

## ***Educational Research: The State of the Art***

*Note: This paper was originally given as an address to the inaugural meeting of the British Educational Research Association in Birmingham on 5 April 1974*

### **JOHN NISBET**

This attempt to review the present state of educational research in Britain is presented under three headings: growth, trends and structure. I would have found it easier to deal with the subject in a course of ten lectures - after all, the Open University course E 341 on educational research required nineteen booklets, eleven radio programmes, eight television programmes, eight assignments and one project, just to provide an introduction to the topic - but I shall do my best within the limits allowed me.

#### ***The growth of educational research***

The fact that I have a problem of compressing the subject is itself evidence of my first point, the growth of educational research in recent years. When I began teaching the topic in university in 1949, the text-books I inherited were Vernon's *The Measurement of Abilities* (1940), Burt's *Mental and Scholastic Tests* (1921), Rusk's *Introduction to Experimental Education* (1913) and Whipple's *Manual of Mental and Physical Tests* (1912). Being very up-to-date, I introduced Charlotte Fleming's *Research and the Basic Curriculum* (1946), which was the first example in Britain of a compendium of research which offered something more than just psychometrics. Nearly twenty years passed before we began to see the present flow of really useful books summarising research findings in a general way, like Thouless's *Map of Educational Research* (1969) and Butcher and Pont's series, *Educational Research in Britain* (1968, 1970, 1973). In the meantime we had to rely on American books, journals and encyclopaedias of educational research, and it is only in the last ten years that educational research has established itself in Britain as a topic in its own right.

The setting up of the British Educational Research Association in 1974 was another evidence of the growth of educational research. Other writers - particularly, Wall, Taylor and Vernon Ward - have calculated how the national expenditure on educational research multiplied ten-fold between 1963 and 1974. Whereas then only 0.01 per cent of all educational expenditure was devoted to research, the figure is now about 0.1 per cent - or, for every pound spent on education, one-tenth of a penny goes to development, research and evaluation. In 1974, for the first time ever, the number of SSRC postgraduate studentships in education reached three figures - 100 exactly. If that sounds a small number, remember that in 1968 there were just fourteen SSRC studentships in education; and even in 1972 there were only sixty-five.

There has been a very substantial increase all round; but one has only to see the present position in context to acknowledge that there is still a long way to go. The figures merely demonstrate that, until very recently, educational research was a spare-time amateur activity for gentlemen of leisure.

A more important form of growth has been the widening range of types of research which can be reckoned as coming within the scope of educational research. For many years in the past, educational research was almost exclusively concerned with educational psychology and testing; and though there were eminent pioneers in the fields of the history of education, the philosophy of education and comparative education, these aspects of the subject were isolated and quite unsupported by formal associations of scholars. Recent years have seen a remarkable growth of interest in these areas, and there are now flourishing societies for each of the three aspects. The emergence of curriculum development as an area of study and research has been the largest single element in the growth of recent years, thanks to the work of the Schools Council. But each of us has his own choice of factors which have led to the multiplication of aspects of educational research: sociology, educational technology, classroom observation, participant observation, administration and management of schools, and so on - a whole range of specialist disciplines.

The emergence of these specialisms, however, also carries with it the danger that the study of education may split up into less and less meaningful sub-divisions. There was a time in Aberdeen University, in 1960, when I taught everything in the Department of Education, alone - from Aristotle and comparative education to statistics and testing. (Needless to say, the teaching was not of a uniformly high standard.) Now in the Aberdeen Department, the number of staff is in double figures, though it is still a relatively small department. But now we have to have special meetings to ensure that there is some link between the various aspects, and we require every member of staff to do some tutorial work across all the boundaries. This is a common problem in all disciplines, nowadays. In educational research, in my view, it is particularly important that the different aspects should not develop in isolation: the empirical social scientist needs to draw on history, comparative studies and philosophy. When Noel Entwistle and I wrote our report on transfer from primary to secondary education (1966, 1969), we had a section on theoretical aspects, a historical section, a chapter on comparative studies, an empirical follow-up study, and a small piece of action research. Perhaps the correct solution is to build up a research team which brings the different aspects together. But I would argue strongly against the fissiparous trend in current educational research.

### *Trends in educational research*

Among the various sub-divisions I have been discussing, one of the most vigorous in its growth is the sociological. For many years, educational research was dominated by psychology: it aspired to scientific precision in research design and hypothesis construction and was preoccupied with measurement and statistical analysis. There has undoubtedly been a swing away from this style of research, not unconnected with hostility to examinations and testing and the selective and classificatory function of education. Some of the enthusiasts for sociological styles of research have moved in on the back of this anti-scientific and anti-measurement wave. Of course they are right to be sceptical about precision in the behavioural sciences. Sometimes important aspects are ignored merely because they are difficult to measure. Often the hypothetico-deductive method is a way of reinforcing our assumptions, for we all make assumptions which provide our orientation to the world and define it for us, and direct our attention so that we see only what we are looking for; and we ought instead to start by trying to see the situation as the other person sees it. Perhaps we are witnessing a change in educational research, like the change in music and art from a classical era to a modern one. But I don't see this trend as dispensing with the need for rigour and

precision. On the contrary, there is a place for both styles, and certainly students should be responsive to the merits of both, so that they can recognise excellence and spot the flaws, whatever the style.

