

PART 1

THE ROLE OF THE RESEARCHER: LIMITS, OBLIGATIONS AND VIRTUES



1

METHODOLOGY, WHO NEEDS IT?¹

[...] sociology is the science with the greatest number of methods and the least results. (Poincaré 1908: 19–20)

Methodologists remind me of people who clean their glasses so thoroughly that they never have time to look through them. (Freud, cited in Sterba 1982: 120)

Methodology is too important to be left to the methodologists. (Becker 1970: 3)

The literature on social research methodology is now very large. Indeed, it may still be increasing at an increasing rate. It is so substantial that it is unlikely anyone could read all of it; or perhaps even keep up with the latest publications. In part, this growth in the literature results from the fact that, in the UK and elsewhere, substantial ‘training’ in methodology has become institutionalised in many postgraduate programmes, notably as a result of requirements laid down by research funding bodies. There has also been increased emphasis on ‘research capacity building’, aimed at improving the methodological knowledge and skills of practising researchers, and this has included the promotion and dissemination of ‘methodological innovation’ (see Travers 2009).

The sheer scale and growth of the methodological literature might be taken as a sign that social science is in robust health. But it is also possible to draw a very different conclusion: that there is an excessive preoccupation with methodology on the part of social scientists, perhaps amounting to a cancer on the face of research. Approximations to both these views can be found, suggesting that there is some ambivalence towards methodology among social scientists at the collective, and perhaps even at the individual, level. Attitudes no doubt vary according to researchers’ degree of involvement in this type of work, from those

¹My title echoes Howard Schwartz’s (2003) ‘Data: who needs it?’, though my concerns and arguments are different from his.

who call themselves methodologists and/or contribute substantially to the literature, through to those who do not write about it, believe that it is only of relevance to novice researchers, or perhaps even regard it as a major distraction or obstruction.

Ambivalence towards methodology has been evident for a long time. In the first decade of the twentieth century, Max Weber complained about a 'methodological pestilence' in German social science (quoted in Oakes 1975: 13), with researchers becoming preoccupied with epistemological issues; yet, at the same time, he himself produced a batch of highly influential methodological writings (Weber 1949; 1975; 1977). Around the middle of the twentieth century, when the importance of methodological training was beginning to be emphasised in US sociology, C. Wright Mills wrote a paper entitled 'On intellectual craftsmanship' that was later developed into an appendix to his book *The Sociological Imagination*, and was reprinted in various forms in other places. It became a classic methodological text for sociologists. Yet, in this text, Mills declares that much methodological discussion simply 'disturb[s] people who are at work', as well as leading to 'methodological inhibition' (Mills 1959a: 27). So, here we have a methodological text which warns of the dangers of methodology. Mills also complains about 'the fetishism of method and technique' (Mills 1959b: 224), and others have echoed this, referring to 'methodological narcissism' (Nisbet 1963: 148), the 'myth of methodology' (Kaplan 1964: 24) and 'methodolatry' (Gouldner 1965; Janesick 1994: 215).

In this chapter, I will begin with a very brief sketch of the methodological ideas that have shaped social science in the past 50 years, and then examine three genres to be found in the methodological literature today and the ambivalence towards methodology to which they have given rise. Towards the end of the chapter, I will consider the role that methodology *ought to* play in social research, reflecting on the value of each of the genres but also on how they can lead us astray.

A BRIEF HISTORY

There has not just been an increase in the amount of methodological literature over the past few decades, its content has also changed considerably; this varying, of course, according to disciplinary area as well as across national contexts and language communities. Around the middle of the twentieth century, methodological texts generally treated natural science as the model to be followed, with *method* being seen as the driving force behind science.² It was widely believed that the development of experimental method in the sixteenth and seventeenth centuries had been crucial to the remarkable success of the natural sciences, enabling them subsequently to make startling discoveries about the nature of the Universe, the constituents of matter, and the character and development of living organisms. Not surprisingly, much effort was soon made to apply 'scientific method' to the task of understanding the social world. Furthermore, it was widely assumed that this could lead to progress in overcoming

²This idea can be traced back at least to the writings of Francis Bacon. For a sophisticated account of Bacon's views in their historical context, see Gaukroger (2001).

the increasingly serious problems faced by large, complex industrial societies. The expectation was that social science could deliver parallel benefits to those which science-based technology had brought to many material aspects of human life.

Despite widespread adoption of natural science as a model, from the beginning there were important differences in views among social scientists about the nature of scientific method; as well as conflicting ideas about whether social science is distinctive in its goal or in the nature of the phenomena with which it deals; and, if so, about whether and how scientific method should be adapted to take account of this. Debates about these matters go back to the nineteenth and early twentieth centuries, when there were philosophical conflicts between inductive and hypothetico-deductive views of science, and also between those who took physics – rather than, for example, biology – as their model. In addition, there were arguments about the necessary methodological distinctiveness of the historical and social sciences (see Hammersley 1989: ch. 1). Moreover, by the middle of the twentieth century, there was an awareness on the part of many social scientists that their disciplines had not achieved the demonstrable progress characteristic of natural science in the nineteenth century, nor the same practical payoff. One response was to insist on the continuing immaturity of, and difficulties faced by, the social sciences. At the same time, this sense of failure undoubtedly stimulated the promotion of approaches that rejected the natural science model, and in some cases the very idea of *science* itself (Bateson 1984: ix; Smith 1989; Harding 1991; Denzin and Lincoln 2005; Hutchinson et al. 2008; Peim 2009).

In the second half of the twentieth century, there were also significant changes in attitude towards natural science in the wider society. Its beneficent image began to be tarnished by public recognition of its negative side: of the uses to which its methods and products had been put, for example in warfare and in the Holocaust; of the environmental consequences of the new industries it stimulated; of the disturbing possibilities it opened up in biogenetics; and even of the means it employed, such as animal experimentation. As a result, there was a shift in view about the nature and value of scientific knowledge. As long ago as 1972, the philosopher of science Mary Hesse noted the consequences:

Various intellectual and moral tendencies are currently combining to dethrone natural science from the sovereignty of reason, knowledge, and truth which it has enjoyed since the seventeenth century. Far from being the paradigm of objective truth and control which will make us free of all natural ills and constraints, science is increasingly accused of being a one-sided development of reason, yielding not truth but a succession of mutually incommensurable and historically relative paradigms, and not freedom, but enslavement to its own technology and the consequent modes of social organisation generated by technology. (1972: 275)

These wider challenges to natural science tended further to undermine its role as a theoretical or methodological model for many social scientists. One consequence of this, in the second half of the twentieth century, was the emergence of a fundamental

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division between quantitative and qualitative approaches within many fields of social science. Views of method as requiring quantitative measurement and the control of variables that were dominant in many areas began to be abandoned by a growing number of social scientists, on the grounds that these were based upon a false, positivist philosophy. Furthermore, qualitative researchers started to draw on very different ideas about the proper nature of social enquiry: from nineteenth-century philosophies like hermeneutics or pragmatism to influential strands in twentieth-century continental philosophy, such as critical theory and post-structuralism. Over time, qualitative research increasingly fragmented into competing approaches that marked themselves off from one another in the name of conflicting philosophical and political commitments: interpretive, 'critical', feminist, constructionist, postmodernist, etc. And these developments led to a considerable diversification of the methodological literature.

THREE GENRES

We can identify at least three broad genres within the literature on social research methodology today:

- 1 Methodology-as-technique
- 2 Methodology-as-philosophy
- 3 Methodology-as-autobiography

In each case, a particular kind of methodological writing is treated as central, on the basis of various assumptions about the nature of social enquiry, what it can produce, and the conditions for doing it well.³

METHODOLOGY-AS-TECHNIQUE

In the 1950s and 1960s, methodological writing tended to focus on research designs concerned with hypothesis testing, the details of experimental and survey method, measurement strategies, and techniques of statistical analysis.⁴ What was involved here was a particular conception of social scientific research, whereby the questions to be addressed needed to be identified and made explicit at the outset, and quantitative methods were generally assumed to be required for a scientific approach; though non-quantitative methods were sometimes included as supplements. Furthermore, it was assumed that research method could be quite closely specified in terms of rules to be followed.

