BERA at 30. Have we come of age?
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Abstract

The increasing demand for research based evidence in the development of policy and practice presents the research community with new opportunities and challenges. We are still troubled by accusations of poor quality and lack of impact. A national research policy, designed to respond to these criticisms has emerged recently which promotes a 'big science' model of research. This increasingly silences other approaches and opens up unhelpful divisions; a particular problem is the re-emergence of epistemological divisions – the paradigm wars. A 'big science' policy for research is also inappropriate in a world where practitioners increasingly want and need to engage in research themselves as a key strategy in 'knowledge transfer'. The paper concludes by arguing that we need to defend a rich and diverse range of approaches to research, promoting debate about quality within different sub communities and encouraging open discussion across epistemological and methodological boundaries.

1. Introduction

This year, BERA is celebrating its 30th birthday. During the last 30 years, our membership has grown substantially, now to over 2,000; our conference has grown year on year, too, with over 1000 delegates this year. All good reasons for celebration. But the question I want to pose, at this key point in our history, is 'Have we come of age?' As an educational research community, are we confident in ourselves, able to look squarely at our own strengths and weaknesses, able to deal effectively with diversity amongst our own members and to represent ourselves effectively to the outside world? I will argue that at present, we are not able to do many of these things; that we still have some growing up to do. That growing up, I believe, is essential if we are to deal effectively with the challenges as well as the new opportunities that we face in a

world where, more than ever before, policy makers and practitioners want to take research seriously.

I should perhaps begin with a caveat. Most of what I want to say relates to England – things are somewhat different, somewhat better, here in Scotland. But as those of us who have lived and worked outside England know only too well, it is not always easy to get our voices heard in key 'national' debates; we also have to recognise that our own discussions in Wales, Scotland and Northern Ireland, are, like it or not, often fundamentally shaped by the English policy context.

As a research community I believe that we are still troubled by our reputation for poor quality work and by the accusation that we have little impact on policy and practice. More importantly, we are increasingly troubled and divided by a national research policy designed to respond to those perceived weaknesses; a policy that appears to prioritise a 'big science' model of research funding at the expense of other approaches.

The contemporary debate about quality was of course initiated by David Hargreaves in his 1996 TTA lecture (Hargreaves, 1996); his criticisms were later 'made official' by the Hillage Report (Hillage et al, 1998) that was commissioned by the incoming Labour administration. Six or seven years on, a perception of poor quality remains prevalent in government circles. For example, Sir Howard Newby (Chief Executive of HEFCE) giving evidence to the Select Committee on Education and Skills in the spring of 2003 said 'education in this country on the whole has a problem with the quality of the research…. It is not as good as it might be and I speak as a former Chairman of the Economic and Social Research Council' (Select Committee on Education and Skills, 2003)

The response of the research community to these criticisms, as Gorard has documented, was and remains robust, at least in public (Gorard, 2004). Moreover, during the last six or seven years, there have been many important initiatives designed to improve the quality of educational research, some of which I will discuss below. But despite our public defence, and despite these initiatives, we are I believe still troubled by the quality of some of our work, a fact recently confirmed by the Research Capacity Building Network at Cardiff (RCBN). In their survey of 25 key stakeholders in the world of educational research, (leading researchers, educational research organisations, educational research journal editors and others) they found that, in the privacy of an anonymised interview, virtually all of those interviewed expressed considerable agreement with

criticisms of poor quality, while stressing that some research was and always had been extremely good (Taylor, 2002).

We are also, I believe, troubled by the accusation of lack of relevance or impact. Despite the spirited defence by Tony Edwards and others setting out what 'Reasonable Expectations' of educational research might be (Edwards, 2000), and despite the laudable work of the ESRC's Teaching and Learning Research Programme (TLRP), there is still an impatience on the part of many policy makers and practitioners that the £60 or so million spent on educational research in England each year does not seem to them to have much direct impact on the quality of teaching and learning in our schools, colleges and universities. Indeed, a concern to make educational research more consistently relevant underpins much of the current debate about the future of the National Educational Research Forum (see Feuer and Smith, 2004)

2. Research and its political context

But why it is that these criticisms come to have such force now? Educational research has always been subject to considerable criticism, so why have those criticisms been taken up with such vigour in the last few years. The answer, as Kennedy (1997) implies, is to do with the changing relationship between research and the state.

