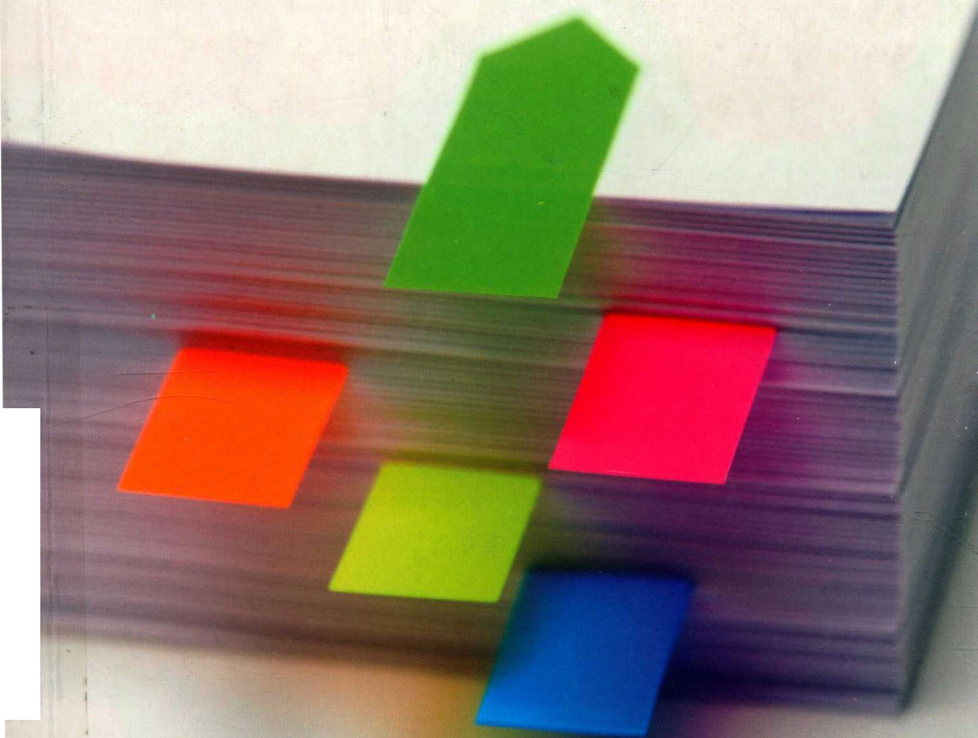


Richard Pring

Philosophy of Educational Research

THIRD EDITION



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- the nature of educational enquiry
- the links between research, policy and practice

In analyzing and interrelating these themes, Richard Pring shows their relationship to central philosophical concepts such as meaning, truth, and objectivity.

RICHARD PRING is Emeritus Professor at the Department of Education and Emeritus Fellow of Green Templeton College, University of Oxford, UK.

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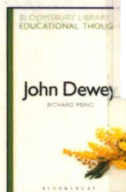
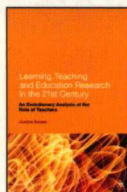
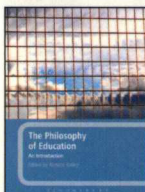
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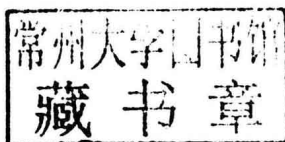
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Foreword to Third Edition

This book was first published in 2000. It was changed a little for the second edition in 2004. But it is still in demand from those who are engaged in educational research, preparing theses, or seeking a research-based approach to educational practice. It is now being used in courses for health care workers. Therefore, what was written 14 years ago needs to be 'upgraded'.

That 'upgrading', however, does not lie in a serious shift in the philosophical basis of the book. Philosophical foundations, if articulated with care, do not change that radically. However, the contexts, to which the arguments are relevant, do change, and these need to be made explicit to today's readers. For example,

- The UK Research Assessment Exercises (RAE, now called Research Excellence Framework or REF) of 1986 and 1989 have grown in number (1992, 1996, 2001, 2004, 2009, 2013). Though barely mentioned in earlier editions, they are clearly significant in defining research quality and decisions upon research funding. This is the case also in Australia and New Zealand. Countries in Europe are moving in that direction, too. But such ranking of research raises philosophical questions about the definition of *quality*. That requires close examination, which is given in a new Chapter 9.
- The four-yearly PISA comparisons of standards in language, mathematics and science across OECD countries, culminating in international league-tables, have become politically important. They raise questions about the meaning of *standard*, crucial to policy and practice. This received little attention earlier, but is now in a new Chapter 11.