Rather than see this as a confrontation, I suggest that the trend of the past fifteen years has been the emergence of a range of styles, which have added greatly to the power of educational research methods. I used the word 'range', but perhaps it is better described as a 'spectrum'-

1	2	3	4	5
<i>Experimental method</i>	<i>Exploratory survey</i>	<i>Curriculum development</i>	<i>Action research</i>	<i>Open-ended inquiry</i>
<b>Empirical Educational science</b>	<b>Fact-finding as a basis for decision making</b>	<b>New syllabus content and method. Field trials and evaluation</b>	<b>Interventionist</b>	<b>Grounded theory. Participant observation. Illuminative evaluation.</b>

*The agricultural model*

**Experiments to improve your products by manipulating treatments**

*The anthropological model*

**Go and live there and see what it is like**

because a spectrum has no sharp boundaries, and also (if it is not straining the metaphor) because you get white light by mixing all the different frequencies!

Categories 1 and 2 represent the empirical tradition, which has a strong Scottish-American flavour. The Scots - Thomson, Rusk, Drever, Boyd - who set up the first educational research council in Europe in 1928 believed in it, and the idea can be traced back to Herbert Spencer and to Alexander Bain, who as a professor in Aberdeen University was responsible for the teaching of philosophy, logic, rhetoric, English literature and language and psychology, and also wrote a book called *Education as a Science* in 1879. In fact, in 1946, in Aberdeen Training College (as it was then called) the Departments of Education and Psychology were combined into a Department of Educational Science - an innovation which was subsequently abandoned. The concept of a science of education is based on the belief that educational problems should - and can - be solved by objective empirical evidence, that precise and accurate research can build up a structure of knowledge which will generate new hypotheses and new experiments, until the whole field is uncovered. This was a common aspiration in the 1920s and 1930s, and it has an initial appeal to each new generation of researchers. It is based on the faith that, if only one could design a good enough experiment, with effective controls, precise evaluative measurement and appropriate sensitive statistical analysis, it should be possible to establish objectively the one best method, the ideal curriculum, the optimum period of instruction, the correct use of aids to learning. Once these points have been established by experiment, and adopted in the educational system, any change must be for the worse. So the Scottish researchers tried to decide whether it was better to teach children to add up a column or down a column; whether in subtraction, the method of 'equal addition' was superior to the method of 'decomposition'; whether 'phonics' was better than 'look and say'; and today, the educational technologists try to determine whether programmed learning is better than traditional methods, whether television is better than a live teacher, whether colour is better than black-and-white; and in higher education, we are asked, 'Which is better, lecture or tutorial?' or 'Is there any evidence to show what is the

optimum length of a lecture?' The educational science idea keeps recurring: in 1964, Sanford, in *The American College*, wrote: 'Practice in higher education, as in politics, remains largely untouched by the facts and principles of science. What our colleges do, tends either to be governed by tradition, or to be improvised in the face of diverse - and usually unanticipated - pressures.' So Sanford envisages 'a science of higher education ... the notion that the field (of research) may ultimately be constituted as a body of fact and theory, a discipline of sorts, in which individuals become specialists.'

But most educational researchers today no longer hold this faith in their power. Perhaps we have lost what was a guiding star to the pioneers, and a great source of strength, but it was always a myth. There is no one best method. Most empirical research studies are, to use Ashby's (1958) phrase, 'miniscule analyses'. This is hardly surprising: all science proceeds by 'miniscule analyses', and the building of a coherent theory is a very slow process on which we have only just begun. For the present, the problems which can be resolved empirically are relatively minor, compared with the major issues which require a judgment of values. Nevertheless, in these major issues, research has an important contribution to make, in defining objectives, in evaluation, in assembling relevant and adequate evidence on which to base our judgments.

And this takes me to the second category in my 'spectrum'. One of the distinctive developments of educational research in the past twenty years has been the recognition that too often educational decisions are made without an adequate knowledge base. A major area of achievement is the fact-finding survey type of research, such as the national reading surveys every four years (approximately) since 1948, or the National Child Development study, or the Isle of Wight study, from which we can tell how many children are deaf, stutter, have behaviour problems, are left-handed, wet the bed (and at what ages) and so on. All the major educational reports - Crowther, Newsom, Robbins, Plowden - have been accompanied by extensive surveys, and are much the better for it.

