’Excellence in research on schools’ - a commentary

by
Harvey Goldstein
Institute of Education
h.goldstein@ioe.ac.uk

Aims of the study

The DfEE in early 1998 commissioned the Institute for Employment Studies at the University of Sussex to undertake a review of educational research relating to schools in England (Hillage et al., 1998). This report, published in August 1998 was based upon a literature review of research activities, 40 interviews with ’stakeholders’ - a mixture of researchers, people from research funding bodies, national agencies, DfEE, LEAs and schools; written responses from researchers and others who felt they wished to respond to an invitation from the research team, and focus groups.

Scope

The stated aim of the study was to examine research relevant to schools. Unfortunately the report consistently fails to distinguish between such research and educational research in general. Thus, the figure of £65 million spent on *educational* research is quoted, but some of this at least will be devoted to education at other levels such as adult literacy etc. This confusion is compounded by an accompanying press release from education minister Tessa Blackstone who quotes the same figure and refers to 'a comprehensive review (of educational research)'. There are other respects in which the report is far from comprehensive.

The authors rely on existing literature reviews of research and interviews with 'stakeholders'. There is no systematic attempt to define or evaluate quality (unlike the Tooley/Darby study, (Tooley and Darby, 1998)), there is no objective attempt to verify what their various respondents say, and despite some acknowledgement that not all research has to be policy relevant, the real emphasis is on what research can contribute to policy.

Of course, given the very limited time and resources available to those carrying out the study, one should perhaps not expect anything very much more detailed than what has emerged: nevertheless, it is difficult to place very much reliance the conclusions and recommendations as representing anything other than a rather superficial summary of the views of a small number of educational activists. I am not, of course, arguing that the report's recommendations are necessarily 'wrong' - I actually agree with 'more partnership' and 'evidence based policy' - but I *am* saying that the recommendations are based upon poor 'evidence' and also that the report does not address some key issues. In the next sections I want to take up some of the issues that I believe are important but neglected.

Research and Policy

In their conclusions the authors state 'The notion that research can (and should) have an impact on policy and practice is widely recognised.' In section 1.2 they refer to their objectives as including the provision of 'a core body of knowledge and theory' but then say that they 'focused (our) attention on the practical value of the research effort in helping to inform the actions and decisions of all those involved (directly or indirectly) in the provision of school based education'. Thus the authors are clear that they are not concerned with research other than for its instrumental value, and as is clear from the above quotation, they imply that this view of the function of research is 'widely recognised'. Even if it *were* widely recognised, however (for which they provide little evidence), that says nothing about whether this *ought* to be the dominant view about research. Given that the Government will presumably act upon the recommendations of this report, it is most disquieting that Tessa Blackstone appears to accept this view of what research is about - 'Most educational research should have a practical use for teachers and others involved in education.'

Apart from a very brief discussion in section 2.1, there is nowhere in the report a serious attempt to analyse this view about research, and this slants not only their analysis and conclusions, but also the emphasis in their interviews and focus groups. This contrasts with discussions about research in other fields where there are extensive debates about the value of 'blue skies' as opposed to directly applicable research. By closing off this debate the report immediately limits its own usefulness both to the research community and to those who ultimately would hope to benefit from good research, namely learners and society in general.

Perhaps the most irritating aspect of this report is its extensive quotation of what particular interviewees thought about aspects of research. This is a good example of qualitative research at its worst. There is no contextualisation of the quotations - for example in terms of the experience or self interest of the person making the statement, or in terms of how representative such views might be. There is no triangulation which attempts to validate or verify what is said, and there are clear attempts to make generalisations when this is strictly inadmissible (see for example section 3.5).

Of course I do not wish to deny that it is important in some circumstances for research to inform policy and practice in a fairly direct way, nor that some research should be funded directly to do this. Indeed, there is a desperate need to have proper evaluations of policy initiatives - an area that politicians seem remarkably unwilling to support to the extent it deserves. There is, however, a very serious issue as to whether *most* research should be of this nature and the sad thing about the present report is that it fails to highlight the need to debate this.