> *The connection between research and practice is not one in which research influences practice, as many researchers might hope, nor one in which practice influences research as many might hope, but rather one in which both research and practice are influenced by and are perhaps even victims of the same shifting social and political context.* (Kennedy, 1997: 9-10).

In the 1960s and 1970s, educational research was seen as having an important contribution to make to the social democratic ideals of the day. Its fundamental purpose (as the OECD suggested in 1995) (OEDC, 1995) was to find out about changing clients' needs and wants and then evaluate the impact of government policies in responding to those needs. Using research in this way was central to the social democratic project.

In the Conservative years of the 1980s and 1990s, there was a move away from the view that either policy makers or teachers and lecturers needed research-based information. In a world where policy was explicitly ideologically driven, where those in schools as well as higher education were seen as part of the problem rather

than the solution, then the findings of research were an irrelevance. Given the Conservatives' educational project at the time, there was little that research could contribute that was of value.

The role of research under New Labour has been rather different. Its primary aim, the reason that it has been invested in so strongly, is to influence the world of practice by finding out 'what works'; to gather a body of research based knowledge that can be utilised to develop a range of policies, including pedagogical guidelines or standards for teachers, and thereafter to facilitate the auditing of their practice. When the Labour Government came into power in 1997, it recognised that the previous Conservative administration had handed it the means of managing the educational system – through market competition and the audit culture. But it also recognised that without more content, without more direction, these strategies alone could not ensure that it achieved its own key ambitions i.e. raising standards and increasing equality of opportunity.

As Alan Luke, a leading educational researcher in Australia who has recently moved into the world of educational government has argued (Luke, 2003), in terms of policy, we are living in a post marketisation world. Despite the creation of quasi-markets in education, despite massively increased competition between schools, colleges and universities, despite the plethora of targets and performance indicators, we still have not solved the problem of under achievement, especially amongst the most disadvantaged groups in society. We therefore, Luke argues, need to turn to the research community to help us find out what 'really' works.

The hope is that research, particularly research on teaching and learning, will provide much needed answers that can guide national policy. This has been the approach behind the English Government's National Literacy and Numeracy strategies as well as the KS3 strategy. In this context in particular, educational research is being valued again because of its potential contribution to forms of new managerialism (Hood, 1991), providing the legitimacy through which the contemporary 'evaluative state' (Neave, 1988) can gain control over 'the last corner of the Secret Garden' - pedagogy.

3. Growing divisions

Under new Labour, therefore, educational research has assumed a renewed importance, but it is also seen as seriously flawed. As a community, educational researchers today have more opportunities

than ever before but also face more serious challenges than at any time in the recent past.

But what solutions are offered to the perceived weaknesses in quality and relevance? I would suggest, that over the last few years, we have seen, for the first time in England, the development of a national policy for educational research taking shape and it is a policy designed to support a particular model of educational research – one that can be characterised as a 'big science' model of research. Through the promotion of such an approach the hope is that we will find a way out of our perceived weaknesses.

The promotion of a 'big science' model of research is of course not as explicit as it has become in America, where the Education Act of 2001 ('No Child Left Behind') ties government funding to the adoption of reading programmes 'scientifically proven' to improve standardised test scores, by which the act means randomised controlled trials (RCTs). As a result of this intervention, there is now, in effect, a federal mandate insisting on a particular research methodology.

In England, moves in this direction have been more circumspect and more contested; they are nevertheless there. For example they are implicit in the constant reference in public pronouncements on research that it must, first and foremost, demonstrate 'what works' (Blunkett, 2000). They are also implicit in the prioritising of randomised controlled trials (RCTs) as the 'gold standard' in research methodologies (e.g. Hargreaves, 2003). They have been more explicit in struggles around how to go about undertaking 'systematic reviews' of research and whether or not the 'medical model' (supported by the Canadian based Campbell Collaboration) can provide a necessary bench mark for quality research in education (Evans and Benfield, 2001; Elliott, 2001; Hammersley, 2001).

