

# INTRODUCTION

The relationship between research, on the one side, and politics, policymaking and other forms of social practice, on the other, has long been a matter of public concern. Indeed, it has been the site of controversies and crises, with recurrent demands for social science to play a more direct role.<sup>1</sup> The most recent crisis, which is the background to this book, was generated by the rise of the evidence-based practice movement in medicine in the 1980s and 1990s, and its later extension to other fields, notably education, crime, and social welfare (see Gray 1997; Davies et al. 2000; Sackett et al. 2000; Trinder 2000; Welsh and Farrington 2001; McSherry et al. 2002; Sherman et al. 2002; Otto et al. 2009).

The idea that evidence should inform political and social practice can be traced back at least as far as Machiavelli, who believed that wisdom distilled from practical political experience and comparative historical analysis could greatly improve the decisions made by 'princes'. Of course, what has been proposed more recently differs significantly, both in the nature of what counts as knowledge and in the role that it is required to play. For example, in the 1960s, in the United States and elsewhere, the evaluation of new government programmes came to be treated as a central task of social science. This was conceptualised by Donald Campbell under the heading of 'the experimenting society', in which the effectiveness of all new policies and practices was to be scientifically tested.<sup>2</sup> Moreover, as Campbell's slogan makes clear, initially the proposal was that this should be done via experimental method (Cronbach 1979).

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<sup>1</sup>For a recent discussion of this issue in the context of criminology, see Loader and Sparks (2010). Nisbet and Broadfoot (1980) provide a history of recurrent debates in the field of education. Of course, the socio-political background to this issue has not been not unchanging. Maasen and Weingart (2005) sketch broad shifts that they label 'the democratisation of politics' and 'the politicisation of science'. These refer, respectively, to the growth in influence of political movements outside of the governmental system and governments' attempts to incorporate these, and to the ways in which researchers have been caught up in these developments and have also come to be involved in diverse institutions offering expert advice to governments and other audiences. In part, the second of these changes reflects a significant shift in the 'contract' between researchers and society, from a patronage model of funding to an investment model (see Guston and Keniston 1994; Demeritt 2000; Hammersley 2011).

<sup>2</sup>For his papers on this topic, see Campbell 1988b. See also Dunn 1998.

Later, methodological prescriptions were modified to allow quasi-experimental and other forms of quantitative evaluation. And subsequently all these forms of evaluation came to be criticised, on the grounds that they failed to measure key variables accurately, and did not take sufficient account of the unintended effects of policies and practices, both positive and negative. In the wake of this, various forms of qualitative evaluation were advocated, as providing a better understanding of policies, their implementation, and their results.<sup>3</sup>

In the 1990s the rise of the evidence-based practice movement involved renewed demands for experimental research that would directly inform policymaking and practice. The repercussions of this latest crisis are still being felt, and it has raised some important, albeit perennial, issues:

- How closely can and should social research be directed towards serving policymaking and practice, and of what kinds? What are the limits to the contribution it can make, in principle and in practice?
- Is there a hierarchy of research designs or methods as regards the likely validity of the findings they produce? If not, how are judgements to be made about what are better or worse methods for particular purposes?
- Is it possible to control and measure social variables? Is this a requirement in all kinds of research, at least if they are designed to inform policymaking and practice?
- Is the cumulative development of knowledge possible in the social sciences? Is it desirable? If so, what forms does it and could it take?
- What purposes do reviews of research literatures serve, and what character should they have? Is 'synthesis' the task, and if so what does this mean?

These are issues I will address in this book.

## EVIDENCE-BASED MEDICINE

At the core of the evidence-based medicine movement of the late 1980s and 1990s was the argument that the effectiveness of much clinical practice is unknown, and that in some cases standard treatments have been shown by research to be ineffective, and occasionally even damaging to patients (Cochrane 1972; Chalmers 2003). Given this, it was insisted that more research on clinical treatments was required (see Daly 2005), and that medical practitioners must make themselves familiar with the latest research evidence, and only employ those treatments whose effectiveness has been demonstrated.

The main model for the kind of research required to supply this evidence was the randomised controlled trials (RCTs) introduced to check the efficacy and side effects of new drugs (Marks 1997). The argument was that this kind of research – involving random allocation of patients to treatment or control groups, or to groups

<sup>3</sup>For outlines of these developments, see Shadish et al. 1991 and Pawson and Tilley 1997: ch. 1.



receiving different treatments, plus measurement of outcome variables across these groups – could be extended to other kinds of clinical treatment. The evidence-based medicine movement stimulated a considerable increase in the amount of research carried out on the effects of a wide variety of clinical practices.

