of the feminine natural universe. Susan Bordo (1986) has suggested that what can be read into Descartes's *Meditations* is an ontological male anxiety about separation, not only from the mother, but much more fundamentally from the organic female universe of the Middle Ages and the Renaissance. The food crises, wars, plagues and poverty of the mid-sixteenth to mid-seventeenth centuries disturbed the sense of equilibrium between human beings and the female universe of nature; nature seemed to be letting people down. Because nature was female, femininity was itself implicated in this sense of ontological disappointment. The cultural and scientific world, being reborn as masculine, gave not only women but men the problem of having to give up an identification with the feminine. A defensive rejection of women followed, logically. Significantly for the modern problem of feminism and the experimental method, advocates of this from the sixteenth century on saw experimenters as men actively dismembering nature in the form of human or animal bodies. By the end of the nineteenth century, experimental physiology focused on the laboratory had become the pre-eminent science, and observational knowledge was established as the 'handmaiden' serving the 'master discourse' of an interventionist experimentalist science (Rose n.d.:6).

When the experimental method of inquiry became established as a subdivision of philosophy sometime in the seventeenth century, it was associated with practices such as alchemy, with connotations of mystery and secrecy (Porter 1995). Bacon called new experimental knowledge 'masculine' (Rose n.d.:11); this was a term of approbation, while to call something 'feminine' was an insult (Schiebinger 1993). Metaphors of 'rape' and torture were used to describe the extraction from nature of 'her' secrets (Merchant 1980). An alternative name for 'Newton's mechanics' is 'Newton's rape

manual' because understanding nature as a woman indifferent to or even welcoming rape was fundamental to these new conceptions of nature and inquiry (Harding 1986:113).[7] In these senses, the 'scientific' way of knowing can simply be renamed a 'super-masculinisation' of rational knowledge (Harding 1981). The reason why science reproduces masculine rationality is because it is the essential expression of it (Haste 1993:229; see also Keller 1990).

The Birth of Social Science

The term 'social science' was first used in France in the 1790s; the Institut de France, founded in 1795, had social science as its 'second' class, with natural science and fine arts as its first and third respectively (Wittrock, Heilbron and Magnusson forthcoming). Between 1750 and 1850 many of the central assumptions, concepts and terms of social science were shaped, though its *institutionalisation* came later. This took different forms in different countries. Crucial factors were the way in which 'the social question' was framed, and the varying relationship of the state to this; the fate of the natural sciences themselves; the development of higher education systems and particularly the rise of research-oriented universities; and the creation of welfare systems leading to transformations in social policy (Manicas 1987; Wittrock and Wagner 1996).

Underlying the emergence of social science was a process in which the natural science model of inquiry centred on induction and experimentation became the model for all human inquiry; 'rational' discourse was confined to statements which were either true by definition or empirically verifiable. The Vienna Circle, a group of scientists, philosophers and mathematicians who worked together in the 1920s, coined the term 'logical positivism' (Neurath 1973), but it was one of the founding fathers of social science, Auguste Comte, who was responsible for the parent term here. Comte saw all knowledge systems as passing through distinct stages, beginning with the theological 'or fictitious' – a stage in which the causes of all phenomena are assumed to be supernatural – and ending with the positivist, when people give up their grandiose untestable ideas and restrict themselves to charting the laws of the relations between observable events and things (Comte 1875; Lepenies 1988:38).

In John Stuart Mill's *A System of Logic*, the analogy for a predictive science of social affairs is the challenge of predicting the tides: while this is theoretically possible, there are many practical difficulties, such as knowing the slope of beaches, the strength of winds, and so forth. Similarly, while many difficulties lie in the path of a predictive social science, such a body of knowledge is feasible in theory. The separation of social from natural science involved the rejection of two linked premises: that of the experimental method, and that of

biological reductionism. Mill rejected the experiment as espoused by Bacon and his followers as one of two approaches inappropriate for social science; the other is the 'abstract geometrical procedure' (Lepenies 1988:105). In the first half of the twentieth century, the hostility of people who called themselves sociologists to biological theories of society achieved for sociology an autonomous existence as a discipline. Its intellectual development, especially in Britain, was largely a by-product of ideological reaction against social Darwinism and eugenic ideas (Halsey 1967).

In establishing itself as a separate discipline, sociology was itself caught between two paradigms: that of natural science, and that of fiction in the form of the 'social reality' novel (Bruner 1966; Nisbet 1976). Many of the early social scientists worked in both natural and social science and translated methods between the two (McDonald 1993). Natural science was as much helped by developments in social science as the other way around (Cohen 1994).