There have also been struggles about the need for and nature of 'capacity building' for educational research. For example, the DfES sponsored National Educational Research Forum (Dyson and Deforges, 2002) has emphasised the need to increase capacity to undertake large scale quantitative studies, while others (e.g. Furlong and White, 2001 and McIntyre and McIntyre, 1999) have argued for a more broadly based conception of capacity building. And while the RCBN at Cardiff has now developed a broadly based programme of capacity building, its formal priority remains the development of capacity to undertake 'theoretically informed, large-scale, publicly-relevant rigorous research' (RCBN, 2003).

Such moves to promote a 'big science' model of educational research in England are not unrelated to the increased selectivity in the funding of educational research. When Sir Howard Newby was giving evidence to the Select Committee for Education and Skills in Spring 2003, their concern was the consequences of reduced core funding for research in so many of our universities following the last Research Assessment Exercise (RAE). Today, there are now only 31 university departments of education in the UK rated 5*, 5 or 4 which receive core funding to support their research, while there are a further 41 departments rated 3a or 3b which have lost funding because of a change in the funding formula. Add to that the significant reduction of funding for departments awarded a 4 in order to fund the new 6* departments – of which there is none in the field of education - and we cannot ignore the fact that national research policy is increasingly concentrating research capacity in a more limited number of centres. It is not the case, following the last RAE, that we as a sector lost out in our funding – the actual amount made available was almost the same - but its distribution across the system is much more concentrated than before and there seems little prospect of reversing this trend at least in the short term.

One consequences of this increased selectivity is that it opens up an unhelpful division between different types of institution, all of which until now have been committed to the research enterprise. Suddenly, those that are 5 or 5* or which have a serious possibility of becoming so next time round (or whatever the new equivalent will be), are starting to ask themselves if it is in their interest to support the aspirations of those institutions with a lower profile. The consequences of divide and rule are a serious challenge to us all as a community.

And as if these problems were not enough, there is evidence that public sponsoring of more 'scientific' approaches to research has given rise to a considerable amount of epistemological wrangling within our community, opening up divisions of a different sort, divisions that those of us with long memories have not seen since the mid 1970s. It is to these debates that I now wish to turn.

4. Epistemological debates – again

When I gave my inaugural professorial lecture at Bristol University in 1998 (Furlong, 1998), I spoke on the topic of educational research, and there was only a handful of contemporary references to read; five years on there are literally scores. Many of them focus on fundamental epistemological debates about the nature of research and evidence. What they show is that not only are we

challenged in terms of our quality and impact, but there is increasing division in our community. These can be seen as divisions between what Smith and Hodgkinson (2002) call 'neo-realists' and the 'relativists'.

Neo-realists, according to Smith and Hodgkinson, begin with a commitment to the idea of a real world out there, independent of our interest in or knowledge of it. This is a reality that can be known, at least in principle, as it really is. As a result of this assumption, neo-realists accept that the metaphors of 'finding and discovering' are appropriate, even essential, to the research process.

Relativists, according the Smith and Hodgkinson, whether they are postmodernist, hermeneutic, constructivists or whatever, do not disagree with the assumption that there is reality out there. Where they differ from the neo-realists is in arguing that we can never know if we have accurately depicted that reality. For relativists, the metaphors of discovery and finding change to becoming metaphors of 'constructing and making'.

