Bread and Circuses: some challenges to educational research in the 1980s

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I

To speak at Lancaster University is not a new experience for me. I attended my first academic conference here in 1970 and gave my first paper here, 12 months later. But just in case you receive the impression that my early associations with Lancaster were particularly auspicious, I should perhaps add that, in my eventual search for a permanent position, this institution was also the source of my very first rejection slip.

I had visited Lancaster in those days to take part in successive classroom research conferences whose participants, incidentally, comprised one of the networks that coalesced in the formation of BERA. My research aspiration, which grew from my own teaching experiences in a Leicestershire comprehensive school, was to unravel the mysteries of classroom life and, by such means, foster the improvement of teaching and learning (my own in particular). I had no expectation of returning to Lancaster in circumstances such as these. I still regarded myself as a schoolteacher; and it was upon that peculiar activity-schooling-that my attention was focused.

Despite (or because of) my early experiences at Lancaster University, that topic is still my major interest even if my intervening career can be likened to Philip Jackson's characterisation of teaching: more like the flight of a butterfly than the path of a bullet (Jackson, 1968, pp. 166-7). Indeed, critical friends should be forgiven for believing that my curriculum vitae makes dilettantism look like a pillar of the protestant ethic. Needless to say, I hold a rather different self-image. I would prefer to characterise my course of life as a series of intellectual detours that have given me greater awareness of the kind of conceptual, methodological and historical pitfalls that litter the path of any research programme.

In my case I have been repeatedly drawn back to questions of the kind, 'What counts as an educational improvement?', 'Is an improvement for teachers necessarily an improvement for learners?' and, 'What is the relationship between improvement, efficiency and progress?' Thus, when pressed to characterise my research career, I tend to describe it to psychologists as a series of decentring movements; to sociologists as a recursive bracketing of my taken-for-granted reality; and to statisticians as a

demonstration of the non-Euclidean axiom that the Shortest distance between two points is not necessarily a straight line.

But there is another sense, too, in which the detour metaphor is appropriate. At times I have pursued topics in the context of an invisible college, rather than as part of a personal programme. That is, I have opted to tackle questions which complemented the work of others, even if such problems were not the highest of my own Priorities. To this important extent, my work has been a social rather than an individual enterprise. Like modern physics' view of the electron, my career should be regarded not as an isolated trajectory but, rather, as a pattern of activity within a web of wider events, wider relationships and wider movements.

This evening, I would like to continue in a similar vein-by sketching in a little more of the contextual background to educational inquiry. First, I shall offer an account of the origins and evolution of the social sciences; secondly, I shall focus more specifically upon educational research as it has been defined and developed since the mid 1960s; and. finally, 1 shall bring the story up to date by reflecting upon the place of organisations like BERA within the general field of educational practice in the 1980s and 1990s.

II

Educational inquiry, as we know it today, can be traced back through at least three important transitions. The earliest of these was the scientific revolution of the sixteenth and seventeenth centuries-a period when Bacon (1561-1627), Descartes (1596-1650), Newton (1642-1727) and others achieved great success in destabilising medieval conceptions of a balanced and 'unchanging natural order. It was at this time that the closely-linked terms 'method' and 'curriculum' appeared in their modern form. Both introduced a dynamism into the practical arts of science and schooling. In the first case 'method' began to refer to the sequence of formalised operations drawn up and deployed to extract new knowledge from nature. And in the second case 'Curriculum' appeared as a label for the sequential programmes of study that accompanied the reform of teaching and learning in the post-Renaissance schools and universities of northern Europe. In each case, an important measure of decoupling also took place. 'Curriculum' brought to schooling the same kind of 'objectivity' that science brought to the wider world of public affairs: both, that is, began to be presented as powerful yet freestanding technologies of social leverage.

Against this general interest in the extension of human potential, Bacon popularised a purpose for science (viz. the public control and exploitation of nature); Descartes furnished a procedure for science (viz. the reduction of complex phenomena to their constituent parts); and Newton provided a demonstration of the potency of science (via successful predictions about the movement of terrestrial and astronomical bodies).