³These three genres are, of course, ideal types. Particular examples of methodological writing only approximate to them. Nevertheless, the typology provides a crude map of the field that may be of some use.

⁴For early examples of texts within the methodology-as-technique tradition, see Goode and Hatt (1952), Festinger and Katz (1953) and Galtung (1967).

Here, methodology was treated as providing the knowledge and skills that are essential for effective social science practice. This involved spelling out the nature of scientific method and its implications for doing social research, along with the provision of advice about how to approach the various decisions involved. There was also great emphasis on the need for social researchers to be *trained* in methodological procedures, especially in statistical techniques, so as to be able to carry out scientific work well.

Later in the twentieth century, methodological texts became broader in their coverage, generally giving more attention to qualitative methods, though they often preserved the emphasis on technique. This emphasis was even true of many early books that were specifically devoted to qualitative method, in the sense that they were primarily concerned with offering practical guidance.⁵

At its simplest, methodology-as-technique is an attempt to codify the methods social scientists use, specifying their character and proper application in relation to the different research tasks, indicating the grounds on which choices among methods should be made, and so on. And the primary audience here is often students and other novices who need to learn how to do research. The aim is to make method explicit and thereby to provide a basis for learning and improving it. Generally speaking, in this genre of writing, an apparently consensual image of how to pursue research is presented. Even where different methodological philosophies are recognised, these tend to be reduced to a relatively small number of clearly defined options that are to be chosen either according to fitness for purpose or as a matter of taste.

At its most extreme, what is involved here is what might be referred to as proceduralism: the idea that good practice amounts to following a set of rules that can be made explicit as a set of prescriptive dos and don'ts, or even in the form of recipes. Quantitative research is often believed to be codifiable in this way; but there is a temptation to try to proceduralise qualitative research as well, on the grounds that this must be done if it is to be scientific, and/or if newcomers are to be taught how to do it. However, the literature within this genre varies considerably in how closely it approximates to the procedural model.

The early methodology-as-technique texts came to be criticised because of the way they privileged quantitative work, for their 'positivist' philosophical orientation, and/or for their encouragement of recipe following. They increasingly came to be seen as at odds with the spirit of qualitative enquiry, not least because of the latter's emphasis on the importance of creativity in research, and on the role of personal, social and cultural factors in shaping it. Proceduralism, in particular, was rejected for being ideological: that it systematically obscures the fact that research is done by people with distinctive characteristics in particular socio-historical locations, and that it is based on philosophical assumptions.

⁵Examples include Junker (1960), Glaser and Strauss (1967), Denzin (1970), Lofland (1971) and Schatzman and Strauss (1973). More recently, a form of literature has emerged covering both quantitative and qualitative methods that is very practice-focused and instrumental in character. See, for instance, Bell (2005), Phillips and Pugh (2005), Denscombe (2007; 2009) and O'Leary (2009). Such books would also come under the category of methodology-as-technique.

METHODOLOGY-AS-PHILOSOPHY

One of the effects of the rise of qualitative approaches and associated criticism of quantitative method, and of subsequent disputes amongst proponents of competing qualitative paradigms, was the flourishing of a new genre, what I will call methodology-as-philosophy. Early textbooks, and other publications, in the methodology-as-technique genre had often included some coverage of philosophical ideas about the nature of science, but this was usually restricted to brief preliminaries. Moreover, philosophical debates were generally presented as either already largely resolved or as of minimal practical significance for how research ought to be done. There was rarely much indication that there were sharply conflicting views among philosophers of science or that there are unresolved philosophical problems surrounding social science; despite the fact that, by the end of the 1950s, the philosophy of science was in turmoil, older positivist ideas having collapsed largely as a result of internal criticism (Suppe 1974).

As already noted, many of the early introductions to qualitative method adopted a primarily practical focus, and they too generally gave relatively little space to philosophical issues – by comparison with many later treatments. However, there were already signs of the emergence of a different emphasis. In their influential book *The Discovery of Grounded Theory*, Glaser and Strauss (1967) argued for a distinctive methodological approach, against the preoccupation with testing hypotheses that dominated quantitative research, and also against the tendency towards a descriptive orientation in much qualitative work. While they make little appeal to the philosophical literature, what they address here are nevertheless philosophical issues: as I noted earlier, there had been a long-running philosophical debate about inductive versus hypothetico-deductive interpretations of scientific method (see Gillies 1993). The year before Glaser and Strauss's book, Bruyn's *The Human Perspective in Sociology* (1966) appeared, and this was largely concerned with outlining the competing epistemological and ontological principles he identified as underpinning qualitative, as against quantitative, enquiry.

Subsequently, the amount of philosophical discussion in the methodological literature increased considerably, as 'new' qualitative paradigms sought to distinguish themselves from earlier ones. Furthermore, the character of the philosophical ideas that were appealed to by qualitative researchers changed over time: the influence of nineteenth-century hermeneutics, pragmatism, Marxism and critical theory was later accompanied or displaced by appeals to structuralism, philosophical hermeneutics, deconstruction and other forms of post-structuralism and 'postmodernism'. In the course of the battles that took place, older philosophical rationales tended to be rejected under the catch-all term 'positivist', this becoming an example of what Passmore calls a 'dismissal-phrase' (Passmore 1961: 2).⁶

⁶For an account of positivism and an argument that what this term refers to still has value, see Hammersley (1995: ch. 1). Ringer (1969: 298–301) notes a similar tendency to brand all that is anathema with the label 'positivist', this time among German academics at the beginning of the twentieth century. He highlights the context-dependent and variable meaning that the term had acquired even then.

Many of these developments raised fundamental issues. For example, within Marxism the question arose: in what sense can there be scientific study of the social world that escapes ideology, and what requirements must be met to achieve this? Pragmatism raised the question, among others, of in what sense human behaviour can be segmented into units among which determinate causal relations operate, and therefore in what sense such behaviour is amenable to scientific investigation. For hermeneutics, the issue was whether and how we can understand other cultures; and, later, what the implications are of the fact that all understanding is a product of socio-historical location. Ethnomethodology generated questions about what would be required for a fully scientific approach to the study of the social world, in the sense of one that does not trade on commonsense knowledge; and about whether social phenomena have the determinate character that is required for scientific investigation. From post-structuralism, there was the issue of whether discourse, perhaps of any kind, can escape being a reification of the world, an imposition on it and an expression of power.