> *Relativists as constructors of knowledge, must hold that while certainly the circle of our interpretive discourse may expand and deepen and become more interesting and even more useful, there is no way out' (of the fact that our understanding is indeed an interpretation) (293)*

For Smith and Hodgkinson, therefore, our world is characterised by two 'epistemic cultures' - cultures which create and warrant knowledge differently. Foray and Hargreaves (2002) take a similar view. They say that within social sciences generally as well as within education

> *There is a powerful bifurcation between two fundamentally opposed epistemic cultures. On the one side stand those who believe that it is possible to treat medicine as a potential model for the advance of knowledge in educational practices and who are thus currently inclined to support the application of the RCT to educational problems. On the other side stand those who reject this totally and favour the epistemic culture of humanism that has deeply influenced work in the arts and humanities in the universities. (Foray and Hargreaves, 2002:12)*

The relativists, of course, sit most comfortably within the humanities tradition of research. It is the oldest tradition in educational research and has certainly become the dominant one in

the last 25 years. And the humanities approach to research is captured in the 2001 RAE definition of research as 'original investigation undertaken in order to gain knowledge and understanding; scholarship; the invention and generation of ideas… where these lead to new or substantially improved insights' (HEFCE, 1999).

But as Burkhardt and Schoenfeld (2003) note, within this approach there is no requirement that the assertions made be tested empirically.

> *The test of quality is critical appraisal covering plausibility, internal consistency and fit to prevailing wisdom. The key product of this approach is critical commentary (p6)*

The outcome of research in this tradition, therefore, is to provide knowledge and insights that essentially work ***with*** professionals, supporting them in their own individualised judgements. Insights from research therefore help in the development of 'reflective practice' (Schon, 1983). As Nisbet and Broadfoot said in their classic analysis of educational research, its purpose is to 'sharpen perceptions, stimulate discussion and encourage questioning - and thus to create the possibility of change and improvement in the system' (Nisbet and Broadfoot, 1980:66).

Although such an approach to knowledge has been dominant within education over the last 25 years, it is now increasingly challenged by a publicly sponsored alternative - that is, a scientific or neo-realist model that is itself increasingly subdivided into an 'engineering' model as well. As Burkhardt and Schoenfeld (2003) note, like the humanities model, the scientific approach to research is also focused on the development of better insight or improved knowledge and understanding 'how the world works', through the analysis of phenomena, the building of models which explain them. 'However, this approach imposes in addition a further essential requirement that assertion be subjected to empirical testing' (6)

The approach is perhaps best articulated by Feuer, Toiwne, and Shavelson (2002) who draw on the report of the US National Research Council's Committee on Scientific Principles in Educational Research, of which Shavelson was chair. They argue that the demand for scientific understanding of educational phenomena is unmatched in history.

> *'Now is the time for the field to move beyond particularised views and focus on building a shared core of norms and*

practices that emphasise scientific principles' (Feuer et al, (2002:1l/12).

In their view, the primary focus of national policy should therefore be the nurturing and reinforcing of a scientific culture of educational research. All science, they argue, including the scientific study of education, shares a set of epistemological or fundamental guiding principles. It should aim to:

- pose significant questions that can be investigated empirically
- link research to relevant theory
- use methods that permit direct investigation of the questions
- provide a coherent and explicit chain of reasoning
- yield findings that replicate and generalise across studies and
- disclose research data and methods to enable and encourage professional scrutiny and critique (Feuer et al, 2002:7)

Feuer et al explicitly reject the idea that federal mandates should limit research to one methodology, applied inappropriately to every type of research question; indeed, within their own paradigm they are committed to methodological pluralism. Like Gorard (2002), they are committed to the notion of the 'completat' researcher with research questions determining the methodology not the other way round. At the same time, however, they also believe that education should use randomised experiments much more 'when well–specified causal hypothesis can be formulated and randomisation to treat and control conditions is ethical and feasible, a randomised experience is the best method for estimating effects' (8)

Qualitative methods are also welcome, but confined to a sub category 'when an hypothesis is poorly understood and plausible hypotheses are scant' (8). Qualitative methods such as ethnographies and other tools like design experiments, they assert, are necessary not as an end in themselves but to describe complex phenomena, generate theoretical models and reframe questions. In short, they remain committed to methodological pluralisms but, in Smith and Hodgkinson's terms, firmly within a neo-realist epistemology.