The excitement generated by these ideas, and the analogic thinking that they prompted, ran rapidly through the whole realm of natural philosophy. Could Newtonian notions account for the behaviour of living as well as inanimate bodies? Was there a moral force,

akin to gravity, that held together the disparate parts of civil society? Could complex social phenomena be reduced to a small number of basic human faculties? And did the motive forces furnished by such dispositions interact together in comparable causal patterns?

Assisted by such questions (as well as the invention of the microscope and the spread of colonialism) the cause of eighteenth-century science was marked by a general enthusiasm for exploration, categorisation and systematisation. For instance, the early decades of the century saw the emergence of Linnaeus' taxonomy of plants; the middle years accompanied the all-embracing endeavours of the French Encyclopedists; and, above all, the concluding decades marked the rise to prominence of a group of social and moral philosophers (e.g. Jeremy Bentham, Adam Smith and Marie-Jean Condorcet) who, it was widely believed, had finally unravelled the workings, if not the evolution, of human societies. This last development-associated with the emergence of the term 'social science' (see Baker, 1975, appendix B)- marks the second turning point in the history of social inquiry.

But if attention to systematisation implies drawing distinctions between different entities, it is perhaps not surprising that the nineteenth century saw the break-up of ‘social science’ into a series of discrete disciplines. Yet, despite this tendency to fragmentation, each new discipline still set sail with a large amount of Newtonian ballast. Each rejected non-rational processes; each sought to explain the world through the existence of stable, 'natural' phenomena (e.g. human nature); and, finally, each accounted for the diversity of social and natural forms in terms of the permutation and combination of a smaller number of lower-order attributes.

Whether this view was logically and empirically justified at the time is an open question. Nevertheless, such real (or apparent) sophistication cleared the way for direct intervention in social affairs. To analyse a living system into its constituent parts was also to identify the levers of social change. Thus, with the help of the early statistical societies of the 1830s and 1840s, the common-sense rhetoric of social linkages (e.g. between poverty, ignorance and crime) was refined into influential cause and effect statements. In turn, these statements helped to shape government policy, notably in the realms of education and social administration (see Cullen, 1975).

Ultimately, however, the statistical societies probably did more for fact gathering than they did for social reform. Despite early hopes to the contrary, their influence declined as unambiguous causality proved difficult to establish. But the 'billiard ball' view of social mechanics did not disappear. Quite the reverse. It took a new lease of life as the word 'variable' began to be an accepted entry in the social science lexicon. Such an innovation-whose history, it seems, has still to be written-marks the third of the transitions I referred to earlier.

Armed with such a powerful resource, statistics sought to break away from its spawning ground in the social sciences and find a new home in the mainstream of mathematics. With hindsight it was only partially successful. Despite the brilliance of Karl Pearson

and others, the ideological gap (cf. relevance versus rigour) that opened up between the old and the new statistics remains as problematic to the educational research of the 1980s (cf. Cuttance, 1984) as it was to the conduct of social inquiry in the 1890s.

Nevertheless, the meteoric rise of the variable had at least one important institutional consequence. As social inquiry took on the trappings of the natural sciences, so it also took up residence inside the Academy.

By that time-I speak of the years following the foundation of the London School of Economics and Political Science in 1895- implementation of the Newtonian paradigm began to have certain new outcomes. First, the idea that science could be a tool of government underwrote the beginnings of substantial public investment in the social sciences. Secondly, the fact that the laws of science were still widely regarded as natural and freestanding underwrote the university training of a new breed of professional policy scientists. And thirdly, the continued faith in atomistic basic elements underwrote the pervasive claim that educational practice could be reduced to the operation of psychological principles (see, variously, Brennan, 1975; Hamilton, 1983; Selleck, 1968; Simon, 1981; Silver, 1983).