Another important feature of evidence-based medicine was the argument that the results of single studies, even RCTs, are unreliable. Effective practice must be based upon systematic appraisal of *all* the relevant research evidence about the treatment concerned. This stimulated the development of a large body of ‘systematic reviews’, often involving statistical meta-analysis (see Chalmers et al. 2002). Many of these were developed and made available via the Cochrane Collaboration, an internationally funded network devoted to this task.<sup>4</sup> The rise of online databases was seen as greatly facilitating access to evidence by clinical practitioners and others.

Within medicine, evidence-based practice was presented as an enhanced form of professionalism, one that ensured that clinical interventions were based upon the latest and best scientific results. At the same time, it was also sometimes lauded as playing a ‘democratising’ role. One aspect of this was the idea that it subverted the dominant medical hierarchy, in which younger, more recently trained staff deferred to their elders, whose knowledge of the relevant scientific evidence was probably outdated, and who were less able to use modern forms of ICT to access the latest findings. Another, perhaps even more important, aspect of democratisation was that the latest scientific evidence would become available to patients, via the Internet, who could therefore evaluate the basis on which decisions about their treatment were being made (Oakley 2000). Here, the shift to evidence-based practice was presented as empowering patients in their dealings with doctors.

In summary, then, in its original or classic version, what counted as evidence was restricted to that coming from ‘scientific research’, this being treated as trumping all other sources of information, and especially that from practical experience. Furthermore, such research was defined as experimental in design and involved the rigorous measurement of variables, with RCTs as the ‘gold standard’. And the findings of multiple studies of this kind were to be synthesised via systematic review.

Another central assumption built into this classic model was that scientific evidence carries direct implications for practice that demand ‘implementation’. Thus, experimental research findings synthesised through systematic reviews were seen as producing information about effect sizes that demonstrates ‘what works’ (and what does not). And it came to be argued that all professional practice, and policymaking too, must operate solely upon this type of evidence.

A final key assumption was that when policymaking and practice are evidence-based in this manner, outcomes will be significantly improved. It is important to note that what is meant by ‘improved’ here is ‘made more effective’. The wider question of what are and are not desirable practical goals and means for policymaking and practice was largely taken for granted. This reflects a predominantly technical or instrumental orientation on the part of advocates of evidence-based practice.

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<sup>4</sup>For information about the Cochrane Collaboration, see [www.cochrane.org/](http://www.cochrane.org/).



## THE SPREAD OF 'EVIDENCE-BASED PRACTICE' TO OTHER AREAS

As noted earlier, the influence of the evidence-based practice movement spread out from medicine into other fields, notably social welfare, crime and justice, and education; though the main initial effect was, of course, on social scientific work in the area of health. One of the significant developments here was establishment of the Campbell Collaboration, which like the Cochrane Collaboration was devoted to generating systematic reviews.

The main focus of the evidence-based *medicine* movement had been upon changing the attitudes and practices of clinicians, encouraging them to make use of the increasing amount of scientific evidence about clinical effectiveness available. However, when the notion of evidence-based practice was extended to other areas, the focus often shifted significantly: there were challenges to what was seen as the inadequacy of existing research in serving evidence-based practice. In particular, it was pointed out that much social scientific work is not directly concerned with determining 'what works' in terms of policy or practice. Furthermore, it was claimed that much of it is insufficiently rigorous when judged against the standard of the RCT.

Literature reviews in social science were also criticised as falling short of the requirements of systematic review. For example, Oakley (2007: 96) declared that 'most literature reviews in social science are selective, opinionated and discursive rampages through literature which the reviewer happens to know about or can easily lay his or her hands on'. She also complains that, even when less haphazard than this, reviews do not usually indicate what search procedures have been used to find relevant literature, and that there is frequently a lack of clarity about how studies were judged to be relevant and how the validity of their findings has been assessed. A further complaint was that traditional reviews tend to go 'no further than a narrative synthesis' (p. 96), the contrast here being with what is offered by statistical meta-analysis. Oakley also argued that different reviews on the same topic have often covered different ranges of literature, with little overlap. She claims that this is the reason why, currently, reviews of research literature in social science often produce conflicting findings, which of course causes major problems for any policymaker or practitioner attempting to act in an evidence-based fashion.