Increasingly, engineering, or what is sometimes also called 'design' studies, is seen as an important subcategory of the scientific approach (see, for example, *Educational Researcher*, 2003). As Burkhardt and Schoenfeld (2003) say, scientific 'research provides

insights, identifies problems and suggests possibilities. However, it does not itself generate practical solutions, even on a small scale.' (6) By contrast, the engineering approach to research is directly concerned with practical impact, not just understanding how the world works, but helping it 'to work better' by designing and systematically developing high-quality solutions to practical problems. Again, to quote the RAE definition, it is 'the use of existing knowledge in experimental development to produce new or substantially improved materials, devices, products and processes, including design and construction' (HEFCE, 1999).

Such an approach is also methodologically open in that it involves using a range of different research methods in the development stage, though of course within a 'neo-realist' frame. As Gorard (2002) says

> *Design science includes creation, artisanship, craft principles, inspiration and fuzzy science. However at the end of the day, the artefact has to work for the design to be successful. There is not room for relativity here. Either the aeroplane flies, or it does not' (21)*

Engineering approaches also have their enthusiasts, chief of whom is the American educational researcher Slavin (2002). Slavin argues that education is on the brink of a scientific revolution that has the potential to transform profoundly policy, practice and research

> *At the dawn of the 21st Century, education is finally being dragged kicking and screaming into the 20th Century. The scientific revolution that utterly transformed medicine, agriculture, transportation, technology and other fields in the 20th Century almost completely bypassed the field of education (16)*

The most important reason for the extraordinary advances in medicine, agriculture and other fields, he argues, is the acceptance by practitioners of evidence as the basis for practice. In particular, it is the randomised clinical trial – more than any single medical breakthrough – that has transformed medicine.

> *Imagine that there were programs underway all the time to develop, evaluate and disseminate new programs in every subject and every grade level… over time each area would experience the step-by-step irreversible progress characteristic of medicine and agriculture because innovations*

> *would be held to strict standards of evaluation before being recommended for wide scale use (17)*

A brave new world indeed.

Of course, the scientific and engineering models of research have been subject to substantial critique. Berliner (2002) for example, writing in the same edition of *Educational Researcher* as Feuer et al, takes them to task, arguing that educational research is the 'hardest science of all' pointing to: 'the power of context', the fact that there are too many variables to quantify; 'the ubiquity of interactions', the fact that there are so many interactions that it is again impossible to quantify them; and the 'decade of findings', the fact that 'solid scientific findings' generated in one decade are of little use in another because of changes in the social environment that invalidate the research or render it irrelevant.

Or to quote one of our own British critics, Dadds (2002)

> *Post-modernists are quite exhausting friends to have. Yet their irritating theoretical presence saves us from the myth of the single meaning, the single interpretation, the single solution: from the dangers of living unquestioningly within the grand narratives of our day with the attendant spectres of fanaticism and unchallenged, simplistic dogma (15) Dadds, 2002*

And so it goes.

In principle, a debate between two very different research sub-communities that take (or believe they take) fundamentally different epistemological stances on how research is conducted should be welcomed; open discussion strengthens us all. Unfortunately, when the arguments of one side trivialise, as they sometimes do, the arguments of the other side; when there is, as there often is, a refusal to recognise the complexity and depth of the theoretical issues being addressed that stubbornly refuse to go away; when the discussion, as it too often does, becomes acrimonious, when it becomes personalised, it is less than helpful and actually damages even further our own reputation as a serious and scholarly community.

This is not to deny that all of us have a position. I myself grew up in the humanities tradition; I was trained as an epistemological relativist. My PhD, undertaken in the early and mid 1970s, was divided into three – it took a single issue and explored it from three

different theoretical perspectives. A risky strategy for a PhD then and I suspect now, but I use it to illustrate the point that I am not neutral in this debate; I have a strong personal position. However, my view, and I believe that it must be the view of BERA, is that there is much we can learn by our community welcoming these different epistemological positions - not just within the humanities community or within the scientific community but across them. For example, I recognise that the research that Halsey, and colleagues from the 1950s onwards, in the political arithmetic tradition, (Floud et al, 1956; Halsey, 1961; Halsey et al, 1980) can still claim to be the body of educational research that has had the most influence on educational policy in this country in the last 50 years; I also believe that the engineering model, if sensitively done, can contribute directly to the improvement of practice. However, I fundamentally disagree with some commentators when their arguments marginalize or rubbish other research traditions without even attempting to explore the complexity of their positions. I take this stance because I believe that (a) the disagreements between the different communities are important and are genuine and (b) because different research traditions have a great deal to contribute to the core purposes of research.