Central to this new literature, often, has been a very different view about the relationship between research and philosophy from that which had informed the earlier concern with methodology-as-technique. The latter treated philosophy as providing a specification of what a scientific approach required, thereby paving the way for a technical approach to research that left philosophy itself behind, relying instead, for example, on statistical theory. In fact, the sort of positivism that underpinned this early literature often assumed that philosophy itself could and should become scientific, with logic as its core (see Friedman 2001: ch. 1). By contrast, many of the philosophical sources on which qualitative researchers drew did not treat science as distinct from philosophy, and certainly not as superior to it or as uniquely exemplifying rationality – unless rationality was itself being dismissed. Some viewed science as a mode of rational thought that was broadly philosophical in character. Others challenged science of the kind that had become prevalent as based on a false philosophy, and therefore as representing a form of intellectual and political oppression.⁷

In addition, a change took place in ideas about the history of natural science over the course of the twentieth century, with the emphasis shifting away from the role of experimental method towards a stress on how philosophical ideas had shaped scientific development (see Burt 1924; Koyré 1957). The implications of this, and of increasing criticism of positivist philosophy of science, were embodied in Thomas Kuhn's (1970) enormously influential *The Structure of Scientific Revolutions*. The impact of this book was much greater in the social than the natural sciences, despite the fact that Kuhn specifically sidelined these as 'pre-paradigmatic' and therefore as pre-scientific. For Kuhn, a mature science generally operates within a largely taken-for-granted framework or paradigm of theoretical and methodological ideas, embodied in major discoveries that are treated as exemplars of scientific work in the field concerned. However, when some of the problems that scientists are working on within a paradigm come to be recognised as recalcitrant, and when an alternative framework is available, fundamental change can occur. In such a 'scientific revolution',

⁷Influential sources for these various views are Habermas (1968), Gadamer (1975), and Lyotard (1993).

philosophical debates emerge about the phenomena being studied, how they should be conceptualised, and what constitutes evidence about them. As a result of this, eventually, the paradigm that had previously been taken for granted may be replaced by another. This sets up a new range of ‘puzzles’ that scientists in the field tackle, and in doing this they treat the new framework of paradigmatic assumptions as given, so that once again science becomes a largely technical activity.⁸

A key feature of Kuhn’s account here is his argument that different paradigms are incommensurable: there is neither an overarching framework that can provide a means of assessing them nor an independent body of data that can adjudicate among their conflicting theoretical and methodological assumptions. This notion of incommensurability undermined the previously influential conception of science as accumulating knowledge over time through the application of a distinctive method. As a result, it became very common for social scientists to see the different approaches in their field as competing, incommensurable paradigms. Furthermore, whereas the natural science paradigms that Kuhn identified differed solely in their assumptions about the nature of the phenomena being studied and how these could best be investigated, social scientific paradigms came to differ also in ideas about what the purpose of research is, as well as about its relationship to politics and various forms of organisational and occupational practice. Indeed, as noted earlier, the model of science itself came to be abandoned by some, in favour of alternatives that included political commentary, autobiography, imaginative literature and art. From these perspectives, the main declared goal of social research sometimes became political change, personal or professional development, the realisation of ethical ideals, and/or aesthetic impact.

As should be clear, methodology-as-philosophy took discussion in methods texts into some of the most contentious areas of philosophical enquiry, including the following:

- 1 Whether research can identify causal processes operating in the social world, or whether what it documents are social constructions that people produce through their interpretations of and interactions with one another.
- 2 Whether enquiry is a process of discovery, in which extant features of the social world are documented, or whether research itself necessarily *constructs* the phenomena that it claims to document.
- 3 Whether any account of the world necessarily reflects the social and personal characteristics of the person(s) who produced it, in a way that undercuts claims to representational accuracy.
- 4 The differences, if any, between social scientific research reports and fictional writing, such as novels.
- 5 The political and ethical responsibilities that researchers have in ‘representing’ the people they study, one issue here being: how can these people and their lives be portrayed ‘authentically’?
- 6 Whether objectivity is possible or desirable; and, in fact, what the term means. There is a host of sub-questions here: Is it possible to represent ‘objects’ in the world as they are in their own terms? Should people be viewed as objects? Is

⁸For a post-Kuhnian elaboration of the role of philosophy in the development of natural science, see Friedman (2001: 20–4).

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it possible to produce accounts of social phenomena that are unbiased; and, if it is not, what are the implications of this for the (at least implicit) claim of social science to produce knowledge that is valid or true?

- 7 Whether enquiry can and should adopt an orientation that is detached from social or political practice. In particular, there is the question: should it be directed towards bringing about some kind of social change, serving the interests of a particular group or category of people, improving some practice, etc.?
- 8 Whether social research should be pursued as a distinct enterprise in its own right or should take the form of 'action research'. And, within this context, there is the issue of whether equity requires that the relationship between researchers and those they are 'researching' be one of partnership, or even involve the researcher adopting a subordinate role.

Needless to say, these are challenging questions, and a wide variety of stances towards them can now be found in the methodological literature, often amounting to what Smith (2004: 51) has referred to as 'deep heterogeneity'.

So, in place of the earlier focus on scientific method, on rules and procedures, there came to be an emphasis in much methodological writing on the philosophical assumptions underpinning various forms of research practice; on the creativity of research, with convergences to imaginative literature and art; on the centrality of ethics and politics; and on the need to be reflexive, continually questioning one's philosophical and political assumptions. This last notion, the commitment to reflexivity, was also central to the third main genre of methodological writing, which also arose largely as a result of the growing popularity of qualitative work.

METHODOLOGY-AS-AUTOBIOGRAPHY

In 1955 William Foote Whyte published a 'methodological appendix' to his classic qualitative study *Streetcorner Society*.⁹ In this, he offered an autobiographical account, or 'natural history', in which he told the story of how he came to do the research on which his book was based: how he had gained access to the Italian community that he was studying in Boston, how his relations developed with informants, the problems that he faced and how he sought to resolve them, and so on. His account became very influential and there was an explosion of such accounts of particular studies in the late 1960s, 1970s and 1980s.¹⁰ This 'methodology-as-autobiography' literature often took the form of chapters or appendices in books or theses, but there were also journal articles and a considerable number of *collections* of research biographies appeared. There were even some whole books devoted to explicating the research process involved in particular studies (see, for example, Rabinow 1977; Cesara 1982).

⁹Whyte's appendix was reprinted and extended in later editions, see Whyte (1993). This was not the first example of methodology-as-autobiography, for example Laura Bohannon had already published a pseudonymous fictional account of her anthropological fieldwork in Africa (Bowen 1954).

¹⁰For a listing of many of these, see Hammersley (2003a).

These 'reflexive accounts' grew to form a very large corpus; and, in addition, there was increased use of autobiographical material, and of other people's accounts of their research, as a source of illustration in qualitative methods texts (see, for example, Johnson 1975; Hammersley and Atkinson 1983).

From early on, some information about the design, data collection and analysis procedures employed in studies had, of course, been included in research reports. But the content, amount, and tone of later more autobiographical accounts were different. Where previously most of the information had been basic methodological facts about how the research had been carried out, perhaps with some technical problems mentioned, the new natural histories often *emphasised* the problems faced, especially those concerning relations with people in the field, how these were dealt with, the researcher's own personal responses to the research process, and so on. Furthermore, what was stressed, often, was how, in practice, research deviated from textbook accounts; natural histories sometimes thereby opened researchers up to methodological and moral criticism.¹¹

The rationales provided for this third genre varied considerably. One involved criticism of the role of standard methodological texts in preparing newcomers to do research. It was argued that they did not cover all relevant aspects of the research process, especially as regards qualitative work. In particular, textbook accounts tended to say little in detail about social relations in the field and the problems that could arise in this area, yet these could be major obstacles. Closely related was an argument to the effect that much of the existing literature was relatively abstract, giving only *general* guidance. It was pointed out that concrete examples could be more illuminating for those learning how to do research. There was also concern about the picture of research presented in methodological textbooks: that it was, to a large extent, a rational reconstruction of the research process, portraying how it *ought to be*, rather than how it *actually is*. The suggestion was that beginning researchers often experienced a huge gap between how methodology texts told them research should be done and their own experience of it, leading to a sense of incompetence and failure, when in fact what they had experienced was normal. So, part of the rationale for methodology-as-autobiography was to provide a more realistic account of the research process for students.

Closely associated with all this was the idea that research is a craft, with the implication that how to do it cannot be learned as an abstractly formulated set of rules or techniques, or derived from some idealised model, but rather only through first-hand experience, and/or through accounts of actual studies produced by other researchers, these providing a basis for vicarious learning.¹² The argument here was that

¹¹Bell and Newby report that they invited the contributors to the volume they were editing to 'own up' (1977: 11). Also influential was the publication in 1967 of the diaries that Bronislaw Malinowski had written while carrying out his early fieldwork. These provoked consternation at the disparaging remarks he made about the people he was studying (see Wax 1971).