Both the rise of social science, and the social survey as the primary method for finding out about society, can be seen as responses to the social disorder and change introduced by industrial capitalism. People, particularly men, wanted to understand what was happening in this new society of theirs; they needed to explain and understand the dislocating effect of capitalist production on work, and on the family and old, settled ideas of community not organised by the discipline of atomised time that ruled the factory (Abrams 1968; Graham 1983; Manicas 1987). In all European societies by the beginning of the twentieth century, groups of intellectuals were voicing the need for an empirically-based social research to illuminate the nature of social transformations and provide data on which concrete social policies and political strategies could be based (Wittrock, Wagner and Wollman 1991). The survey method was born as a way of knowing, not only about the local community, but about the nation and the state.

Learning to Count

The term 'statistics' came from eighteenth century Germany, where its referent was the science of the description of the state. This was at first literary, and then numerary; by the nineteenth century, only the numerary referent survived. Modern statistics was born in the 1890s with the work of the eugenists Francis Galton and Karl Pearson. In the twentieth century statistics has come to mean mathematical procedures for analysing data: the denominator of the state has dropped out of the equation (Desrosières 1991; Wahl 1996). An important strand in this history is that of mathematics as the carrier of the main burden of scientific reasoning and the core of the major theories of physical science (Kline 1972). The growth of abstract numerical calculations is a feature of an increasingly complex society; numbers can serve

as a technology of moral and social distance. It has been suggested that this is one reason why surveys became the preferred vehicle for studying the insane, the unemployed, factory workers, prostitutes and cholera victims (Porter 1995).

The logic of quantification as a strategy for imposing distance can most clearly be seen in the example of medical technology, where the growth of new mechanical and pharmacological ways of knowing what is going on inside human bodies neatly bypassed the need for people themselves to tell doctors what they thought was going on. Interestingly, it was not the initiative of doctors which introduced quantitative technology into medicine, but the drive to state regulation; for nineteenth-century doctors, the new technology of stethoscopes for listening to patients' hearts was just about acceptable, because only the physician could hear the sound the stethoscope produced – the results were not available as 'objective' data for anyone else to hear or challenge (Reiser 1978). Nineteenth-century doctors also resisted the rise of statistical description, maintaining a case that looks very like the later feminist one – that each patient is an individual, whose situation and history can only be satisfactorily probed in a face-to-face interview (Desrosières 1991).

The Dangers of Simple Histories

We can say, then, that the history of methodology in science, including social science, is more complex than the paradigm argument within, and outside, feminism would suggest. Continued adherence to the idea of opposed methodological paradigms depends on an oversimplified view of the positivism implied by quantitative methods; but the truth about positivism, as the above brief history indicates, is 'less straightforward than the fantasy' (Phillips 1992:96). Sociology did not mistakenly imitate the positivism of the natural sciences, and then discover its mistake, a mistake which is particularly acute with respect to the representation in what counts as knowledge of the experiences of oppressed social groups. As Jordanova (1989:162) has put it, 'It is all too easy to see the tangles around gender, science and medicine as simply evidence of sexism . . . such a judgement is both simplistic and facile'. The origins of positivism lay in the abandonment of metaphysics as a way of establishing knowledge about the world, and an alternative insistence on the importance of knowing through experience (Kolakowski 1972). The dismissal of the role of values was similarly intended to emphasize the importance of separating out valid from invalid knowledge (Giddens 1978). While at each point in their history, ways of knowing have been anchored in the social context and political concerns of the time, we cannot abstract from this a straightforward picture of two communities of scientists – the social and the natural – developing methods of study which bear no relation to one another. Nor is it clearly the case that 'quantitative' methods have served no relevant