Within many fields of social research, in the UK especially, the impact of the evidence-based practice movement occurred against the background of an earlier shift away from the use of quantitative method and towards reliance upon qualitative approaches. Not surprisingly, therefore, it was usually qualitative work that came to be subjected to most criticism by the proponents of research-for-evidence-based practice – though quantitative work that does not involve random allocation to treatment and control groups was also sometimes challenged (Chalmers 2003).

These criticisms of the deficiencies of existing research were often framed not just in methodological terms but also as complaints about an inadequate 'return' on public 'investment'. And there were sometimes appeals for external, government intervention to rectify the situation (see, for example, Hargreaves 1996). This reflected the fact that, in influential quarters, the notion of evidence-based practice had quickly become entangled with 'the new public management', a set of ideas about how the



public sector must be reorganised. This was a major influence on politicians and other policymakers, and on the media, in many Western societies from the 1990s onwards (Ferlie et al. 1996; Pollitt 1990; Clarke and Newman 1997; Mayne and Zapico-Goni 1997; Pollitt 1998; Lane 2000; Levy 2010). What was involved here was a demand for ‘transparent’ accountability on the part of those professional occupations that formed part of the public sector, from doctors and nurses to teachers, social workers and probation officers. It was believed that requiring the work of these professionals to be explicitly based upon research evidence about ‘what works’ would make them accountable, and thereby increase their effectiveness – with ‘cost-effectiveness’ increasingly being brought into the calculation.

## THE CRITICAL CASE OF EDUCATION

While the evidence-based practice movement affected several social science areas, the one where it probably had the most dramatic impact was education. In the UK, the first major sign of what was to come was a lecture by David Hargreaves in 1996, sponsored by the Teacher Training Agency, in which he criticised educational research for failing to provide the kind of evidence that is needed for evidence-based practice. The requirement he laid down was that it should demonstrate ‘conclusively’ that some change in practice leads to a ‘significant and enduring improvement in teaching and learning’ (Hargreaves 1996: 5). In making this critique, Hargreaves held up as a model what he saw as the very different situation in medicine.<sup>5</sup>

Subsequent to Hargreaves’ lecture, both the Office for Standards in Education (Ofsted) and what was then the Department for Education and Employment (DfEE) set up inquiries into educational research, and both these reported in 1998. In a brief introduction to the first of these reports, Chris Woodhead, then Chief Inspector of Schools, declared that much educational research is ‘on this analysis, at best no more than an irrelevance and a distraction’ (Tooley 1998: 1). Furthermore, in the press release for the report (which was headed ‘Majority of academic educational research is second-rate’) he suggested that ‘considerable sums of public money are being pumped into research of dubious quality and little value’. The DfEE-sponsored Hillage Report also raised questions about the quality, and especially about the usefulness, of much educational research, suggesting both that it should be more policy- and practice-relevant and that government ministers and policymakers needed to take more notice of research evidence (Hillage et al. 1998). These two critical reports on educational research were followed by a government statement about what needed to be done to remedy the situation. In the words of Charles Clarke, then Parliamentary Under-Secretary of State at the DfEE, the aim was to ‘resurrect educational research in order to raise standards’ (Clarke 1998; emphasis added).

In the wake of these developments, the UK government instituted various policies designed to reform this field of inquiry. One was the establishment of a National Forum for Educational Research, whose task was to facilitate the identification of

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<sup>5</sup>For a detailed assessment of Hargreaves’ argument, see Hammersley 2002: ch 1.



research priorities, to specify quality standards in the field, and to maximise the impact of research on policymaking and practice. Another initiative was the creation of the Economic and Social Research Council's (ESRC) Teaching and Learning Research Programme, which for several years became the conduit for the bulk of external funding for research on education (see Christie and Pollard 2009). A 'research capacity building' arm of this programme was also set up, designed to up-skill education researchers, particularly in the area of quantitative techniques. Equally important, the Evidence for Policy and Practice Information Co-ordinating Centre (EPPI-Centre) was established at the Institute for Education, University of London, in order to facilitate the production of systematic reviews of available research. These reviews were aimed at policymakers and practitioners, so that they could determine which school policies or pedagogical techniques are effective, and thereby improve the performance of the British education system. Equally important was the hope that the production of systematic reviews would reshape the form and character of educational research in the future, so that it would more closely meet the requirements of policymaking and practice.<sup>6</sup> There were also some schemes designed to facilitate schoolteachers doing research (this had been one of Hargreaves' recommendations) and to disseminate research findings across the profession.<sup>7</sup>