¹²Around the same time as the growth of published natural histories, there was an increasing tendency to introduce project components into research methods courses, so as to give students direct experience of actually *doing* research. Note, though, that this had long been central to the education of neophyte sociologists at the University of Chicago, where case study work had been pioneered; see Bulmer 1984.

research is a practical rather than a technical activity: it necessarily involves making judgements, often on the basis of uncertain and inadequate evidence. This stems, in part, from the fact that it is subject to all manner of contingencies to which the researcher must respond. These contingencies are especially severe in the case of qualitative research. For instance, Everett Hughes argued that ‘the situations and circumstances in which field observation of human behavior is done are so various that no manual of detailed rules would serve’; though he insisted that the basic problems faced by all field researchers are more or less the same (Hughes 1960: x). In other words, doing research in ‘natural’ settings – that is, under conditions that are not specifically designed for carrying out research – and often over relatively long periods of time, mean that it is essential to adapt the research process to the situation and to any significant changes in it. This may be necessary even just to ‘survive’ in the field so as to continue the research. However, there are also specifically methodological reasons why qualitative research cannot usually be a matter of following some pre-specified plan. For one thing, failure to adapt to the situation being studied is likely to maximise reactivity and thereby to threaten the validity of the findings. Furthermore, the open-ended approach to data analysis which is characteristic of qualitative work means that ideas about what data are required will change over time; the requirements cannot be identified completely at the beginning.

Thus, it was argued that social research involves improvisation on the basis of past experience, plus situated judgements about what is and is not possible and desirable in particular circumstances. And the conclusion drawn from this by many qualitative researchers was that while methodology can supply heuristics, such as ‘tricks of the trade’ (Becker 1998), it cannot provide recipes for doing research or even specific guidelines. Moreover, these heuristics are best conveyed by concrete examples derived from actual research experience.

We can find many of these arguments in the introduction to one of the earliest and most influential collections of natural histories, that of Bell and Newby (1977). But these authors add another point as well. Besides complaining that textbooks do not represent the research process accurately, they also reject what they describe as their ‘normative’ character (p. 10). It is the emphasis on ‘what ought to be done’, they suggest, that leads to textbooks presenting a misleadingly ‘context-free’ account of research. In particular, what are neglected are the political aspects of research: ‘everything from the micropolitics of interpersonal relationships, through the politics of research units, institutions and universities, to those of government departments and finally to the state’; and they argue that ‘all these contexts *vitaly* determine the design, implementation and outcome of sociological research’ (p. 10). What is required, from this point of view, is a descriptive rather than a normative approach to methodology.¹³

Another argument underpinning methodology-as-autobiography was that textbook accounts present a false image of the researcher. For example, Whyte complained that these accounts place the discussion ‘entirely on a logical-intellectual basis’:

¹³This blends with ideas about the sociology of sociology that were influential at the time, see Friedrichs (1970) and Gouldner (1970).

they fail to note that the researcher, like his informants, is a social animal. He has a role to play, and he has his own personality needs that must be met in some degree if he is to function successfully. Where the researcher operates out of a university, just going into the field for a few hours at a time, he can keep his personal social life separate from field activity. His problem of role is not quite so complicated. If, on the other hand, the researcher is living for an extended period in the community he is studying, his personal life is inextricably mixed with his research. A real explanation, then, of how the research was done necessarily involves a rather personal account of how the researcher lived during the period of study. (1955: 279)

In fact, Whyte's argument here subsequently came to be applied even to those only 'going into the field for just a few hours at a time'. It was emphasised that in all research the decisions made in the field will necessarily reflect the social identity, personality, and feelings of the researcher – including her or his reactions to the events and people being studied.

As this indicates, a crucial issue is the effect of doing research on the researcher. Bell and Newby, for example, note that in the course of their own work 'we became different people' (1977: 16). They, and other commentators, emphasised that research can be a stressful process, and that how the work is done will inevitably be shaped by how researchers feel about the people they are studying, their fears about what might happen, etc. So one of the major themes in the methodology-as-autobiography literature came to be the emotional dimension of research (Henry and Saberwal 1969; Carter and Delamont 1996).

Whereas in methodology-as-technique the image is of the researcher as a rational actor deploying technical skills to resolve standard problems, and remaining much the same throughout the process, in methodology-as-autobiography the researcher is very often portrayed as at the mercy of events; as coping or failing to cope with contingencies; as winning through by luck as much as by expertise; and as changing in attitude and feeling over time. It came to be argued that reflexive accounts should reveal 'at least some of the human costs, passions, mistakes, frailties, and even gaieties which lie behind the erstwhile antiseptic reports of most social scientists' (Bell and Newby 1977: 14).

An important aspect of this argument, emphasised by some commentators, was that most textbook accounts of social research tended to portray it as a smooth, cooperative process. What came to be highlighted instead, often, were the conflicts that researchers often found themselves involved in with some of the people they were studying, especially those in powerful positions. And this was sometimes taken to signal that researchers might need to adopt a strategic, even a Machiavellian, approach in order to get the data required, on the model of investigative journalism (see, for example, Douglas 1976).

Another strand of argument promoting methodology-as-autobiography was concerned with what readers need to be provided with if they are to be able to assess the findings of a study. As noted earlier, prior to the emergence of this genre, studies had offered some information about how the research had been done, but this was quite

limited in character. Since research was assumed to involve following particular methods, or applying specific techniques, minimal information about the researcher was thought to be necessary. However, once it was recognised that qualitative research cannot take a pre-designed and standardised form, it followed that a much fuller account was required of the research and of the researcher, if readers were to be in a position to assess or even interpret the work.

One version of this argument was that researchers should provide an 'audit trail', so that how they came to the conclusions they reached is made available to readers for checking (Lincoln and Guba 1985; Schwandt and Halpern 1988; Erlandson et al. 1993). This was seen as constituting an alternative form of rigour to that characteristic of quantitative research. In place of the argument that rigour involves following rules, thereby allowing replication as a test for the reliability and validity of the findings, it was suggested that the demand for rigour could be met by continual and careful reflection on the research process by the researcher, in terms of possible sources of error, plus documentation of this reflexive monitoring for readers, so that the latter could make their own assessments of likely validity.

Other writers took this notion of reflexivity in a different, more radical epistemological direction.¹⁴ Here it was argued that any research is necessarily infused by a distinctive personal perspective. As a result, notions of bias and error are eclipsed: research reports are not to be evaluated in terms of impersonal criteria, but should rather be judged in relation to the person and process that generated them. This involves a move away from the idea that research findings can accurately reflect the nature of the phenomena studied, in favour of a more constructivist point of view. On this basis, it often came to be argued that any account is necessarily partial and subjective, and as such should be assessed in ethical or aesthetic, rather than epistemic, terms.¹⁵

Also relevant here are ethical views which see reflexivity in terms of fairness: that if a researcher is asking people to expose themselves by providing information about their lives, then the researcher's own character and life ought to be included within the focus of the research. Not to do this, it was sometimes argued, is to imply the superiority of the researcher, to suggest that he or she is or could be a god looking down on the world, offering 'a view from nowhere'.¹⁶ This led to the argument that natural histories of research should not be separated off from the main body of the research report but incorporated into it, so that the whole report should have a self-reflexive character (see Stanley and Wise 1983; 2002).

These radical versions of reflexivity arose from increasing emphasis on the creative character of research, the insistence that 'the personal is political', and the growing use of literature and art as models, in place of natural science. One formulation, that of Denzin and Lincoln, portrays the researcher as a bricoleur, who draws on a variety of resources to produce images or impressions of the world, in ways that are analogous

¹⁴Reflexivity' is a term that is used in a variety of ways. For an outline of these, as part of a critique of the sense of the term I am discussing here, see Lynch (2000). See also Hammersley (2004c).