But there is also a potential weakness in this kind of research, for much of it is news rather than science:

Children in the North of England watch television more than children in the South of England;  
Eighteen per cent of junior school teachers do not know how to begin teaching reading;  
Thirty per cent of medical students live at home.

In the absence of theory or hypotheses, these are useless pieces of information. To quote one reviewer (Holmes, 1972):

At first sight ... research might appear to be thriving. But this impression results from the use of the term 'research' to cover work which might better be designated as ... development, survey or information. This umbrella usage tends to obscure the fact that controlled evaluative research ... is rare.

My third category is curriculum development, which is the largest single growth area since 1960. It occupies a middle position between my two extremes, drawing on theory and survey, using experimental work and field trials, prepared to venture into open-ended inquiries - as is appropriate for practical-oriented development work which attempts to bridge

the gap between theory and practice. If there is a danger of weakness here, it is that curriculum development is inclined to isolate itself as a special new kind of discipline, with its own specialists, its own techniques, its own jargon and even its own funding organisation, instead of recognising how much it has to gain from well designed experiment and evaluation on the one hand, and interventionist and exploratory-type studies on the other hand.

And so we come to the last two categories, which offer a promise of transforming the whole field of educational research, if only we can prevent them from being used by people as a short-cut to 'instant research', to avoid the trouble of thought and planning, or to cover up a lack of knowledge or a willingness to submit one's ideas to the test of hard evidence. It was Michael Young (1965) who pointed out the calamity of the gap between research and innovation; there is innovation without research - new ideas based on hunches, never tested objectively; and there is research without innovation - academic studies which make no impact and are unintelligible except to other researchers. Bringing research and innovation together in 'interventionist-type studies' gives 'action research', in which research monitors change, research is a guide to action, and the results of action are a guide to research.

Halsey's (1972) review of action research in Chapter 13 of *Educational Priority*, Volume 1, is as lucid an analysis as is to be found; and I doubt if there is much I can add to what is said there. As I see it, the problem is to keep research alive beside its dominant and vigorous partner; but the action men in the Educational Priority Area (EPA) studies saw the problem as winning freedom for action from the cold restrictions imposed by the researchers. The tension exists between the two concepts, action and research: action has all the popular qualities - commitment, involvement, belief, enthusiasm; the qualities needed in research have a more limited appeal - detachment, suspension of belief, scepticism. Or to use the vocabulary of Elizabeth Richardson (1975): for action, there must be loyalty, and loyalty is 'a collusion to maintain the pretence of infallibility'; but research requires a tolerance of heresy, 'a willingness to submit the most sacred ideas to the test of reality'.

Action research developed as a protest against the scientific detachment of traditional psychological and psychometric studies. This protest has now been carried to its logical extreme in the last of my five categories of research. On the anthropological model, to understand the educational process, to do any effective research in education, one must see it from the viewpoint of the learner. Traditional empirical research, especially when it involves testing, experiment and statistical analysis, starts from our assumptions, our framework of thought, and it imposes that framework on what we innocently call the 'subjects' in our experiments. Not surprisingly, the framework usually does not fit; and so we adjust our control mechanisms until we have a situation where we can use our preconceived models - and, not surprisingly, this kind of research produces results of limited value and limited application.

So we have a new style of research, and it is one which we must come to terms with. Just as psychology dominated the 'educational science' style, so sociology dominates this opposite extreme. Here it is important to build constructs on the basis of open-ended inquiry. The case study reveals the unique features of a situation. Participant observation enables the observer to get inside the skin of a situation, instead of studying it in a detached way. Grounded theory is built up from observation, not imposed *a priori*. The descriptions used by different participants to explain their experience provide an exploratory tool; and thus theory is grounded in the everyday life of the people who are being studied. Illuminative

evaluation uncovers the nature of what has happened; it does not prove that  $x$  is better or worse than  $y$  (How could it be? They are different, and comparison is irrelevant.).

This style of research, like the others, has its potential weaknesses. It can be an excuse for indiscriminate data collection, for tiresome transcripts of trite interview exchanges - research without ideas, which is research without interest, the anecdotal model, rather than the anthropological model. But at its best, this is a highly sophisticated and perceptive style of research (and I wish we knew how to teach the skills on which it depends). It can be even more demanding than the relatively straightforward laboratory experiment, and it may require the use of complex statistical analysis or elaborate procedures like the repertory grid.

We are often presented with this style of research as a challenge to the traditional model. To some extent it is; but my position is that no one of these styles is 'right', and none is altogether 'wrong'. The most effective research employs a variety of strategies, across the spectrum.