- Criticisms of research, identified in previous editions, are updated partly in Chapter 1, but *passim*, as they have affected public support for research.
- There is an increasing interest in 'comparative research' and in what is referred to as 'policy borrowing'. Therefore, thoughts on that have been added to Chapter 11.
- Previous editions failed to deal with pragmatism, a foundation for 'meaning' and 'truth' affecting theory and research, especially in America, from Dewey onwards. This requires expansion of Chapter 8, but with implications elsewhere for 'truth' and 'action research'.
- More emphasis is given, in Chapter 10, on the relevant 'virtues' of the researcher.
- Developments in educational policy have often appealed to relevant research, ('what works'). A new chapter 11 reflects on the relationship between research and policy, and the nature of the research community and of its relation to those with power.
- The issues are of interest to medical education – both in the education of doctors and in the frequent reference to the model of medical research to education. Hence, throughout the relevance and yet dissimilarities are referred to, but especially in Chapters 4 and 5.
- Finally, Chapter 9 on the Quality of educational research spells out key principles in doing good research based on the philosophical considerations of this book. This, I hope, should be of interest to those embarking on doctoral theses.

Perennial philosophical issues underpin educational deliberations about practice, policy, and therefore research. Such issues concern the nature and accessibility of knowledge, what it means to be and behave 'as a person', the basis of values we think worth pursuing, the relationship between mind and body and between the individual and society. Such issues, though constantly reformulated, will ever remain central to thinking about education, to the conduct of research, and thus to this third edition.

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1

Setting the scene: criticisms of educational research

Criticisms

Until the 1970s, public financing of UK universities assumed a close, if not essential, relationship between teaching and research. Therefore, a substantial proportion of university income (approximately one-third) supported that research. Then came a change of policy. Was all that research worth supporting?

Many believed that this money was not well spent, and this was reflected in Britain (with respect to educational research) in the 'Hillage Report' (1998), sponsored by the Department for Education and Employment. It argued that research

- does not provide answers to the questions Government asks in order to decide between alternative policies on 'what works';
- does not help teachers in their professional practice (e.g. in the most appropriate teaching methods);
- is fragmented – lots of bits and pieces which, though often addressing similar questions, start from different positions or use different samples, not creating a coherent and reliable basis for practice or policy; and

- is often tendentious or politically motivated – and exclusive of those who do not share the ideological underpinnings of the research programme (Tooley and Darby, 1998).

Such criticisms were emerging in the political arena some years before. This is illustrated by the contribution of Lord Skidelsky to the debate in the House of Lords concerning the proposal to transfer responsibility for the funding of educational research in the United Kingdom from the Higher Education Funding Council to the Teacher Training Agency.

Many of the fruits of that research I would describe as an uncontrolled growth of theory, an excessive emphasis on what is called the context in which teaching takes place, which is code for class, gender and ethnic issues, and an extreme paucity of testable hypotheses about what works and does not work. (Quoted by Bassey, 1995, p. 33, together with his excellent response in *The Times Educational Supplement*)

This sceptical attitude towards education research was by no means confined to teachers and policy-makers, or indeed to Britain. Criticisms emerged from within the educational research community itself. Hargreaves (1996), drawing upon a North American critique of educational research and his own Leverhulme-funded project, argued that, despite the enormous amount of money spent on research and the large number of people who claim to be active researchers, there was not the cumulative body of relevant knowledge which would enable teaching to be (like medicine) a research-based profession. For it to be so, it would be necessary to change, first, the content of that research, and, second, the control and sponsorship of it.

Content would need to focus on the practice of teaching and learning in order to build up sufficient, well-tested bodies of knowledge to serve as guidelines for professional practice in, say, the teaching of reading or in the grouping of pupils in classrooms. Of course, such a corpus of knowledge would be complex and would need to be used flexibly because situations, context and personalities of both teachers and learners affect the relevance of the research conclusions. None the less, such a research exercise

would seem possible. Teachers would need to be involved (as doctors are in the development of research-based medicine) in identifying research needs, in formulating the questions which respond to these needs and in collecting the data to make it 'rooted firmly in the day-to-day professional practices'. The relationship between 'professional researchers' and teachers would be more integrated in the setting of agendas and in the undertaking of the research. This was reiterating what Stenhouse (1975, chapter 10) had argued, namely, that only the teachers could appreciate, and have access to, the complexity of data required to understand the interactions of the classroom.

This concern about the quality and relevance of educational research was not a peculiarly British phenomenon. Hargreaves quoted Lortie (1975) as saying of the United States,

Teaching has not been subjected to the sustained, empirical and practice-oriented inquiry into problems and alternatives which we find in university based professions . . . [T]o an astonishing degree the beginner in teaching must start afresh, uninformed about prior solutions and alternative approaches to recurring problems . . . (3)

The issues were thoroughly discussed in the pages of *Educational Researcher* (see Slavin 2002). Carl Kaestle (1993) asked the question, 'Why is the reputation of educational research so awful?' In a collection of papers addressing these matters, Goodlad put the problem bluntly:

Criticism of educational research and statements regarding its unworthiness are commonplace in the halls of power and commerce, in the public marketplace, and even among large numbers of educators who work in our schools. Indeed, there is considerable advocacy for the elimination of the locus of most educational research – namely, schools, colleges and departments of education. (Berliner et al., 1997, p. 13)

But the reasons seemed to lie not so much in the lack of an adequate knowledge base. Indeed, Gage dismissed those critics who said

that research had not provided the well-tested generalizations which can inform practice. But he did take researchers to task for their failure to develop an adequate theoretical framework within which well-established research might be brought to bear upon educational understanding and practice. There was a need for the 'meta-analyses' of existing research to meet the needs of those who wanted to know the evidence for supporting one policy rather than another, or one educational practice rather than an alternative (Cooley, Gage and Scriven, 1997). Berliner draws a similar conclusion: there was the body of knowledge, but it was not synthesized in a way which could relate to teachers' administrators and politicians.