The connection with the 'new public management' was important here. This promoted the view that the primary role of the education system is to facilitate national economic growth and competitiveness. As Alison Wolf points out in her book *Does Education Matter?*:

Politicians' faith in education is fuelled by a set of clichés about the nature of the twenty-first-century world: globalized, competitive, experiencing ever faster rates of technical change. In this world, it seems, education is to be a precondition of economic success, and indeed survival, to an even greater degree than in the century before. (Wolf 2002: xi)<sup>8</sup>

And these ideas were extended beyond schooling to universities and the research that takes place within them. In the words of one of the politicians centrally involved in the crisis, universities should be aiming to 'turn ideas into successful businesses' (Clarke, in Department for Education and Skills 2003). Along with this came the demand that university researchers maximise the 'impact' of their work, this being *measured* in order to document the 'return' on the investment made by funding bodies.

<sup>6</sup>On what is required from research syntheses in the context of policymaking, see Davies 2006.

<sup>7</sup>This continues today, for example via the Centre for the Use of Research and Evidence in Education: see [www.curee.co.uk/home](http://www.curee.co.uk/home). See also Evidence-Based Education UK (EBE Network). See <http://www.cemcentre.org>. For a similar venture in the United States, see the What Works Clearinghouse: <http://ies.ed.gov/ncee/wwc>. See Foster 1999 for an assessment of the quality of some of the research stimulated by some of these schemes. In fact, there had been practitioners of action research movements within the field of education preceding the rise of evidence-based practice. See Chapter 7.

<sup>8</sup>Wolf mounts a cogent challenge to these assumptions.



The field of education also took the brunt of the criticism in the United States. An early signal there was the publication of a book by the then Secretary of Education which purported to determine ‘what works’ in teaching and learning (Bennett 1986; see Glass 1987). In 1999, the Reading Excellence Act was passed, specifying the sort of ‘scientifically-based’ research upon which recommendations for teaching reading should be based. In 2000 there was a request from the National Educational Research Policy and Priorities Board to the National Research Council to set up a committee to examine what constitutes ‘scientifically-based education research’. Its report was published in 2000, defining this primarily from within the methodological framework of quantitative method – though it recognised the value of qualitative work under this heading, as well as acknowledging that of ‘non-scientific’ forms of inquiry in the field of education (see Feuer et al. 2002 and National Research Council 2002).

Subsequently, however, the reauthorisation of the Elementary and Secondary Education Act (‘No Child Left Behind’) defined ‘scientifically-based research’, in other words that which could receive Federal funding, more narrowly as involving hypothesis-testing through experimental and quasi-experimental designs, with a preference for random allocation to treatment and control groups. Furthermore, the Department of Education’s 2002–7 strategic plan was published, which specified the goal that 75% of its funded research addressing causal hypotheses should use random assignment by 2004. There was also an Education Sciences Reform Act in 2002, designed to set up an institutional framework aimed at ensuring that research serves evidence-based policymaking and practice; here, however, a slightly broader definition of what counts as ‘scientific’ educational research was adopted than that of the 2001 Act (Eisenhart and Towne 2003).

In this political climate, some members of the educational research community sought to promote randomised controlled trials and systematic reviews (Mosteller and Boruch 2002; Slavin 2002, 2004). Others, especially qualitative researchers, mounted a vigorous critique.<sup>9</sup> Many denounced what they saw as a ‘new orthodoxy’ (Hodkinson 2004) and dismissed not just its definition of what counts as scientific research, and the priority given to this, but also its conception of the contribution that research can and should make to policymaking and practice.

## LIBERALISATION WITHIN LIMITS

Over time, and not least as a result of the move into social science, the notion of research for evidence-based practice came to be liberalised in important respects. One of these concerned what counts as scientific evidence. It was acknowledged that other kinds of research besides RCTs can be of value, though sometimes the

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<sup>9</sup>For examples, see Erickson and Gutierrez 2002; St Pierre 2002; Atkinson 2004; Lather 2004; Lincoln and Cannella 2004; Maxwell 2004; MacLure 2005; Ryan and Hood 2004; Denzin and Giardina 2006; Eisenhart 2006; St. Pierre 2006. See also Cook 2001, Biesta 2007, Donaldson et al. 2009, and St Clair 2009.



relaxation was quite limited and grudging. For example, addressing the issue of what types of research should be included in systematic reviews, Farrington and Welsh (2001: 9) state that:

In the case of criminology and criminal justice, this means experimental (randomized and nonrandomized) and quasi-experimental designs. Ideally we would have been able to limit studies in systematic reviews to only those that used randomized experimental designs, as this is the most convincing method of evaluating crime prevention programs (Farrington 1983). However, for systematic reviews of criminological interventions, this is rarely feasible ...