5. Research, policy and practice

Current policy developments in the field of educational research, designed to address the issue of perceived poor quality and lack of impact, are therefore, I believe, increasingly leading to a dangerous schism within our community; one that reminds those of us with long memories of the acrimonious and unhelpful paradigm wars of the 1970s (OECD, 1995). On the one side we have those who look to medicine, and particularly the randomised control trial, as a model of how research knowledge can improve our understanding of educational policy and practice; on the other side we have those who see themselves working more in the humanities tradition where the aim is to provide practitioners with insights and understandings into complex processes rather than to tell them what to do.

But before we come down on one side or the other, we need to ask how research actually works. How, in reality, does it come to influence policy and practice? We can gain some insight here by looking at the important work of Carol Weiss (1980; 1998) who has explored different aspects of these relationships for over 20 years. What her work demonstrates is that, in most cases, the impact of research is indirect, contributing in the long term rather than the short term through what she calls 'knowledge creep'. (1980b) As Nutley et al (2002) writing as part of the ESRC Network for Evidence Based Policy and Practice state, the ultimate goal of the

evidence based policy movement is generally to effect changes in behaviour, but the instrumental use of research is in fact quite rare and is most likely where findings are non-controversial, require only limited change and will be implemented within a supportive environment; in other words, they do not upset the status quo (Weiss, 1998).

But even if research findings are not used directly, research knowledge can offer insights and ideas and new understanding of practice. Indeed, the conceptual use of research represents a substantial and important category (Weiss, 1987). More widely, as research moves into common currency and becomes accepted, it can change premises that are taken-for–granted and the issues that are defined as problematic. As Nutley et al say, if one takes this perspective, if research utilisation is more broadly defined than its direct translation into changes in practice, then there is much more cause for optimism about the impact of research.

But if one of the main ways in which research is influential is not through direct instrumental change but through conceptual change, then this does not privilege scientific or engineering types of research more than any other – research in either of our two basic epistemic communities can and does contribute to changed understanding. This is not to suggest that the science and engineering models do not have much to contribute to the advancement of policy and practice – they do; nor is it to suggest that we don't need more research capacity in undertaking such studies – we do. However, it is to suggest that the way such studies work in practice means that in reality they have far more in common with research carried out in the humanities tradition than might appear at first sight.

We must also recognise that the science and particularly the engineering models of research are far more likely to fall into the trap of taking rather than making problems; comfortable for politicians but is that our only purpose? As Edwards (2000) has argued, research must recognise what findings might realistically affect policy and practice but it also has an obligation to raise 'difficult questions'. Edwards quotes Robert Lynd, writing in the 1930s, to make the distinction between research technicians, on the engineering model, and research scholars, whose function is to be troublesome. The research community, he argues, needs both, but so does the 'public interest' if not the immediate interests of government. There are, he argues, too many recent examples of concentrated funding leading not only to untroublesome findings but to findings intended or manipulated to give the funders what they want.

6. The quality debate

BERA must, then, continue to argue for a diversity of approaches to research; diversity is essential both for the different types of problem that need to be investigated and because diversity is essential for a pluralist culture.