¹⁵See, for example, Mauthner et al. (2002), Denzin and Lincoln (2005).

¹⁶This widely used phrase seems to derive from Nagel (1989).

to collage or jazz as art forms (Denzin and Lincoln 2005). An alternative would be the idea that the path which any research project follows is necessarily both contingent and constitutive; that it is under the control of nothing and no-one, and *represents* nothing and no-one – certainly not reality or rationality. In other words, it must be seen as a matter of necessarily arbitrary ‘decisions’ among incommensurable possibilities. Either way, we are as far as we could be from the idea that research involves following the procedures of scientific method. From this radically reflexive point of view, methodology – in the sense of a concern with specifying techniques and methods, how they should be used, what would count as valid measurement, etc. – is simply a distortion of the research process; one created through the ideological imposition of a natural scientific or technical model, under the influence of a false positivist philosophy.

THE RISE OF ANTI-METHODOLOGICAL RHETORIC

Earlier, I argued that there is considerable ambivalence about methodology among researchers. In part, this amounts to a reaction against methodology-as-technique; for example by those who insist that social scientists must not neglect philosophical or political issues (see Chamberlain 1999). What is being rejected here is not methodology *per se*, but a particular version of it; one that prescribes rules to be followed that constitute ‘a system for offering more or less bankable guarantees’ (Law 2004: 9).

However, there are more fundamental sorts of anti-methodological argument to be found in the methodological literature. One of these has its roots in the craft model of research associated with some versions of methodology-as-autobiography.¹⁷ As we saw, this model generates scepticism about the value of abstract discussions of methods and of the rationales for them, whether these take a technical or philosophical form. Instead, it is argued that research problems can only be dealt with in concrete terms, in particular contexts. Thus, in his influential discussion of ‘intellectual craftsmanship’, C. Wright Mills comments that ‘serious attention should be paid to general discussions of methodology only when they are in [...] reference to actual work’, and he adds that if all social scientists followed this ‘obvious and straightforward’ practice, ‘at least all of us would then be at work on the problems of [social science]’ (1959a: 27).

This craft position does not amount to a total rejection of methodological writing. Rather, the latter is seen as playing a narrowly defined role, one that is very much subordinate to the actual practice of research. Thus, Clive Seale argues that ‘intense methodological awareness, if engaged in too seriously, can create anxieties that hinder practice’, but he recognises that ‘if taken in small doses (methodology) can help to guard against more obvious errors’ (1999: 475). Within the craft tradition, then, there is often an insistence on the limited and subordinate function of methodology in relation to the practice of enquiry. At most, the methodologist’s role is that of an

¹⁷On the craft model, see Hammersley (2004b: 550–2).

under-labourer, clearing the ground for research, and offering guidance about dangers. If methodology is given too much weight, so the argument goes, it becomes a diversion from the real work of enquiry, and perhaps even a positive hindrance.

A rather different version of anti-methodological rhetoric emphasises the need for researchers to have the freedom to engage creatively with their data in finding answers to research questions, with methodology (at least of a certain kind) being seen as amounting to an unwarranted form of constraint. For example, in his critique of methodolatry, Chamberlain argues that:

developing a good interpretation requires thought and creativity, and its outcomes should be provocative and insightful. Codified approaches to method and analysis have a particular problem in capturing and presenting this. (1999: 290)

Here, an objection to ‘inflexible and inappropriate guidelines’ can merge into a general opposition to codification on the grounds that this obstructs good-quality research, this requiring the exercise of free theoretical interpretation directed not towards the discovery of facts about the world but rather to the construction of perspectives that can shape practice in ways that bring about social change (see also Law 2004). We should perhaps note, though, that, from the point of view of the craft tradition, the solution to the problem of methodolatry Chamberlain proposes – that we start from epistemology, then move to the issue of theory, finally ending up with method (p. 294) – might be seen as a symptom of the illness rather than a cure, on the grounds that it involves an (over)emphasis on methodology-as-philosophy.¹⁸

Interestingly, these kinds of anti-methodological argument do not occur only among social scientists. They have been directed more widely against attempts to specify the nature of scientific method in the philosophy of science. For example, echoing Bridgman’s (1955: 535) definition of science as ‘nothing more than doing one’s damndest with one’s mind, no holds barred’, but developing the point in a rather different direction, Feyerabend argued, in *Against Method* (1975), that methodological prescriptions not only do not accurately capture how successful natural scientists do their work but actually obstruct the process of scientific investigation. A slightly different version of this argument, developed in the humanities and relating to methodology-as-philosophy not just to methodology-as-technique, is the position of Fish. He writes:

Historians do not gain credibility (or anything else) by becoming meta-historians, that is by giving big answers to large questions like, What is the nature of fact?, How does one determine what counts as evidence?, Can the past be reconstructed?, Can the distinction between the past and the present be maintained? Whatever answers you give to such questions will be entirely unhelpful and beside the point when you return from their airy heights to the questions historians appropriately ask. (Fish 2001a: 510; see also Fish 2001b)

¹⁸For this kind of judgement about much methodological writing, see Mann (1981).

METHODOLOGY, WHO NEEDS IT?

These anti-methodological arguments fitted the spirit of some of the qualitative approaches that gained ground in social research in the second half of the twentieth century in many fields. And, ironically, this anti-methodological spirit has itself added to the methodological literature. For example, Phillips reports that his book *Abandoning Method* was written to justify a course in methodology (1973: xi). Much more recently, Law (2004) has suggested that we must move beyond 'method', recognising that the 'realities' that social science deals with are often 'messy' rather than well-defined, and therefore cannot be captured by rule-based procedures and theories, so that in an important sense social science 'makes' the phenomena it purports to describe and explain. On its back cover, his book is described as being 'essential reading for students, postgraduates and researchers with an interest in methodology'. We might interpret all this as suggesting that social scientists' ambivalence about methodology amounts to a futile revolt against the inevitable need both to try to spell out the methods they use and to engage in methodological and philosophical reflection. However, it also shows an awareness of the limitations and dangers involved in this.

Against the background of the diverse character of the methodological literature today, and ambivalence towards and within it, in the next section I want to try to determine its proper nature and role. I will look again at the three genres I have identified, emphasising both what they can contribute and the limits to their value.

THE FUNCTION OF METHODOLOGY: EVALUATING THE THREE GENRES

We need to begin by looking at the meaning of the word 'methodology', as currently used. In its core sense, this refers to a discipline concerned with studying the methods employed in carrying out some form of enquiry. However, its meaning also extends to include the body of knowledge built up through this methodological work. And, in the context of the diverse orientations within social research outlined above, it is a short step to the use of 'methodologies' to refer to distinct approaches to studying the social world that involve conflicting ideas not just about methods but also about the intended goal and products of research, the ontological and epistemological assumptions involved, how the role of research is defined in relation to other activities, and so on. As a result, 'methodology', when used to refer to an area of study, has now come to include not just discussion of methods but also discussion of the philosophical and political issues that differentiate the many approaches to social research that now exist.

In this section I will argue that there is a place for all three of the genres of methodological work that have been identified: they all serve important functions. However, at the same time, each is in danger of, and has been subject to, 'overdevelopment'.

Methodology-as-technique. It should be clear that some knowledge of the various methods available is essential for social researchers. Newcomers must learn what these are, how they differ from one another, what is involved in their use and the

problems that may be faced. Also, more experienced researchers will often have to improve or revise their knowledge of methods when embarking on new projects, since few if any of them will have a comprehensive, in-depth knowledge of all the techniques and strategies available in their field. Moreover, new methods sometimes emerge, or new applications of old ones; and, sometimes, new problems can arise in the use of existing methods as a result of changing circumstances. For these reasons, methodology-as-technique will be relevant to experienced researchers as well as to new entrants.