A more liberal approach was generally adopted in the field of education, with qualitative work increasingly being included as a source of evidence. Nevertheless, it was often seen as playing a subordinate role, for example providing information about the perspectives of those on the receiving end of policies or programmes (see Harden 2006). Furthermore, the narrow conception of the purpose of research characteristic of the evidence-based practice movement – as being concerned with ‘what works’ – was retained. This first aspect of liberalisation prompted various efforts to specify criteria by which qualitative research should be judged, for inclusion in systematic reviews and more generally (Spencer et al. 2003; see Chapter 7), and also the development of procedures for qualitative synthesis (see Chapter 10).

A second area of liberalisation involved increasingly explicit recognition that any form of practice necessarily involves the exercise of interpretation and judgement, rather than simply the ‘application’ of research findings. This was reflected in a shift in terminology from *evidence-based* to *evidence-informed* practice. Once again, though, this was a slackening of the constraints imposed by the original, classical model, rather than a substantial change in its character. Indeed, sometimes this liberalisation was not much more than window-dressing, with advocates oscillating between classic and more liberal versions.<sup>10</sup> Furthermore, within policy circles, some research evidence continued to be treated as if it were demonstrably valid and carried direct instructions to practitioners. Thus, in the field of education it came to be accepted as conventional wisdom that teaching reading via phonics had been demonstrated by RCTs to be the most effective pedagogical strategy, even though the evidence does not support this: it shows that ‘systematic phonics’ increases ‘reading accuracy’ but tells us little about its effect on reading comprehension. Moreover, there are important caveats about the reliability of this evidence (Torgerson et al. 2006: 46–7).

Liberalisation reduced the areas of disagreement between advocates of evidence-informed practice and many of their opponents. In doing this, however, it drained the original idea of its radical distinctiveness: the resulting position does not differ significantly from earlier calls for social research to be more policy- and practice-relevant, or indeed from the commitment of many social scientists to maximise the

<sup>10</sup>Pawson (2006: viii) describes the phrase ‘evidence-informed’ as ‘thin-lipped, prissy and politically correct’.



impact of their work. A further important consequence of this was that liberalisation reduced the visibility of important issues that the classic model had highlighted.

One of these concerned whether there is a sharp, and hierarchical, distinction to be drawn between the knowledge produced by research (of some specified kind) and ‘spontaneous’ understandings generated by practical engagement with the world. The classic model of evidence-based practice treats research as providing definitive knowledge whereas practical experience is portrayed as thoroughly unreliable. A second issue raised by this model is whether the knowledge produced by research is limited to factual knowledge about some delimited realm, or can provide a comprehensive basis for action that can replace unreliable lay understandings, and lead to the improvement or even transformation of policymaking and practice. Here questions about the relationship between expertise, technique and values are involved.

It is worth noting that these two issues relate to central themes of Enlightenment thought, and in broad terms can be traced even further back, for example to Plato’s *Republic*. In the nineteenth century they were developed into ‘grand’ conceptions of the role of social science, along divergent lines initially mapped out by Comte and Marx, and these continued to be influential well into the twentieth century (Hammersley 1999). From this perspective, the social scientist should be a public or organic intellectual (Hammersley 2011: ch. 2), if not a ‘legislator’ (Bauman 1987).

Such views, at least implicitly, underpin social scientists’ frequent complaints that their research findings have been ignored by policymakers or practitioners, or that current policies or practices fly in the face of these findings. Indeed, those views often motivate even the strongest opponents of the sort of positivist conception of social science that characterises the evidence-based practice movement. Thus, most varieties of ‘critical’ research – whether influenced by Marxism, feminism, or anti-racism – present themselves as supplying conclusions about what is wrong, and what should be done, that are superior to those of ‘commonsense’; in fact, the latter is frequently dismissed as ideological. In these terms, researchers may be portrayed as dispensing reason, where others are preoccupied with instrumental concerns, values, emotions, and political imperatives (Garland and Sparks 2000: 19). Of course, there is a major difference here in what is taken to be the source of knowledge and how its validity is grounded (systematic theory or a critical perspective rather than ‘scientific’ research design), as well as in the character of the implications drawn (major social change rather than ‘piecemeal social engineering’). Nevertheless, in its fundamentals, a grand conception of research is widely shared among social scientists: it is often assumed that the knowledge they produce can generate conclusions that should replace or correct the practical knowledge of actors, and that this will bring about substantial improvement in the world. At this most fundamental level, there is continuity between arguments for evidence-based practice and much of the rest of social science, despite other important differences.<sup>11</sup>