But we also have to recognise that each research tradition needs to engage in its own rigorous debate about quality. All of us, whatever our epistemological and methodological commitments, have a responsibility to ensure that our work is of the highest quality. But we cannot do that unless we are clear, again within our own research tradition, as to what quality is in the first place. Rather than arguments about whose approach to research is correct; rather than trying to develop some universal criteria for what constitutes good research, it seems to me that the right place to start the quality debate is within each different research sub-community. The criteria for high quality ethnography are not the same as those for high quality RCTs. As a community we cannot run away from the quality debate; but the place to start is with ourselves. I would like to see far more methodological discussions of this sort promoted by BERA and reflected in our national conference in the years to come. We need to move beyond the blanket response that 'quality is a problem' to detailed and specific discussions as to what constitutes quality for each of us, whatever our approach to research.

7. Beyond managerialism?

I argued above that the reason research has become so important under new Labour is because of its links to forms of new managerialism. The hope is that research, particularly research on teaching and learning, will provide much needed answers that can guide national policy, allowing the government to take legitimate control over ever more specific areas of educational practice including pedagogy.

But we also need to recognise that there is an increasing belief in government circles that this sort of centralised, command-and-control approach has reached the end of its useful life. As David Hargreaves has argued in his most recent Demos paper

> *Governments must learn to abandon command-and-control as the primary means of intervention to achieve progressive social ends for two reasons. First, command-and-control is simply unsuited to the complex, unpredictable demands of*

> *organisational life in the knowledge age. Secondly, command-and-control systems tend to treat people in in instrumental ways in which government priorities and values are used to control others, when in fact their active consent is needed (Hargreaves, 2003:72)*

What Hargreaves and others are calling for are more distributed notions of educational management and change where professionals themselves are encouraged to contribute to the development of practice in their own institutions and through networks into the community at large. But what are the implications of such a strategy for a national research policy? Certainly such a vision seems strangely out of tune with the 'big science' model of research currently being supported. Rather than the development of a small number of elite research centres that feed directly into prescriptive national policies, don't we need a more distributed vision of knowledge production and use?

We can perhaps learn some lessons of what might be involved by looking at the Best Practice Research Scholarship Scheme (BPRS) that has been sponsored by the DfES over the last three years. I and my colleague Jane Salisbury of Cardiff University (Furlong et al, 2003) recently completed an evaluation of this scheme in which teachers have been undertaking small scale projects within their own institutions. What our evaluation showed was that projects varied substantially in the degree to which they could be recognised as 'research'; indeed, for most teachers we met, 'research' was not the main point. As Yvonne, one of the teachers, explained:

> *I need to say one thing about the research methods issue, is that actually, when we come into it, it was <u>not</u> for doing research. It was to raise our own ability to use computer software with EAL (English as an additional language) children and we had to gradually make a shift and remember, "oh yes we are supposed to be doing research". I think really that our focus was always – <u>this is a means to an end</u> [emphasis] in terms of professional development, and having a mentor who can support us doing that*

Rather than undertaking 'research', their aim was to improve their practice. What the BPRS scheme gave them was access to a 'discourse' of research – reading theoretical and research based literature, using research strategies to generate evidence in their own classrooms and schools. As such, it was a powerful addition to the traditional strategies they might have used to augment their practice.

During our evaluation, we were struck by the similarities between what we saw happening and what Michael Gibbons and colleagues (Gibbons et al, 1994) characterise as 'the new production of knowledge'. They argue that universities, for so long the home of science, are no longer the only places in modern societies where knowledge is produced. Rather, Gibbons et al argue, the growing demand for specialist knowledge in our increasingly technical society and the expansion of the numbers of potential knowledge producers (as a result of the massification of higher education) means that in many sectors of society, conditions are now set for the emergence of a new model of knowledge production - what they call Mode 2

> *In Mode 1 (which for many is identical with what is meant by science), problems are set and solved in a context governed by the, largely academic , interests of a specific community. By contrast, Mode 2 knowledge is carried out in a context of application . Mode 1 is hierarchical and tends to preserve its form, while Mode 2 is more heterarchical and transient…In comparison with Mode 1, Mode 2 is more socially accountable and reflexive. It includes wider, more temporary and heterogeneous sets of practitioners, collaborating on a problem defined in a specific and localised context.*

Knowledge produced in this way, they argue, will be more transitory, more context specific, more frequently located within individuals themselves and their particular working context than in scientific journals. In short, it is, at least in part, 'embedded' knowledge. The criteria for judging its quality must also be different; they must include judgments about its impact on practice and its impact on practitioners themselves.