Such was the view of educational inquiry that seemed to hold the floor in those days. And it is the view that many of us absorbed as part of our own initiation into the Academy. But it was not the only available paradigm. Other perspectives, notably those that took account of historical and comparative data made equally valid claims. Indeed, international surveys even achieved official recognition with the establishment in 1895 of the Board of Education's Office of Special Inquiries and Reports. But, in the event, we are probably more likely to remember Charles Spearman, Cyril Burt and Godfrey Thomson as our founding forebears than we are to celebrate the earlier name of Michael Sadler, the first Director of the Office of Special Inquiries (cf. the history offered by Rusk, 1932).

Within education these new notions about the application of science became tied to new ideas about the organisation of schooling and the practice of teaching. Those few professional educationists who figure prominently in the histories of educational research devoted their energies to research programmes built around the biometric (or eugenic) sciences-notably psychology, statistics, biology and genetics. The thrust of such programmes was technocratic and administrative. The social problem that they addressed was that of creating a rationale for the scientific management of secondary school expansion (Sutherland, 1984). To this extent, their research fed (or was intended to feed) the interests of a supervisory cadre in education.

In fact, their ultimate impact upon education authorities seems to have been patchy. And, as in the USA (See Berman, 1983, pp. 308-9), they also ran into difficulties with those members of the Academy who had already (i.e. since the 1890s) committed themselves to the professional development of elementary school teachers (see Rich, 1972, pp. 221-33). By 1912-the year following the publication of F. W. Taylor's *Principles of Scientific Management*-the tension between classroom pedagogics and administrative training had

become readily apparent. In a discussion of Taylor's work, for instance, John Adams (Professor of Education at the University of London) pinpointed the issue at stake: 'Perhaps the most important problem in the educational theory of the future', he wrote, 'is the place the teacher is to occupy' (Adams, 1912, p. 379).

In this context, William Boyd's career at Glasgow is, I think, also relevant-if only as the exceptional case that proves the rule. Although Glasgow University had no direct involvement in initial teacher training, Boyd's views on educational research were very teacher-centred. In 1919, for instance, a belief in 'teachers' self-government' was, he reported later, an important factor in his becoming the first convenor of the Research Committee of the Educational Institute of Scotland (Scotland's largest teacher organisation). And in 1924, Boyd was able to claim that the eventual & 'main inspiration' of his research into test construction was not so much the goal of creating perfectly standardised measures, as the thought of 'helping teachers' to make a 'more accurate estimation' of the results of their teaching,(Boyd, 1924, pp. 6-7). Nevertheless, despite these and a wide range of other achievements Boyd's preferment within the Academy never matched that of Godfrey Thompson, progenitor of the Moray House Tests and Boyd's Edinburgh counterpart.

If the concerns of Adams and the case of Boyd and Thompson have any wider validity, they tend to reinforce the view that the Academy selectively nurtured the social sciences in the interests of what have been called the regulative functions of the state. And it was largely through this kind of research (e.g. into vocational guidance, child development and secondary school selection) that the Academy collaborated with the official thinking of the inter-war years . It legitimated, that is, the efforts of 'administrative progressives' (Tyack, 1974) to achieve what they believed was a more rational, efficient and meritocratic educational system. Whatever its practical successes at that time, this movement's most enduring achievement was, I believe, the insertion of a particular view of research into the common-sense world of schooling. As Liam Hudson commented at a conference in 1970, it had taken psychologists 50 years to sell mental testing to the public, and it would take them another 50 years to buy it back.

III

By 1970, as you well know, the influential view of educational science laid down immediately before and after the Second World War had already begun to lose its lustre. As a result, the Newtonian paradigm was only able to sustain its original truth claims with the aid of varying degrees of fudging. Since we are at Lancaster, let me give you a local example (which is also one of my favourites). In 1976, Noel Entwhistle claimed in the preface to *Teaching Styles and Pupil Progress* that the veracity of Neville Bennett's findings was, I quote, "suggest(ed) in unequivocal terms" (Bennett, 1976, pp. viii-ix).