feminist goal. Historical innovations in empiricist methodology such as the social survey were made primarily by people, including women, who sought policy-relevant knowledge as ammunition for social reform. Reformers such as Jane Addams, Harriet Martineau, Florence Nightingale and Beatrice Webb carried out social investigations which served the reformist cause by revealing the extent of poverty and inequality (McDonald 1993). Feminist social reformers advocated the need for statistics to demonstrate the conditions of women's lives. For example, the astronomer Maria Mitchell in 1875 urged the collection of statistics to describe the situation of women scientists in the United States. In the campaign against women's exclusion from higher education, statistics were used to disprove the masculinist medical notion that education damaged women's health (Reinharz 1992). There was a feminist tradition of defending women from the 'science' of craniometry, the first biological theory to be 'supported' by quantitative data. Craniometrists found that women generally had smaller brains than men, and contended that in this regard they belonged to the same category as Negroes, apes and children. Those who advocated and practised craniometry regarded themselves as 'servants of their numbers, apostles of objectivity' (Mastroianni, Faden and Federman 1994:112). The painstaking gathering of statistics was even seen as a uniquely feminine capacity (Reinharz 1992). Significantly, the work of feminist social reformers often combined the numerical approach of the social survey with other ways of knowing; for example, Beatrice Webb became a participant observer by getting a job in tailoring to help her find out how the industry functioned (McDonald 1993:277); Charles Booth, Webb's cousin, lived in working-class lodgings in the East End of London to collect data for his poverty survey, widely hailed as the first example of such an investigation, and carried out to win an argument with a friend of Marx's that the extent of poverty was not as great as the socialists claimed it was.[8]

The history of the ways the experimental method has been used in psychology and health care is, unfortunately, less benign than this story about 'quantitative' methods (Coney 1988; Mastroianni, Faden and Federman 1994; Oakley 1992a; Sherwin 1992). But there is a distinguished and relevant history of sociologists' use of the experimental method to evaluate many different types of social intervention, a history which demonstrates both that the method is feasible and can be used in ethical ways, and that it can be a valuable approach to answering questions about effectiveness. However, this is a story which deserves separate attention (Oakley 1997a).

Quantification for Women?

Today, most feminist critics of quantitative methodology do ultimately concede that there *is* a social reality which has an objective existence beyond people's competing interpretations of it (Stanley and Wise 1993:9). We cannot

end up with a phenomenological morass, either as feminists or as social scientists. The danger of rooting knowledge in the description of individual experiences is that one never moves beyond them. The grounding of research questions and findings in women's experiences of everyday life is a laudatory feminist aim; and an essential aid to a comprehensive understanding of the social world. But the subjectivity of the researcher remains, as in all science, a potential influence on the knowledge-claims that are made.

Feminism's interest in an emancipatory social science suggests a need for a range of methods within which 'quantitative' methods would have an accepted and respected place. The extensive socio-demographic mapping of women's position that underscored second-wave feminism would not have been possible without large-scale quantitative surveys. Women's oppression could neither have been demonstrated nor understood without an opportunity to examine their relative positions *vis-à-vis* men in the labour market, the education, health and welfare systems, political organisations and government, and the private world of the home and domestic relations. The underlying gendering of structural inequalities that occurs in most societies could not be discerned using qualitative methods on their own. Statistics derived from official records or from large-scale surveys continue to demonstrate the ways in which gender, class and ethnicity intersect as axes of discrimination (see, for example, Arber and Ginn 1991; Bagilhole 1994; Department of Health 1995; Dex 1987; Humphreys and Rubery 1995). A recent example of the use of survey data on women's position as a feminist consciousness-raising strategy is the development and work of the 'Support Stockings' in Sweden. The Support Stockings are a network of women engaged in supporting women in politics. The network was formed in 1991, when the percentage of women in the Swedish parliament fell for the first time since 1919, and a number of political and social developments suggested a move away from a gender-equal society. One of the Support Stockings' strategic tools was the use of gender-based statistics as a way of disseminating information about women's position (Stark 1997). Such examples are fuel for the rehabilitation of 'quantitative' methods within feminist social science research techniques, and for a development of these in the direction of a 'quantification for women', to borrow Dorothy Smith's (1988) phrase. Similarly, there is a strong case (which is not considered here) for developing experimental research methods so as to reflect feminist values and the goals of an emancipatory social science. In both cases what is likely to be required is adaptation of the 'malestream' models to suit feminist values; there is much work to be done collecting and integrating examples of 'good practice' and providing guidelines for future research.

The call for feminist models of quantitative and experimental work to be developed clearly recognises that 'the baby need not be thrown out with the bathwater', and that procedures used in 'malestream' research which are inconsistent with feminist values can be altered without abandoning the basic

methodological techniques themselves. The critical question remains the appropriateness of the method to the research question.

Conclusion

Feminist social science researchers since the 1970s have extended the anti-quantitative and anti-positivist methodological positions developed within such perspectives as ethnomethodology, poststructuralism and hermeneutics. Feminist scholarship has provided a passionately argued case for rejecting quantitative and experimental methods on the principled ground that these imply oppressive power relations. The prominence given within the feminist methodological literature to the importance of understanding what methods 'do' both to research participants and to research 'findings' has been very important in reconstituting knowledge-claims and in helping to develop a more democratic social science. The argument about masculinist bias hiding behind the lens of 'objectivity' has been especially crucial in highlighting how mainstream definitions of valid knowledge may unintentionally reflect the partial view of men as the dominant group.