In these terms, we could ask whether the methodological knowledge that most researchers have today is adequate. There are those who believe it is not. A currently influential charge is that many social scientists have a sound knowledge of qualitative method but lack the necessary grasp of quantitative techniques. There may well be some truth in this, but there are also those who would insist that there is a widespread lack of adequate knowledge about qualitative method too. Furthermore, a broader critique could be mounted, relating to both sides of the qualitative–quantitative divide. It might be suggested, for example, that many social scientists are not aware of the full range of data collection and analysis strategies that would be relevant to the projects in which they are engaged; that they tend immediately to adopt standard methods and standard forms of these. If any of these charges is sound, it would require that methodology-as-technique should be given even more emphasis than it currently is; though there will be disagreement about what form this ought to take, and about the methods that should be emphasised.

Of course, methodology-as-technique also carries dangers. The most important, already noted, is proceduralism: a belief that rules can be laid out for applying particular methods, for example on analogy with specification of the steps that must be followed in using software to carry out statistical tests, or in doing the calculations oneself. In fact, very little of the research process can be reduced to rule following of this kind: it is too uncertain and complex a business, so that a significant level of judgement is always required. Given this, attempts to specify rules will have negative consequences. One example of proceduralism – to be found in many general, introductory methods texts – is the reduction of research design to a fixed and standard sequence of steps. This requires the researcher to: turn research questions into specific hypotheses; operationalise the variables making up those hypotheses; establish procedures for the control of both the hypothetical cause and confounding variables; and identify the statistical techniques that will allow the resulting data to test the hypotheses and reach conclusions about their likely validity. While, in one form or another, these various activities constitute essential aspects of any piece of social research, it is a mistake both to assume that all research must start with hypotheses and be directed towards testing them, and to insist that these various activities must, can, or should be carried out in this fixed sequence and form, each being completed before the next is begun. Instead, research design should be seen as an iterative process in which judgements must be made about what it is best to do in relation to all these issues, given the purpose of the research and present circumstances, throughout the course of enquiry. Furthermore, this may even involve changes in research focus and in the kind of knowledge aimed at, and therefore in the ways in which the various tasks making up research design are approached (see Maxwell 2004).

Also involved in proceduralism is the idea that the research process can and should be made 'transparent', in other words that it can be fully specified, thereby allowing for replication by other researchers. It is certainly true that readers of research reports need to have information about how the work was carried out, and that in some cases replication can be a useful means of checking the results. However, just as doing research cannot be reduced to following procedures, so too it is not possible to give a complete account of how any piece of research was actually carried out; nor would it be productive to attempt this. Rather, a reader only needs *sufficient* information to be able to make an assessment of the likely validity of the findings. For the purpose of replication, the information required may be greater, but it is still selective. Moreover, the aim of replication is not simply to copy what was originally done, but to find out whether it is possible to produce the claimed results, and this may involve strategic variation in the methods used.

Given this, the fear that methodology-as-technique can put blinkers on the researcher, restricting creativity and innovation, is a genuine one. At the same time, creativity cannot operate in a vacuum. There is always a need for knowledge about existing methods and methodological ideas. While proceduralism is a danger, so too is ignorance.

Methodology-as-philosophy. Doing research always involves relying upon philosophical assumptions, and there will be times when these need to be reassessed. This can arise in a variety of ways. It may be that practical difficulties emerge in the course of an investigation, which then lead to doubts about what was previously taken for granted. Equally, though, doubts may be generated by external influences. As noted earlier, over the years social science has been shaped by ideas from a variety of philosophical traditions, and these have sometimes stimulated re-evaluations of the assumptions on which past work had relied. The belief that research can be an entirely technical or practical matter – philosophy-free, as it were – is an illusion; there must always be some reflection on what is being done and why, and sometimes this will involve issues that have preoccupied philosophers. Much can be gained from drawing on their work. Moreover, in a context where there are multiple conflicting external influences, and where there are often several competing approaches within any research field, attention to philosophical issues will normally be required in order to decide on and to justify the approach being adopted in any particular study.

However, there are also some dangers with methodology-as-philosophy. One is that philosophy will be plundered and misinterpreted in ways that are not helpful for the pursuit of social enquiry (see Hammersley 2006a). Many years ago, Tudor noted how sociologists have used philosophy 'much as the military might use a guided missile':

Recognising the incipient power of labels borrowed from philosophy, sociologists have strewn them about with little regard to their detailed significance. Indeed, if armies were so irresponsible (and they may yet be) I should not be writing, nor you reading, this essay. We would have since vanished in clouds of nuclear fallout. (1982: 1–2)

Another problem is that attention may be given to philosophical issues that do not have major implications for actually going about social research. For example, while

there are undoubtedly important differences between positivist and realist accounts of science (see Keat and Urry 1975), in my view it is doubtful whether these, in themselves, carry much import for social scientific work. There are two reasons for this. First, there is a great deal of variation within each of these philosophical approaches, so that what they can be taken to recommend overlaps substantially. It is only if we take extreme versions of both that clear water appears. Secondly, drawing inferences from each of them about how social research should be carried out is necessarily a process of interpretation, in which other assumptions are relied upon, for example about what is possible in a particular area of enquiry. So, for example, not all of those who have been influenced by positivism have given primary emphasis to quantification and measurement; and some of those doing quantitative work appeal to realism rather than to positivism.¹⁹

A third point is that there are some philosophical assumptions on which social scientists rely whose validity there is no point in their questioning *in the course of planning and carrying out their work*. This is because the assumptions concerned cannot be avoided if social research is the task. In *On Certainty*, Wittgenstein refers to these as ‘hinge’ assumptions. He provides an example: if we were to start doubting whether the past extended beyond living memory, suspecting that what we take to be signs of the more remote past are simply artefacts constructed so as to make us believe in its existence, the very possibility of most historical work would be undercut (Wittgenstein 1969: paras 206, 234, 311).²⁰ Similarly, to dismiss sequential time as ‘a recent and highly artificial invention of Western civilisation’ (Ankersmit 1994: 33), which privileges Western over non-Western ways of viewing the world, and which therefore should be abandoned, is not to ‘re-think’ history (Jenkins 2003) but rather to abandon it (see Evans 1997: 141). More generally, to raise doubts about the very possibility or desirability of knowledge, in the sense of accounts that represent facts about the social world, removes the point of academic social science. We cannot do this kind of work if these assumptions are genuinely doubted. What results from abandoning them is not some new form of social scientific enquiry but rather turning it into something else: a form of philosophy, imaginative literature or political commentary; or into the blend of these that is characteristic of the diverse body of French writing that has come to be labelled postmodernist.

Questions about whether knowledge of the kind pursued by social science is possible and desirable can be usefully subjected to philosophical and other kinds of investigation, but these are not issues that there is any point in social scientists considering as part of their methodological deliberations. In doing social research we *must* believe that there are facts about the phenomena we are investigating that can be discovered, and that this knowledge is worthwhile. If we do not, then there is no point in pursuing the activity. Of course, outsiders – scholars in other disciplines and members of funding bodies or of the general public – may take a different view about these matters; and in response to this social scientists will have to address these

¹⁹For a different view about this issue, see Halfpenny (1982; 1997).

²⁰There are some differences in the interpretation of Wittgenstein’s argument here, see Kober (1996), Moyal-Sharrock and Brenner (2007).

questions in order to defend social science and sustain its funding. Similarly, individual social scientists may experience a crisis of faith about these matters. However, arguments about them cannot be part of methodology, they are not relevant to how researchers can best do their work. Despite this, there is currently much discussion of these matters in the methodological literature; along with arguments premised on the assumption that knowledge, in the ordinary sense of that word, is impossible or undesirable (see, for example, Denzin and Lincoln 2005; Peim 2009).