Between us, no doubt, we could furnish many more similar examples. But, in an important sense, fudging is not the issue. Even if Newtonian methods were shown to be above technical reproof, their proponents could still be accused of serving the interests of the managers rather than the consumers of schooling. It was this last weakness, I believe,

that finally broke the mould of educational research in the late 1960s and early 1970s. Louder voices could be heard around the educational research arena claiming that the Newtonian paradigm had little, if anything, to do with the classroom life of teachers and learners (see, for instance, MacDonald & Rudduck, 1971; Parlett, 1972; Taylor, 1971).

My own teaching experience contained a good example of this particular problem. The school in which I worked was approached to assist in the validation of a well-known junior personality scale. On a given day classes were disrupted while all the second-year children were sent to the hall to complete a series of tests. A few days later the researcher reappeared with a list of those pupils who had failed to complete all the test items. Children were withdrawn from classes once again, but against the protest that such a procedure publicly identified (and stigmatised) all those pupils who had reading difficulties. As I recall those events I still feel angry, both about the exploitative nature of the research and about my inability to criticise it more adequately.

It was the cumulative effect of circumstances like these, and not a major crisis in research philosophy, that forced the pace in the 1960s. A kind of pincer movement brought matters to a head. On the one hand, curriculum developers found that the urgency of their task could not be fitted to the one-variable-at-a-time methods then current in research (i.e. before widespread access to multivariate computer programmes). And on the other hand, disenchantment with educational prescriptions was voiced by young and inexperienced teachers who had found that their training (and their grammar school background) were inadequate to teaching in mixed ability comprehensive schools.

Both of these groups, however, were cushioned in their plights. The respective curriculum developers were backed by substantial financial investment; and the out-of-depth teachers were offered fresh buoyancy by the rapid expansion of certificate- and degree-level inservice provision. While the effects of the former are probably more visible in the historical record, we should not underestimate the importance of the latter. The English-model part-time master's degree irrevocably shaped the educational research of the 1970s, just as the Scottish post-graduate Ed.B. degree had done so in the preceding half-century.

But these new degrees differed from their northern predecessors. The research values that they espoused were increasingly curriculum-based rather than subject-based, pedagogic rather than psychometric, and educational rather than managerial. Moreover, with the various expansions of the late 1960s and the early 1970s, their influence soon became felt within teacher training, within new curriculum development projects, and even, if to a lesser extent, within 'mainstream' educational research. With the facilities and freedoms afforded by such institutionalisation, new ideas, networks and publications began to receive wider attention. On my first visit to Lancaster in 1970, for instance, I shall never forget Brian Torode trying to convince Ted Wragg that the existentialism of Jean Paul Sartre had more to offer classroom research than the interaction analysis of Ned Flanders.

Looking back, I find it difficult fully to comprehend those developments-except perhaps in such seemingly anecdotal terms. From one viewpoint they brought together those who sought to accommodate the Newtonian paradigm to new, school-based purposes (and who, through their attention, gave new life to the levels of analysis problem in statistics). From another perspective they could be regarded as the resurgence of a long-standing brand of teacher-oriented progressivism (and here important lines of descent run from John Adams to Denis Lawton and from William Boyd, through Stanley Nisbet, to Lawrence Stenhouse). While, from another standpoint, still, they might be remembered simply as marking an era of pluralistic uncertainty about the purposes and assumptions of both social science and maintained schooling. Overall, however, I like to think of them as a time when piecemeal social engineering began to be challenged by its unkempt 1960s offspring-wholemeal social engineering.

Soon after 1970, however, funds for educational expansion in general and educational research in particular began to be the subject of much tighter controls. Reassertion of the claim that managerial efficiency was essential to social advance provided a fertile ideological terrain upon which the Newtonian paradigm could be recultivated. The latter's technocratic orientation could be readily harnessed; to the shift of funds from research to development as proposed in the Rothschild-derived White Paper *Framework for Government Research and Development* (HMSO, 1972). Similarly, the Newtonian-derived (and Darwinian-derived) attention to psychological variables fitted well with renewed attempts to explain the shortcomings of schooling in terms of the attributes of teachers and pupils. And, not least, Newtonian assumptions about the timelessness (or 'external validity') of scientific results made it much easier to claim that policy generalisations flowed 'naturally' from the data furnished by educational researchers.