This paper has argued that behind the feminist methodological 'case' lies a more fundamental and gendered history. The 'paradigm argument' in which qualitative and quantitative ways of knowing are seen as opposed, is a historical and social construction. What is at issue is a 'sterile' and 'false polarization' (Jayaratne and Stewart 1991) which crucially repeats the patriarchal character of many dichotomies. Consequently, and as Dreher (1994) has argued, in most social science research projects the terms 'quantitative' and 'qualitative' neither add insight nor credibility, and nothing would be lost by their omission. Furthermore, there would be gains, because, as this paper has argued, feminist social scientists who use the language of 'quantitative' and 'qualitative' paradigms participate in this construction of an unequally dichotomised social world. Dichotomy is neither a property of nature nor of human life and experiences (Du Bois 1983). The conception of an opposition between two elements that denies the existence of any middle ground between them could not have been derived empirically; it is, rather, itself a social construction informed by a basic requirement of patriarchal societies, which is the rule of a clear opposition between men and women (Jay 1981).

Following object relations theory, we can understand how male scientific rationality as an ideological representation is founded on the *actual* socialisation of men, which implies the repression of infantile longing for union and the distorted social relations resulting from this (Haraway 1989; Keller 1990). Within such a social framework, qualitative ways of knowing become 'the Other' – the 'second' sex of Simone de Beauvoir's classic analysis. As Shulamit Reinharz phrases it (1990:294): 'The quantitative is the Establish-

ment and the qualitative is the social movement protesting the Establishment. The quantitative is the regular army and the qualitative the resistance. The qualitative approach is the outside trying to get in . . .' The case against quantitative ways of knowing is based on a rejection of reason and science as masculine and an embracing of experience as feminine; but this is essentialist thinking which buys into the very paradox that it protests about (Grant 1987). The result is likely to be the construction of a 'difference' feminism where women are described as owning distinctive ways of thinking, knowing and feeling, and the danger is that these new moral characterisations will play into the hands of those who use gender as a means of discriminating against women (Oakley 1997b). The construction of 'quantitative' and 'qualitative' methods as opposed impedes critical thinking about developing and using ways of knowing capable of respecting the autonomy and subjectivity of the researched, at the same time as minimising bias, in creating an appropriate knowledge for women. The more we speak the language of 'the paradigm argument', the more we use history to hide behind; instead of looking forward to what an emancipatory (social) science could offer people's wellbeing, we lose ourselves in a socially constructed drama of gender, where the social relations of femininity and masculinity prescribe and proscribe, not only ways of knowing, but what it is that we do know.

Notes

1. I use the term 'feminist' to refer to those who see women as exploited, devalued and oppressed, who are committed to changing this, and who consequently adopt a critical perspective towards dominant intellectual traditions that have ignored or justified women's oppression (Acker, Barry and Esseveld 1991: 150–1).
2. The focus of the paper is methodology within sociology; other social sciences such as economics and psychology have somewhat different methodological traditions.
3. I am here trying to summarise a substantial literature written by feminist social scientists about research methods; this literature includes some of my own work (Oakley 1981). The journey taken in this paper from a defence of qualitative methods to a recognition that what feminism and social science both 'need' is a more integrated approach encapsulates an autobiographical path; there is no sense in which I see myself as outside 'the feminist critique'.
4. Metaphorical uses of the term 'rape' are problematic in that they can be seen as trivialising actual rape.
5. In such arguments, terms such as '*p* values' have a metaphorical rather than a precise meaning, referring both to technical procedures for analysing data and to the underlying epistemological tradition of regarding numbers as the, or a, principal means of representing knowledge.
6. The feminist methodological literature uses the term 'experiment' in two different ways, but often does not distinguish between these. In the first sense, experiment means what psychologists or scientists do in the laboratory; and, in the second, an experiment is something that is done in the real world, usually with an element of inbuilt evaluation which will determine whether or not the

experiment has been successful. In these kinds of experimental studies, the researcher not only collects data from the researched but actively intervenes in their lives. Experiments are thus 'invented experiences' (Silverman, W.A. 1985:6).

7. Harding later regretted this analogy (Nemecek 1997).
8. The socialist argument was that around a quarter of the population lived in poverty. Booth's survey put the figure at closer to one-third (Oberschall 1972:105).

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