Recommendations for the funding and direction of research

The report makes several recommendations. I want to examine just two.

A national research forum

The authors of the report advocate a body, preferably set up by but independent of the DfEE, which should 'develop an overall research strategy and framework and (to) co-ordinate and monitor developments'. The intention here is that this body would have a very large say in what research was to be done and how it should be funded. The report claims that 'there is an overwhelming need for an overall strategy and framework and that some form of National Education Research Forum needs to be established to bring it about'. Yet they present little rational argument in favour of this proposition and their perception of 'need' is little more than a personal preference for a particular bureaucratic procedure. This forum is clearly meant to take decisions about what should be promoted and funded.

What is being proposed, therefore, is a highly centralised research control mechanism which would effectively determine the direction of most research. While the report talks about it being 'owned by all the participants' it says nothing about the relative representation of different interest groups, nor how any reconciliation between opposing views would be resolved. In the highly unlikely event that a majority of members were in fact 'researchers', one would anticipate that they could only represent a minority of research interests and almost certainly would tend to be drawn from the research 'establishment'. To give such a body these powers seems quite unprecedented in research. If successful it would take away the power to determine the direction of research from the research community which would, over time, simply become responsive to the demands of this body. In practice, of course, it is very likely that the body would be dominated by those bodies with political and funding authority, and one would anticipate a large political steer. In the long run this could only be detrimental to the health of the whole research enterprise.

I am not arguing against setting up mechanisms whereby researchers and policymakers can communicate - there are already many of these: there are conferences to seminars organised by both researchers and local and national policymakers. What these should not become is bodies which seek to direct and control research, and the research community should strenuously resist any moves towards such an end.

Dissemination

The report wants 'clear dissemination strategies…built into all major research….in a way that is most likely to influence practice'. This assumption that all 'major' research is concerned with influencing practice, by which they and Tessa Blackstone mean the practice of teachers and schools and related policy matters, is clearly false. Much major educational research is methodological or theoretical. It may provide new perspectives on learning, teaching or policy. It may elaborate new critical frameworks for thinking about education. It does not *have* to be of immediate relevance and attempts to make it so will eventually stifle it - and that can surely be in no-one's real interest.

It is not difficult to give examples of research which has been hailed as important for practice yet on closer examination, failed to live up to such expectations. A topical example is the school effectiveness work in the early 1980s which struggled to establish criteria for judging institutional effectiveness. At first derided or ignored by most policymakers, some of its basic understandings were eventually accepted, at least formally, by government. Yet more recent work which seeks to place limitations on some of the earlier promise of this research in terms of how far 'value added' judgements are useful or meaningful, is currently either ignored or regarded as 'nit-picking' by many of those charged with directing central government policy. In other words, what some current researchers are saying is that school effectiveness research actually may not have immediate important relevance to practice. This does not mean that it is not worth doing. On the contrary it promises to provide important insights as it becomes more sophisticated, and in certain circumstances, for a minority of schools, it will help in school improvement processes, but it does not provide a magic formula for ranking all schools on an effectiveness index. (See Goldstein, 1997 for a more detailed discussion).

In conclusion

At best this report resembles pop journalism, carried out by people with no previous experience of such research, who indulge in a great deal of hand-wringing over the views of people they have spoken to. The authors produce a final summary and recommendations about what should be done to improve matters, with little real thought about the practical consequences of their suggestions.

In my comments on the Tooley/Darby report (http://www.ioe.ac.uk/hgoldstn/) I suggested that a *serious* study of educational research should look at the various influences on researchers, including funders and policymakers and how researchers of all kinds and research institutions behave as well as attempting to look at the quality of educational research in comparison with other disciplines. As far as policy goes, it is quite possible objectively to study the relationship between policy and research, and there are good examples in the literature, but this is hardly alluded to in the present report.

My own recommendation to the educational research community, and especially BERA, is that they would do well to resist being panicked into supporting measures which would effectively give away control of the direction and content of research to others. If the recommendations of this report were to be implemented I would have very grave concerns for the future of educational research in this country.

References

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