<sup>11</sup>Of course, in the last decades of the twentieth century the contrasts between reason and unreason, knowledge and opinion, etc., came under challenge from that heterogeneous range of ideas labelled ‘post-structuralism’ or ‘postmodernism’. It is important to point out, though, that this involved re-inscribing the distinction between knowledge and opinion in sceptical terms, so that now the contrast was between a select intellectual elite who know that all value, meaning and knowledge are arbitrary and uncertain, and the rest of us who do not.



In many ways it is this grand conception of the role of social science that is being challenged in the chapters that follow. One of my central concerns is limits on the capabilities of research: what is it able to produce, and what is the relationship between this and the demands of policymaking and practice? I argue that the evidence-based practice model greatly exaggerates the current capacities of research, and involves naïve assumptions about the nature of policymaking and practice. But it is not alone in this.

## DIVERSITY IN RESEARCH AND PRACTICE

Both social research and policymaking/practice can take diverse forms, whose character and requirements may differ significantly. In the case of research, we need to distinguish between academic and practical forms, the first being geared to building knowledge in a disciplinary field, the second to addressing some specific issue with a view to supplying relevant information to lay audiences concerned with it (Hammersley 2002: ch. 6). And both these types of research must be differentiated from what I refer to in Chapter 7 as inquiry-subordinated-to-another-activity.

The status and value of *academic* research has come under severe challenge as a result of the rise of the evidence-based practice movement, with its prioritising of a particular kind of applied work. This has been exacerbated by the emergence of the ‘new public management’, and the way that this has shaped research funding and the internal organisation of universities (Collini 2012). Despite recurrent acknowledgement of the importance of ‘blue skies’ research, the predominant emphasis has been on the need for research that assesses the ‘effectiveness’ of policies and practices.<sup>12</sup> Given this, it is very important to emphasise the distinctive character and value of academic research (Hammersley 2011), and this requires us to challenge the economic rhetoric that currently dominates policy talk, and even infuses discussions about the ‘strategic planning of research’ within universities.

Just as there are different kinds of research, so too there are many forms of practice, and these can vary considerably in how they use research findings. They include:

- 1 Individual consumers or service users who are faced with personal decisions to which social science findings might be relevant.
- 2 Occupational practitioners and organisational managers who, in the course of their work, must make decisions, adopt or devise strategies for dealing with problems etc., and may turn to social science for help. The issue here could be the effectiveness of strategies available to them, but research can supply other specific kinds of information, offer a new perspective on the situation faced, or provide background information that allows for better decisions to be made.

<sup>12</sup>There are, in any case, important differences between blue skies research and academic inquiry. Furthermore, while the first is almost always a low priority for policymakers in practical terms, the second is either off the radar or is itself a target of abuse: see Hammersley 2000a.



- 3 Policymakers in governments, and politicians, may draw on social science in the two ways already mentioned. However, they may also be interested in using social science as a source of ammunition for promoting, protecting, or challenging particular policies.
- 4 Citizens within polities that provide some scope for public participation in policy formation may draw on research findings relevant to the issues in which they are interested. They may be concerned with coming to an informed judgement about what is wrong and what should be done, but they may also often seek to use research as ammunition.
- 5 Interest groups involved in the policy process will also have diverse attitudes towards research evidence, and like politicians they may be locked into particular positions that they feel compelled to promote or defend.

It is also of significance that any use made of social science findings takes place not only in the context of the availability of information from other sources, but also within fields where ideological viewpoints of various kinds are in play, under the influence of conflicting interests. These viewpoints may not only represent particular issues as having the highest priority but also carry frameworks for understanding them that favour accepting the validity of some research findings while discouraging acceptance of others (Weiss 1983). In other words, in late modern Western societies the contexts within which individuals, citizens, practitioners, policymakers and others operate are often ones in which a diverse range of agents is promoting particular views about what are important problems, how they should be understood, how they ought to be dealt with, and so on. And many of these agents label what they offer as the products of research.