More recently, there have been further moves in this direction. As you are well aware, educational inquiry has been subjected to additional controls, as part of the general restructuring of public expenditure. Indeed, if the 1971 Rothschild Report (HMSO, 1971) pushed research into the market place, and added terms like 'customer' and 'contractor' to our working vocabularies, there is a sense in which more recent events have drawn it out again and placed it under the control of central government departments. Incidentally, both the Social Science Research Council and the Royal Society noted this possibility in their original responses to the Rothschild Report. The SSRC, for example, cautioned against a 'monopolistic role' being adopted by central government-a state of affairs that, as the Council of the Royal Society put it, "tend(s) to divorce scientists from the real users as opposed to their representatives in government" (Select Committee on Science & Technology, 1972, pp. 292, 62).

If what I have said is true, we are now in a post-Rothschild era-one that, if you like, began with announcement in 1982 of the closure of educational research's busiest (and noisiest) market place-the Schools' Council; an era that has continued with the relatively restricted remit allowed to the SSRC's successor, the Economic and Social Research Council; and an era that has reached its maturity with the unprecedented financial and/or prescriptive power placed at the disposal of such regulative agencies as the Assessment of Performance Unit and the Manpower Services Commission.

At this short distance, it is difficult to assess the significance of these shifts and reorientations. Clearly, the government of the day (or at least certain sections of it) is attempting to take a more interventionist stance. One possible consequence, foreshadowed in the 1983 University Grants Commission Circular Letter (UGC, 1983) is that a revision of the dual support system may even jeopardize internally supported university research. Indeed, many departments in my own institution already require the direct patronage of government research funds to maintain, in the UGC's terminology, their 'well-found' status (Advisory Board for the Research Councils, 1982a). I suspect that, for more and more of us, such difficult if not demeaning circumstances are the main reason why we seek to join the funding circus. But joining a circus is no guarantee of good research, any more than competence at fund raising is positively correlated with good school teaching. In such circumstances, the organisational means may obscure the educational ends: 'Never mind the quality' so to speak, 'Feel the BBC micro'.

I think, too, that these most recent political initiatives will further reinforce the Newtonian paradigm. Here, for instance, I am mindful of the close coupling of education with psychology in the Education and Human Development Committee of the ESRC; the same Council's seeming acceptance that issues surrounding 'Research Resources and Methods' (to which it assigns a separate committee) are reducible almost entirely to the technical problems that beset multivariate analysis; and I am mindful, too, of the Working Party on Postgraduate Education's technocratic recommendation that a doctorate should be more a training in research methods than an opportunity to pursue 'original inquiry' (Advisory Board for the Research Councils, 1982b, p. 89).

But, as before, a counter current also exists. Today it is fed from various quarters: from the grassroots aspirations of teachers destined to remain on the lowest rungs of the career ladder; from the reaction of local education authorities to the erosion of their curriculum autonomy; from the intellectual critiques of interest groups, like black parents, who feel victimised by Newtonian-derived testing programmes; and, not least, from the growing fear in the Academy that, in a climate of redistribution rather than expansion, creation of so-called centres of excellence will expel most of us into the same peripheral archipelago that has claimed many of our untenured colleagues.

IV

Where, then, does all this leave BERA? If government and the academy are in a state of tension, should we regard that tension as an enduring, even necessary, feature of our existence; or should we regard it as dysfunctional, and seek to reduce it? Are we just playing against the wind of monetarist politics? Or has the game changed altogether?