It should be added that *any* activity involves presuppositions on which it necessarily relies – without which it could not be pursued. These can always be questioned from the point of view of epistemological scepticism, as well as from other perspectives. Furthermore, there is no reason to suppose that the assumptions underpinning various kinds of empirical academic enquiry, including the social sciences, are any more open to reasonable doubt than those which structure other human activities, including those to which some qualitative researchers seem to want to turn: such as politics, ethics, literature, or art. Finally, in the most general terms, it makes no sense to doubt the possibility or value of knowledge, since in one form or another enquiry is integral to all human activity; nor does redefining ‘knowledge’ as belief or personal expression provide a satisfactory alternative.

So, there is a considerable amount of discussion today within the genre of methodology-as-philosophy that, in my view, is irrelevant or obstructive to the practice of social enquiry. Philosophical issues are frequently discussed in a manner that does not take proper account of the arguments developed to deal with them by philosophers. And they are sometimes treated as if they had direct and determinate implications for practice when they do not. Even more obvious is the way in which sceptical arguments about truth and value are deployed in much methodological discussion, in ways that flout the hinge assumptions of social enquiry. Inevitably, these arguments are used selectively – to criticise other positions – while their global implications are quietly forgotten when it comes to the critic’s own position and form of work.

Methodology-as-philosophy is essential, then, since we cannot avoid relying upon philosophical assumptions, and these can be problematic. However, a considerable amount of methodology-as-philosophy is excessive.²¹

Methodology-as-autobiography. Above, it was argued that much writing under this heading was stimulated by what I referred to as the craft tradition. The fundamental idea here is that research is a practical activity: it is directed towards particular products, and depends upon judgements and learning in specific circumstances about how best to produce these. This is clearly at odds with any idea that research is a matter of following rules or procedures, but it also denies that the problems it faces can be resolved primarily by philosophical means. It places great emphasis on situated decision-making, and the wisdom that can arise from experience in doing research and reflecting upon it. To a large extent it is a matter of trying out various strategies in the context of particular projects, and the circumstances in which these are being pursued, and then reflecting upon what has happened and been produced, with a

²¹In some quarters excess is valued, notably under the direct or indirect influence of Bataille (1991). By contrast, my model here is Aristotle’s notion of the mean; see Gottlieb (2009).

view to further practical decisions.²² So we must see methodological reflection as a recurrent activity that is closely tied to the process of learning involved in doing research. Indeed, reflection in, and on, the research process is the core of methodology. The methodological literature is *secondary*: it comes out of the ordinary methodological thinking involved in carrying out research, even though it also feeds back into it.

There is a tendency for some commentators within the craft tradition to deny the value of more abstract forms of methodological thought and the literature this produces. Here, again, is Mills:

As one begins his studies of some problem, he naturally turns first to studies that have already been done, and as he examines them, he certainly notices the methods their authors have used. He would be a fool if he did not do so. But once he goes beyond such an examination of the methods used in one area or another, and once he tries to transform methods into 'methodology', he often becomes quite abstracted. He loses firm connection with the kinds of problems for which given methods have been devised, and, in the end, makes quite formal, and often even useless, his examination of methods. Although not necessarily the case, this is surely a very real danger. (1959a: 26)

Clearly there is a genuine danger here, but any blanket rejection of more abstract methodological work can also be counterproductive. It is important not to see methodology as solely an individual, even less an idiosyncratic, pursuit.²³ The methodological thinking of the individual researcher or research team must be informed by collective discussion of methodological matters, and in such a way that progress can be made at least in understanding, if not in resolving, the problems that are faced by those working in the relevant area. Moreover, necessarily, this collective discussion will address more general questions than those which are specific to any particular project, and even to any single field.

So, one problem with methodology-as-autobiography is that its proponents often underestimate the value of methodological literature that goes beyond autobiographical accounts by individual researchers of particular projects. Sometimes what is in operation here is the idea that doing research is a matter of common sense, of ordinary knowledge and skills, and that the methodological literature renders it overly complicated and therefore opaque. Yet there *are* difficult problems, and there is much to be gained from noting differences between various kinds of work, following out the implications of particular lines of argument in fields that are different from those in which they were developed, addressing problems that face a wide range of kinds of work, and so on. The interaction between the specifics of what is involved in any particular project and more general reflection needs to be preserved. Without

²²Here, I am blurring the ancient Greek distinction between *poiesis*, the making of things, including art works, and *praxis*, the kind of decision-making characteristic of life more generally and of politics in particular. This blurring is also a feature of much argument about the nature of professions, especially where a contrast is drawn with the work of technicians.

²³It is striking in this context that Bell and Newby preface their introduction with the following quotation: 'idiosyncrasies of person and circumstance are at the heart not the periphery of the scientific enterprise' (Johnson 1975: 2; quoted in Bell and Newby 1977: 9).

this, every researcher is on her or his own, starting from scratch, and the result will be little methodological progress overall.

I have accepted that the core meaning of ‘methodology’ is the corpus of practical thinking and theory generated in actually doing research; so that in this sense *writing* about method is secondary. However, methodological writing is not simply the *inscription* of such thinking, it almost always involves its further development. It is an activity, or set of activities, that has a somewhat different, although no doubt overlapping, range of purposes even from oral discussion of methodology (and especially from the sort of internal dialogues in which researchers and research teams engage). Furthermore, writing about methodology can itself take a variety of forms, some closer to what is involved in oral methodological discussion, others further away. For example, blogging about method will be very different in character from writing a journal article or an introductory text. However, the value of more abstract kinds of methodological writing, for example drawing on specialised bodies of knowledge such as philosophy, mathematics, or literary theory, should not be dismissed.

There is also a problem with the concept of reflexivity, as this is sometimes interpreted within the methodology-as-autobiography genre. On some interpretations, it is taken to imply that researchers must somehow make their work fully transparent, explicating all that has gone into it, in terms of their own personal biography and philosophical-cum-political assumptions. Yet, not only is such self-explication impossible to achieve, since the process is never-ending, but the attempt to achieve it is likely to have very undesirable consequences for the pursuit of research. Taken to its logical conclusion, either no research will be done or the research that *is* done will not tell us much about what goes on in the world, but rather will be almost entirely preoccupied with explicating the ‘subject positions’ of the researcher. This is especially likely where ‘reflexivity’ is taken to signal that what is produced by research can be no more than a personal perspective on the part of the researcher, or a contingent co-construction with the people studied.

Another problem with methodology-as-autobiography is its insistence that methodology should be descriptive, not normative. Realistic reports of how particular studies were carried out are of great value, but they do not exhaust what is required. The fact that methodology is essentially normative follows directly from the practical character of research, which is central to the idea that it is a craft. The thinking that researchers engage in during the course of their work is about what they *ought to do*: whether what they have done was sensible, what they need to do now, what will have to be done later; and it will also extend to thoughts about the strengths and weaknesses of alternative strategies. Given that the function of methodological writing is to aid the practice of research, it must be normative. Even accounts of how researchers actually carried out particular pieces of work must be framed in ways that are relevant to practical concerns. In other words, they must be designed to lead into assessments of the advantages and disadvantages of different methods, the threats to validity associated with them, and so on. And they must be complemented by other kinds of writing that are more overtly normative in character. Of course, these must not be overly prescriptive – they must take account of inevitable variation in research goals and in the practical circumstances that researchers face. In other words, the

methodological literature can only offer general, not specific, guidance. But offering guidance is its task; it is not equivalent to either the sociology or the philosophy of social science.