Scriven, however, was much more censorious, declaring that educational research did not in itself contribute its 'principal duty to the society'. Indeed, using the medical reference again,

If medical research had only contributed explanations of disease but had neither identified nor developed any successful treatments, we would surely say that it had failed in its principal duty. (Cooley, Gage and Scriven, 1997, p. 20)

In modern parlance, educational research was conducted without reference to possible or likely 'impact'.

The debate was best summarized by Kennedy's (1997) contribution, referred to by Hillage. This gives four reasons for the failure of research to have impact on educational practice:

- its seeming irrelevance to practice;
- its poor quality, compared with research in other fields (particularly social sciences);
- its inaccessibility; and
- incapacity of the educational system to make decisions on the basis of research.

This US debate was summed up by Lagemann (1999, p. 3) as follows:

It has to be acknowledged that studies of education tend to 'get no respect'. Indeed, no other areas of scholarship have ever

been more scorned and demeaned. Educational research has been accused of ignoring important questions while reinforcing practices that stand in the way of fundamental reform. It has never been a professionally dominated field of study and as a discipline has been internally fragmented.

The consequences of these criticisms are of two kinds. On the one hand, much research is dismissed as worthless, not deserving funding. Its quality and relevance are challenged. With its own peculiar language (often technical, abstract and obscure), the research is seen to have gone adrift from the complex, but common sense and practical world of teachers.

On the other hand, though considered not altogether worthless, research is seen to need greater external control so that it is made to serve the purposes and answer the questions of the non-research communities – politicians, administrators and teachers.

Consequently, there have been initiatives, both in the United Kingdom and in North America, to learn from the developments in evidence-based health care and, through systematic reviews of research (especially randomized controlled tests), to answer specific policy and professional questions by reference to well-established evidence (see the series of papers in Thomas and Pring, 2004). It is, of course, part of the role of a philosophical study to examine and to question the relevance of medical and social science 'models' of research to the area of education. This book will do that. But the questioning of the value of much research in general and of educational research in particular has given rise to that change of policy, referred to at the beginning, namely, the regular assessment of the quality of research as a basis for the distribution of research funds, which is explained below.

Meanwhile, to answer these criticisms would require a detailed account of research over the years and an analysis of its impact upon policy and practice (see Kirst and Ravitch, 1991, for illustration of US policies which have changed as a result of research and Edwards, 2000, for similar examples from Britain). As Hammersley (1997) argued, there is a danger in looking for a too direct relation between research conclusions and specific rules for successful practice. Human beings (and the social life in which they interact) are not

the sort of things where there can be simple causal relationships between specific interventions and subsequent behaviours. And this affects the possibility of 'cumulative knowledge'. Again, the impact of research may be indirect, a gradual shifting of public and professional consciousness in the light of growing evidence, rather than a direct relation of conclusions to practical decisions. On the other hand, the impact has been direct in certain policy decisions such as in the establishment of Educational Priority Areas, following the research of Halsey on the relation of educational performance to social conditions (Halsey, 1972). Another example would be the effect of research about school effectiveness on both policy and practice, beginning with the Rutter Report (1979) and Mortimore and Sammons (1997) and also Mortimore (1999). Black and Wiliam's (1998) 'Assessment for Learning Group', beginning with *Inside the Black Box: Raising Standards through Classroom Assessment*, created a base for both further research and professional practice. One might also cite the way in which research entered into major curriculum development projects, shaping their direction and thereby influencing rather than determining the practice of the teachers. An example would be the research into the influential Humanities Curriculum Project or Nuffield Science (Elliott and MacDonald, 1975).

None the less, possibly in response to these criticisms, the Economic and Social Research Council (ESRC) funded a United Kingdom's largest ever research investment in education, directed by Andrew Pollard at the Institute of Education, namely 'Teaching and Learning Programmes 2002-9'. It addressed many of the criticisms referred to above, in particular (see Briefing Paper 80) 'quality criteria for the assessment of educational research in different contexts'. An account of research bearing upon educational practice is a constant reference point for the philosophical issues raised in this book.

However, for all research which is successful in the sense that it is carried out impartially, validly and reliably and affects policy and practice, there is much which fails to impress or to have impact upon policy and practice. Of course, part of that failure might be due to the inability or unwillingness of teachers or policy-makers to heed the findings of research (see, e.g., Hillage Report, 1998, pp. 49, 50), and there is a need to see how research might enter into the thinking and decision-making of those who work within the education system. Or