So, all use of social science findings today operates in very ‘noisy’ and conflictual environments.<sup>13</sup> The discourse that takes place in these is, almost always, very different from the sort of rational dialogue that has often been seen as the ideal in academic discussion, and that some proponents of deliberative democracy believe could operate within the public sphere. What this highlights is the importance of being realistic about the nature of the world in which research, policymaking and practice now operate (Geuss 2008; Swift and White 2008). Such realism is not, I suggest, commonly found in advocacy of evidence-based policymaking and practice, or for that matter in much discussion by social scientists of the political or practical implications of their work.

We should also recognise that policymaking, while itself a form of practice, is institutionally distinct from the various kinds of occupational practice to which it relates. Thus, there are important differences, and complex relationships and tensions, between them. A striking feature of the last couple of decades is the way that research has often come to be entangled in these relationships. Thus, in the context of the ‘new public management’, research has frequently been used as a means of challenging practitioner claims to expertise, thereby extending policymakers’ control, and

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<sup>13</sup>Loader and Sparks (2010: 3) note how policymaking in crime and justice has ‘heated up’, coming to be dominated by ‘punitive passions and short-term political calculation’. There has also been a heating of some other policy areas, including those of social work and education.





subjecting various occupations in the public sector to new regimes of monitoring purportedly based upon research knowledge. In this manner, many researchers have become implicated in the currently dominant neoliberal model of policymaking (see Steger and Roy 2010). Whatever one's political assessment of this, it is not hard to recognise that it has been consequential for social science.

## OVERVIEW OF CHAPTERS

Each essay in this book is designed to stand on its own, despite the interconnections among the issues discussed. As a result, there is occasionally some overlap, but generally the chapters traverse the terrain via different routes, so that where the same ground is covered it will usually be viewed from a somewhat different angle.

The first chapter outlines the key assumptions underpinning the notion of evidence-based practice in its classical form, and the role that research is required to play in enabling it. I begin by noting the radical claims advocates of this notion frequently made, and their reliance upon rhetorical ploys: the very name 'evidence-based practice' seems to dismiss what it excludes, treating this as irrational – who would deny the legitimate role of evidence in policymaking and practice? The chapter goes on to examine how certain kinds of research are privileged – those employing randomised control trials or quantitative methods more generally – and how research evidence is valued above other sources of knowledge, notably professional experience. I argue that there are features of research-based knowledge that count against the role that the notion of evidence-based practice requires it to play. Equally important, I show that the transmission of research evidence to policymakers and practitioners, and their use of it, are much more difficult and complex processes than is typically assumed. Finally, I examine the rise of the 'new public management' and its role in fuelling the influence of 'evidence-based practice'. I spell out why the claims to 'transparency' on which managerialism relies are false, noting the distortions that an overemphasis on 'objective' indicators introduces into policymaking and forms of occupational practice.

Chapter 2 explores how arguments about research for evidence-based practice relate to recurrent debates about both the 'failure' of research to serve policymaking and practice *and* the 'failure' of policymakers and practitioners to make proper use of research findings. The assumptions underpinning the idea of research-based practice are spelt out in more detailed terms here, and doubts about them identified. Central are the widely held ideas that research can supply all the knowledge required by practitioners and policymakers, and that research-based policies and practices will greatly improve outcomes.

The next chapter focuses upon the way in which a particular image of scientific research is central to arguments about evidence-based practice. The claim is not just that policymaking and practice should rely primarily or exclusively upon the best scientific evidence, but also that they should adopt a mode of rationality that is taken to be characteristic of science, in which conclusions are derived from evidence, via some highly determinate means, such as deduction or calculation. I examine the assumptions involved here, noting that they derive from a simplified version of the kind of positivism influential in the philosophy of science in the first half of the





twentieth century. I argue that one does not need to adopt the more extreme arguments within the philosophy and sociology of science to recognise that scientific work necessarily relies upon tacit knowledge and judgment, and is therefore very similar in form to the kind of occupational practice that advocates of evidence-based practice criticise and seek to reform.

Chapter 4 begins by looking at the nature of evidence, and in particular at how what counts as evidence is dependent upon the particular question being addressed, as well as on the level of reliability required. I then examine the arguments proposing the superiority of the evidence from randomised controlled trials, suggesting that this relies upon erroneous methodological assumptions. The second half of the chapter points out that policymaking and practice cannot be ‘based on’ research evidence, that they necessarily involve experience and judgment and therefore demand expertise that has the character of phronesis.