What we felt we were witnessing in our evaluation of the BPRS scheme was a sophisticated form of 'knowledge transfer' (though we came to recognise that the term itself is seriously inadequate). By working collaboratively with teachers in their own and other schools and with those in HE, and by deploying research skills themselves, teachers were able to use research based knowledge in the development of their own practice.

But knowledge transfer of this sort cannot happen alone; it still needs a professional research community. We found the research communities were essential in two ways. They were necessary in the support of projects: providing readings, helping in research designs etc. In essence, professional researchers had to be prepared to 'give away' their skills to classroom teachers. They were also essential in that projects themselves needed to be

informed by high quality research. One of the weaknesses we found with many BPRS projects was that individual teachers were not always knowledgeable or critical enough of other people's research; nor were they sceptical enough about their own research findings. If we are to be confident in such a model of knowledge transfer, then professionals need linking with those in higher education and elsewhere who are themselves experienced researchers and who have a wide knowledge of high quality research studies that are relevant to practice.

But again, such a vision of the role of research based knowledge in contemporary society is very different from that currently supported at national level. As I have tried to argue, if it is to be of value to society, then we need research which is of the highest quality and we need research that is diverse in its methodology and its epistemological assumptions. Undertaking high quality research is demanding and difficult; it is a professional activity in itself that necessarily involves careful and substantial training and good resourcing. And we urgently need a national policy that supports such a professional approach and a research community that takes responsibility itself for improving the quality of its work. But the challenge is that if our research, however good it is, is to have value in society, if it is to be accessible and useful in helping practitioners and policy makers both directly and indirectly, then we also urgently need a policy, and a commitment by the research community itself, to support forms of research based enquiry by professionals and policy makers. This means we need a rich and diverse research community that has many different centres of excellence as well as expertise widely distributed across the UK as a whole.

8. Conclusion

BERA has achieved a great deal in the last 30 years. Our conference has established itself as the single most important educational research event in the UK; we sponsor the UK's most highly rated educational research journal; we provide a growing portfolio of activities for our members – from student conferences to methodological master classes; we have a growing number of international links; and we are increasingly looked to by government and other national bodies to provide evidence and advice on a range of different issues relevant educational research. There is then much to be proud of.

However, as I have tried to argue, the shifting social and political context that we face provides many challenges for us. Our increased significance in a society that now recognises the importance of research based knowledge means that we have more

opportunities to make a contribution to the development of policy and practice than ever before - but we also face more criticism and more challenges than ever before. As the voice of educational research in the UK, BERA has a vitally important contribution to make to the development of research policy. But before it can do that with confidence, it still, I would argue, has some growing up to do.

What I have tried to argue in this paper is that, firstly, we must be grown up enough to recognise the richness and diversity of the research community, tolerate differences and contradictions and promote dialogue amongst those who hold different perspectives. Our diversity should be seen as our strength not our weakness. Secondly, we must be grown up enough to insist that each different sub-community engages in its own robust review of what quality is and how it can be promoted. Only when we ourselves take the quality issue seriously, will we be taken seriously by government and by our peers in other disciplines. Finally, we must be grown up enough to welcome the opportunity to work directly with other professionals on the issue of knowledge transfer, helping them develop the skills necessary to use the best quality research in the development of their own policy and practice. Working collaboratively in this way is not a substitute for high quality research, nor is it something that only those committed to action research must undertake; it is a necessary strategy for us all in ensuring that our work does indeed have an impact. If we do these things then we can with confidence argue that the community a whole, not merely an elite group of institutions, deserves support because we make a vital contribution to the development of education in this country.

Finally, BERA might consider adopting this quotation from Michael Foucault as its motto for the next 30 years. It is I believe the key to being genuinely grown up.

> *I believe too much in truth not to suppose that there are different truths and different ways of speaking the truth (Foucault, 1979:51)*

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