Indeed, there is a danger that realistic reports of how actual studies were carried out, taken alone, will legitimate adaptations to circumstances that amount to bad practice; in the sense of strategies that are not effective means of pursuing the goals of the research. What is expedient in a particular study is not necessarily sufficient to produce conclusions that are more likely to be true than information from other sources; yet this is what is required for academic enquiry to be worthwhile. So, the kind of 'realism' that is at the heart of methodology-as-autobiography must not be taken to entail that whatever it is possible to do in particular circumstances must be treated as sufficient to warrant drawing the conclusions reached. Instead, we will have to recognise, for instance, that some research questions are simply not answerable given the constraints under which we are operating, and that attempts to answer them are misguided. In short, the fact that research methodology must be realistic does not mean that whatever was, or can be, done should be treated as adequate. Normative assessment is essential.

A further danger associated with methodology-as-autobiography stems from the idea that every researcher must be her or his own methodologist (Mills 1959b: 224). In an important sense, this is correct. But it can lead to the false conclusion that there is no need for some social scientists to specialise in methodology, or indeed that such specialisation is undesirable. A rankling objection to specialist methodologists is evident in Mills' writing on method:

I feel the need to say that I should much rather have one account by a working student of how he is going about his work than a dozen 'codifications of procedure' by specialists who as often as not have never done much work of consequence. (1959a: 28)

It is certainly true that there are dangers with specialisation, especially when this results in a cadre of methodologists who have little or no recent experience of actually carrying out social research, or where methodology becomes an enterprise with its own agenda that is not directed towards practical assistance and improvement of social research. But there are several reasons why a division of labour is desirable.

One is that researchers who have experience of a range of projects, perhaps of quite diverse kinds, or who have a talent for reflecting on methodological issues, can be an important source of insight and advice. A second point is that if methodological work is to be sound, and of value, then those doing it need to be familiar with the existing methodological literature. Contributions in this field, as in others, must take account of what has already been done and what is already known – if the same ground is not to be covered over and over again. Yet most practising social scientists do not have the time or resources to gain a reasonably comprehensive and deep understanding of what is now a very large literature. For any researcher, time taken reading the methodological literature is time not doing research. Given this, for most experienced researchers, most of the time, reading the methodological literature

should be limited to searching for material relevant to specific, pressing problems. But, at the same time, there can be gains from some people specialising in methodology.

Of course, not all kinds of contribution to the methodological literature make great demands upon the writer in terms of familiarity with the methodological literature. We can identify a continuum in this respect, running from publications chronicling particular research experiences, through accounts that formulate those experiences in terms of particular sorts of methodological 'issue' or 'lesson', to sustained arguments about proper and improper use of particular research methods or strategies, their advantages and disadvantages, as well as more abstract discussions of fundamental methodological or philosophical issues. The first two types of contribution are important and do not demand much in the way of knowledge of the extant methodological literature. There is no need to become a specialist methodologist to produce them. By contrast, the last two sorts of contribution make much larger demands on methodological expertise if they are to be done properly. They may require the author to draw not just on the existing methodological literature but also on philosophy, social theory, psychology, literary criticism, statistical and mathematical theory, natural science, and so on. Indeed, some contributions to methodology may take the form of presentations of ideas from these fields that are relevant to the pursuit of social enquiry, outlining what their implications might be. And it is important that such contributions are based on a sound understanding of the material being used.²⁴

I am arguing, then, that while methodological reflection is an essential element in the practice of research, and thus a necessary and proper activity for *all* researchers, a division of labour is required both in reading the methodological literature and even more so in making contributions to it, especially those of a more abstract and general kind. Furthermore, while methodological reflection is important for all researchers, generally speaking it must be limited for most of them to what is necessary for doing particular pieces of research, and to contributions that do not make great demands in terms of knowledge of the existing literature.

Specialist methodologists are under an obligation to disseminate the results of their work, whether through producing textbooks, reviews of portions of the literature, guides to ideas on particular methodological topics, or whatever. In other words, to provide resources that researchers can use in thinking about and tackling the problems they face. However, this activity, and the fact that it is essentially normative, raises the question of the extent and sort of authority that they can and should seek to exercise. Any division of labour involves an unequal distribution of authority, but this authority is always fallible and limited to particular issues. Needless to say, the response of individual researchers to recommendations and cautions in the methodological literature must be reflective, rather than a matter of automatic acceptance. Even if these recommendations and cautions are sound in general terms, they will

²⁴It is equally important that they draw on good knowledge of the relevant methodological literature, rather than it being considered, as a starting assumption that the new perspective renders this obsolete. Law's (2004) application of ideas from 'the discipline of science, technology and society' (p. 8) to the field of methodology is open to this charge. The claim that 'the proof of new ways of thinking about method [...] lies in their results and their outcomes, rather than in their antecedents' may be true, but the idea that this can be judged without learning from past methodological and philosophical discussions is a modernist illusion.

not usually apply straightforwardly to particular studies. At the same time, blanket scepticism about the value of the methodological literature is not wise; it will hamper or prevent improvements in social research.

There are many researchers who specialise in methodology today, not least as a result of the demands of postgraduate training programmes. Whether this division of labour works well is debatable. Those who write introductions to research methods, and make other general contributions to the methodological literature, do not always display knowledge of the most relevant previous discussions in that literature, or a sound understanding of the more specialist fields on which they draw. Indeed, it sometimes seems to be assumed that anyone who has done some research has what is needed to write sensibly about methodology. As a result, many contributions repeat what has already been covered, in apparent ignorance of what can be learned from the existing literature, and in the process they often perpetrate confusions that had previously been cleared up. Some also misrepresent and misuse previously well-understood ideas. Moreover, many stock assumptions that need to be questioned prevail in contributions to the methodological literature, about both quantitative and qualitative method. For example: that research is always necessarily political; that any enquiry must begin by specifying the methodological framework adopted and must stay within its limits; that the central assumption of positivism is that there is a real world, independent of our experience; that the findings of quantitative research are generalisable while those of qualitative work are not; that the sort of correlational analysis used by most quantitative research involves the effective control of confounding variables; that discourse analysis, in its usual forms, can avoid reliance upon a correspondence theory of truth; and so on.²⁵

CONCLUSION

In this chapter I began with a brief history of social research methodology and outlined the diverse forms it takes today, distinguishing between methodology-as-technique, methodology-as-philosophy, and methodology-as-autobiography. I showed how the emergence of the last two genres stemmed from the rise of qualitative work, and how this had been associated with ambivalence towards methodology, resulting in influential anti-methodological strands within the methodological literature.

In the face of understandable scepticism about the value of methodology, in the second half of the chapter I outlined my own views about its nature and function. I argued that its core must be the sort of awareness of, and reflection about, methods in which researchers should normally engage during the course of enquiry, but that there is also a need for specialist methodologists (who must themselves also be engaged in research or be close to it). I insisted on the value of all three genres, but also outlined some boundaries that ought to be placed around methodology. In the course of this I highlighted ways in which it had come to be overdeveloped: the tendency for methodology-as-technique to encourage proceduralism; the irrelevance and destructive character of some methodology-as-philosophy; and the

²⁵For discussion of these various issues, see Hammersley (1992a; 1995; 2009b).

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tendency for advocates of methodology-as-autobiography to downplay the role of general discussion of methodological issues and methods, and to forget the necessarily normative character of the task.

The answer to the question in my title is that we all need methodology: it is an essential component of social scientific research, both at the practical level and in more specialised terms. However, the methodological literature today suffers from serious distortions in relation to all three of the genres I discussed, and from a failure to learn from its past, leading to both repetition and a lack of coordination (Hammersley 2004d). This is unfortunate given that social science of most kinds has, in my view, been a good deal less successful than is frequently claimed by many social scientists; and that it faces fundamental methodological problems that require a great deal more attention, and more thoughtful investigation, than is usually recognised.