A central assumption of the evidence-based practice model is that, in order to provide sound evidence, research must *measure* outcome variables. This has not usually been given the attention it deserves by advocates of evidence-based practice. Critics of the evidence-based practice model have, of course, often denied that the sort of measurement claimed is possible, and many qualitative researchers typically dismiss the whole concern with measurement as relying upon a false positivism. In Chapter 5 I argue that achieving accurate social measurement is a very demanding task, and I explore the reasons for this. At the same time, I point out that, interpreted in broad terms as ‘linking concepts to data’, measurement is a problem that arises for qualitative inquiry just as much as it does for quantitative research.

Chapter 6 is concerned with an issue that has been given a great deal of attention in the wake of the evidence-based practice movement: the question of the criteria by which qualitative research should be judged. I examine whether such criteria are possible or desirable. I also explore how the application of any criteria necessarily relies upon background knowledge and expertise, and the problems that this generates for policymakers and practitioners making use of research evidence. Finally, I suggest that there are fundamental divisions within qualitative research today, centred on notions of rigour and also deriving from constructionism and what I call activism, that need to be resolved before there could be any agreement about what would be relevant criteria of assessment; and it is unclear how these can be overcome.

As I noted earlier, the idea that research should have a direct relationship to practical or political activities is by no means restricted to the evidence-based practice movement. In Chapter 7 I consider one of the most prominent examples of this idea, action research, which has been presented by some as a component of evidence-based practice (Hargreaves 1999) but by others as an alternative to it (Elliott 2001). I explore its rationale against the background of older views that tended to privilege *theoria* over *praxis*. I accept the pragmatist notion that all inquiry arises out of human activity, but not the instrumentalism frequently associated with it. I suggest that, even in everyday life, inquiry is often prompted not by some practical problem but by intellectual puzzlement. I propose that inquiry must be treated as operating on the same plane as any other activity, but that the relationship between it and other activities will always be less than isomorphic, and that this creates the prospect of



severe tensions. These can be managed contextually in two ways: either by subordinating inquiry to the other activity, or by making it primary and insulating it to a considerable degree from external demands. Both strategies are legitimate, but any attempt simultaneously to treat inquiry and some other activity as equal priorities, as in the case of much action research and some forms of research for evidence-based practice, faces contradiction.

As we saw, the production of ‘systematic’ reviews of research findings was integral to the demands made on researchers by the evidence-based practice movement. Such reviews synthesise the findings from multiple research studies, relying upon exhaustive search procedures and explicit evaluation of the ‘quality’ of the evidence provided. Chapter 8 examines the assumptions about research, and about the task of reviewing, which are built into the concept of systematic review, suggesting that these are, in important respects, false.

This theme is continued in the next chapter, which focuses on what it means for a review to be ‘systematic’: that it involves synthesis, is issue- or remedy-focused, and is ‘comprehensive’ and ‘transparent’. I argue that these features are far from straightforward, and not always of value. Moreover, the attempt to achieve them can have negative effects on the quality of the review produced. Furthermore, I suggest that the currently influential contrast between systematic and traditional reviews obscures important issues, in particular the art and politics of producing reviews for lay audiences. My conclusion is that we need a more complex typology: one that takes into account the various functions, and kinds of audience, that can be served by literature reviews; and one that does not carry an obfuscating evaluative load, in the way that ‘systematic/unsystematic’ does.

In Chapter 10 I evaluate a very different critique of ‘traditional’ reviews from that presented by advocates of evidence-based practice, and one that leads to contrasting recommendations. This comes from some qualitative researchers, leading to advocacy of various forms of ‘interpretive review’. They treat traditional forms of reviewing as incompatible with the principles of qualitative research. It is suggested that the *aim* of research should be to ‘surprise’ the reader, to ‘challenge’ perceptual habits, or to ‘recast’ social relations. These critics abandon any idea that reviews should be designed to contribute to the cumulative development of knowledge. The chapter begins by outlining the arguments behind this critique, and then subjects them to critical assessment.

In the final chapter I examine the idea of qualitative synthesis, which was stimulated, in large part, by the development of meta-analysis and systematic reviewing. I explore a number of issues here: what ‘synthesis’ means in this context, and how qualitative synthesis differs from primary research and from traditional reviewing. A range of methods have been proposed for qualitative synthesis, and I look at two basic forms: that which seeks to apply the techniques of grounded theorising; and meta-ethnography, as proposed by Noblit and Hare (1988) and developed by others. I conclude that what is done under these headings does not differ significantly from many traditional reviews. Nevertheless, there are benefits to be gained from the idea of qualitative synthesis, in so far as it encourages careful, comparative reading and assessment of the literature, with a view to clarifying and developing the current state of research knowledge.