In the short term, my answer is to point to the inclement weather. Accordingly, I think we should assume that many recent developments are open to reversal. Yet, in the longer term - and here I am speaking of decades not years-I feel much less confident about the structures and assumptions we employ today? But is it possible to reconcile these two positions? Are there no alternatives, other than repairing the status quo or designing

Utopian and unrealisable futures? How are we to act so that today's solutions do not limit tomorrow's options? Let me offer some suggestions, starting from where we are now.

As currently constituted, BERA provides an independent forum-one of the largest in Britain-for educational currents to meet, to take stock, to share ideas, to recruit new members, to sell their publications and even, perhaps, to exchange the odd intellectual hostage. My own view is that BERA should continue this important work. It should aim to extend its influence, not in an imperialistic manner, but in the support of intellectual reciprocity. Conferences like this demonstrate its capacity to provide a range of sounding boards-each of different acoustic qualities-against which educational practices can be examined, debated and judged. Today, this need has become more acute as, for instance, research student numbers decline, as research capacities are generally trimmed; as administrative rationalisation begins to favour large against small departments; and perhaps, too, as both the adrenalin and the surplus fat in the body politic become exhausted.

But is this enough? It may have been so in 1974, but I do not think it adequately embraces our aspirations in 1984. Three new tasks, I feel, are worth adding to our current agenda. The first relates to the question: 'Where does BERA stand following the reorganisation of the SSRC and the closure of the Schools Council?' Is there any way in which we should acknowledge the vacuum that has been created? While I am not over-enthusiastic about the idea of setting up a Schools Council in exile, I do think that we could give further support in our journals and our conferences to the networks that such institutions have brought into being.

My second suggestion is that BERA should continue to foster self-critical debate about its own status in the world of education. Again, it would be trying to compensate for the decline of an existing institution-in this case the dual support system. In past meetings of BERA, the 'teacher as researcher' debate questioned some of the distinctions (e.g. teacher-as-expert versus teacher-as-operative) that, as I suggested earlier, were built into the foundations of educational research. Today, I think BERA could take a similar stance towards such interrelated topics as 'craft know-ledge', 'action research', 'conviction research' and, more generally, 'the new philosophy of science'. Is the classroom knowledge of teachers necessarily inferior to the high-level abstractions of educational researchers? Can the technical aspects of teaching be separated from the ethical dimension? Does the Newtonian paradigm merely serve to legitimate the male-centredness of our culture? Are value commitments a help or a hindrance in the focusing of our educational problems? (see, variously, Atkins, 1984; Capra, 1983; Carr, 1984; Carr & Kemmis, 1983; Cronbach & Associates, 1981; Eisenstein, 1984; Elliot, 1983; Fritzell, 1981; Hirst, 1983; Rose, 1983; Schon, 1983; Toulmin, 1982).

By challenging our own presupposition such debates perform an essential service. Sometimes the challenge fizzles out; sometimes it is sent packing; but always it alerts us to one of science's greatest pitfalls-intellectual complacency. Without such anticipation-and the intellectual sophistication that goes with it-BERA would not only be a very dull organisation, its members would also be woefully ill-prepared for the future.

This brings me to my final suggestion. I believe that it is not enough for BERA to address these and other problematic issues in a self-interested way. Rather, it should seek to make them more widely Accessible by, for instance, extending the participation of part-time Diploma and Master's degree students in its conferences and seminars. Without such an outward-looking stance BERA runs the risk of having its intellectual claims dismissed as elitist and obscurantist. In times like these, few of us can afford such a luxury.

To conclude, in this address I have tried to combine my personal views on educational inquiry with my stewardship responsibilities as the incoming president of BERA. It would have been much easier to speak about some relatively minor but attractive intellectual detour and, by doing so, ignore the intellectual and personal difficulties which many of us face. Nevertheless, by building upon the guiding assumptions of BERA's ten-year history I hope I have been able to offer some positive prospects for the future. The difficulties and uncertainties should not be underestimated. But if we face up to them in a principled yet open manner, I think we can also anticipate the continued vitality of the democratic community of interests in which BERA plays a recognised and appreciated role.

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