
PRESIDENTIAL ADDRESS

WHAT CAN WE REASONABLY EXPECT OF RESEARCH IN SCIENCE EDUCATION?

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ABSTRACT

I want to focus on some questions which have always been important ones for science education researchers to consider but which are, I think, growing in prominence and therefore in importance. They are about the role and purpose of science education research. What is science education research for? What do we hope to achieve through the research, which we do? What can we reasonably expect of science education research? These are issues which cross-national boundaries and concern us all, whatever national context we work within. So although I will make some reference to recent debates about educational research in the UK, the issues that are raised are international.

1. RESEARCH AND PRACTICE

What can we reasonably expect of science education research? First it might be helpful to clarify who 'we' in that question are. I want to consider the question both from the perspective of people outside the research community who look to research to provide them with useful information and guidance, such as teachers or policy-makers, and from the perspective of people like us, inside the research community. It may be important to ask: do both groups have the same view on what we can reasonably expect of research, or are there differences of emphasis?

In the UK in recent years, there has been a high-profile debate about the value of educational research. The lecture, which started it off, was given by Professor David Hargreaves in 1996. In it he explored the question of whether teaching can be said to be a research-based profession. Hargreaves argues that at present it is not:

Given the huge amounts of educational research conducted over the past fifty years or more, there are few areas which have yielded a corpus of research evidence regarded as scientifically sound and as a worthwhile resource to guide professional action. (p. 2)

He argues for changes in the way educational research is planned and funded and for an end to:

second-rate educational research which does not make a serious contribution to fundamental theory or knowledge; which is irrelevant to practice; which is uncoordinated with any preceding or follow-up research; and which clutters up academic journals that virtually nobody reads. (p. 7)

Of course, teachers are not the only potential ‘users’ of educational research. Some research relates to issues of policy rather than to classroom practice. A review of educational research in 1998 for the UK Department for Education and Employment concluded that:

the actions and decisions of policy-makers and practitioners are insufficiently informed by research, ... where research does address policy-relevant and practical issues it tends to:

- be small scale and fails to generate findings that are reliable and generalisable;
- be insufficiently based on existing knowledge and therefore incapable of advancing understanding;
- be presented in a form or medium which is largely inaccessible to a non-academic audience; and
- lack interpretation for a policy-making or practitioner audience. (Hillage et al., 1998, p. xi)

By 2000, we have the Secretary of State for Education arguing, in a major speech, that

We need to be able to rely on ... social scientists to tell us what works and why and what types of policy initiatives are likely to be most effective. And we need better ways of ensuring that those who want this information can get it easily and quickly. (Blunkett, 2000, 2)

These extracts that I have quoted are a tiny part of a major debate about the role of educational research in the UK, about the kind of research that should be done, and about whether or not it is worth funding from public resources. From conversations I have had with colleagues, I think that similar arguments and debates are taking place in many countries. Similarly the mechanisms for funding educational research in the European Union emphasise research to inform policy-making and support specific social and economic policies.

A key phrase in the debate about educational research in the UK is ‘evidence-based education’, or ‘evidence-based practice’ in education. More recently some people have begun to use the rather softer phrase ‘evidence-informed practice’. These terms are chosen to make a deliberate parallel with the idea of ‘evidence-based medicine’. In clinical medicine it is now widely accepted that doctors’ decisions about the treatment to recommend for a particular patient should be based on evidence that it leads to better outcomes (or is more likely to lead to better outcomes) than other treatments – and not simply on traditional, or established ways of proceeding. Although many established procedures and treatments are not evidence-based in this sense, it is unlikely that any new treatment would be introduced unless it was supported by convincing evidence that it works better than the current one.

So can these ideas be applied to education? In particular, can they be applied to science education? Is it possible for science education research to tell teachers or policy-makers ‘what works’? Can it tell us that one approach is better than another for achieving a particular goal? Are these reasonable expectations of science education research?

Before I address those questions let me first make a number of observations. First, we should all welcome, I think, this evidence that policy-makers are interested in research. Most of us hope that our work will be noticed, and that it will be useful. There is, of course, a corresponding danger that it is the policy-makers who set the

agenda and define the framework within which the research questions are identified. Research has a role to play in challenging the current assumptions and questioning current practices, and few of us would be happy to restrict its scope to the issues that policy-makers currently define as central.

Some have argued that the focus on research that tries to tell us ‘what works’ is too narrow. In one recent article, Jenkins (2001) even questions whether all science education research has to be about improving practice. He comments that the chapters in the *International Handbook of Science Education* (Fraser & Tobin, 1998)

seem to assume that science education as a field of activity is exclusively concerned with the practice of teaching and learning, together with supporting activities such as assessment, evaluation, and teacher education. Correspondingly, research in science education is about improving practice. (Jenkins, 2001: 11)

Jenkins argues for a view of science education research that would include historical and sociological studies of scientific institutions and practices and implies that ‘understanding for its own sake’ may be sufficient justification. It has always seemed to me, however, that the best studies of this sort are precisely those which carefully choose their subject matter and present their arguments in such a way as to illuminate current practices. I think most of us would see science education research as applied; its rationale, and the justification for doing it, is to gain insights that can contribute in some way to the improvement of practice. Its purpose is ‘critically to inform educational judgements and decisions in order to improve educational action’ (Bassey, 1995:39). The issue – which brings me back to the questions I raised a few moments ago – is not whether research should contribute to improving practice, but how it can contribute.

Although terms like ‘evidence-based practice’ may be new, the idea behind them is not. The goal of a ‘science of education’ goes back to the 19th century. Underlying it is the view that there are clear and definite answers to be found. Some have questioned this. The Canadian science educator, Doug Roberts, for example, argues that:

There can be no such thing as a science-like ‘theory of science education’, that is, an explanatory theory with predictive capability. The reason is simple. The events of science education are unique [and] non-replicable. (1980: 65-6)

Another educational researcher, Michael Bassey, makes the same point:

Teaching situations are so varied that it rarely, if ever, possible to say with certainty ‘Do y instead of x and your pupils will learn more’. (1999: 48)

But does this mean that there is nothing that we can say? Are we satisfied with that conclusion? I’d like to quote a short passage from a chapter which Piet Lijnse wrote for a book that I edited about two years ago, along with Jonathan Osborne and John Leach, called *Improving Science Education*. Lijnse writes:

I still remember my disappointment when, as a newly appointed didactician, I had to develop an innovative series of lessons to introduce quantum mechanics at secondary school. I turned to theories of education and educational psychology for help. However, hardly any such help appeared to be available. (p. 309)

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