### *Structure in educational research*

Structure includes both the organisational structure of research funding and the infrastructure of research support, and here there is clearly a need for a meeting place like the British Educational Research Association, to bring interested parties into effective communication with each other. But many people would want to go further. For example, the Universities Council on the Education of Teachers, in a 1971 research policy document, stated:

There should be consideration at national level of the possibility of establishing better machinery than at present for the identification and discussion of research priorities and the co-ordination of research policies and initiatives.

Can we envisage the creation of an organised and integrated structure for educational research, for planning, funding, monitoring, for developing research support services, overseeing the provision of research training and financing postgraduate students? I confess that the creation of such a structure is a grand ambition - or, perhaps, a pipe-dream - which I have had myself from time to time. I suspect it was in the minds of some when the Schools Council was set up in 1964, and again when the Educational Research Board was established, in 1965. In both cases, if people had such expectations, they have been disappointed. But from time to time there is talk of creating an Educational Research Council, a supreme body which would oversee the work of the wide range of institutions concerned with research and development in education, integrating their activities and determining their priorities.

None of the bodies involved in research funding is enthusiastic about this kind of monolithic structure. Perhaps they are merely defending their vested interests, but the argument against it is well made in W. C. Radford's report, *Research into Education in Australia, 1972* :

Co-ordination is useful, provided it does not throttle intellectual independence and initiative. In the complexities of the social sciences, complete co-ordination of research would require omniscience and should never be attempted. (Quoted from Conrad, 1960)

The development of a subject is to a large extent a gradual uneven growth and the most that can be done is to ensure that the system ... discourages the growth from being too uneven or too gradual. (Quoted from Cunningham, 1972,)

Let me make clear immediately that I do not believe in the laying down of priorities in research by a central body ... My reason is simple. Such a laying down of priorities to me implies an impossible omniscience, and lays up trouble for itself ... Provided that those engaged in research develop adequate channels of communication between themselves ... I believe there will not be any greater gap between the nature of problems and the information from research available to solve them, than there would be were there to be a central determination of a limited number of priority areas in which alone study would be supported. It is as well to remember that, not very many years ago, 'education as investment' and 'manpower studies' loomed very much larger as matters of research than now seems warranted by later experience ... Had the major part of the research apparatus swung over to such studies in 1965 or 1966, a good deal of work now known to be more valuable would not have been done.

I am reminded of Berlyne's story (see Yates, 1971) of the response of an imaginary advisory council in 1810, asked to forecast the development of the transport system. 'One thing', they concluded, 'has stood the test of time over several thousand years: the horse has come to stay. Authorities as diverse as Genghis Khan, Dick Turpin, Julius Caesar and Buffalo Bill, all agree on one thing, from long experience, that there is no better way of getting from one place to another than on a horse.'

So the attitude of the Educational Research Board has been mainly responsive. 'Responsive' does not mean waiting for others to make suggestions: it means being ready to respond to imaginative ideas, and resisting the temptation to impose one's own ideas. Few other bodies are prepared to do this; few are able to do it with public money. Mrs Thatcher (then Secretary of State for Education), in 1970, expressed the directly opposite view for DES-sponsored research:

There was clearly only one direction that the Department's research policy could sensibly take. It had to move from a basis of patronage - the rather passive support of ideas which were essentially other people's, related to problems which were often of other people's choosing - to a basis of commission. This meant the active initiation of work by the Department on problems of its own choosing, within a procedure and timetable which were relevant to its needs. Above all, it meant focusing much more on issues which offered a real possibility of yielding useable conclusions.

This is an appropriate view for a government department, but it is also appropriate for a body like ERB to be prepared to operate outside the limits of established policy. The 1973 report of ERB said:

When the Board reviewed the problem early in 1973, its decision was to reaffirm the 1971 policy statement, that whereas 'the other major bodies are chiefly concerned with policy-oriented research, SSRC should have a more basic and intellectually innovating role'. The distinction between policy-oriented and basic research is open to question, and there is no implication in the statement quoted that SSRC is interested only in 'pure' social science inquiries: the intention is to bring out an emphasis on the theoretical contribution which each approved project should make as at least part of its results. This is an aspect

of educational research which does not seem to be adequately dealt with by the other funding agencies.

There is a place for both approaches and a need for partnership between them. There is a place also for each, occasionally, to do something of the other's role; thus, the ERB has recently embarked on the task of instituting a programme of research in pre-school education, and this will be developed in conjunction with the national programme of the DES, DHSS and the Scottish Education Department.

There is, however, one aspect of the organisation of educational research where there is an obligation - an urgent need - to undertake positive initiatives. This is to build up an extensive *infrastructure* for research and development. William Taylor (1972) has argued this point persuasively in writing and in speeches: the most effective way to improve the quality of educational research, he says, is to build an adequate 'research floor' - funds, equipment, personnel, procedures, training programmes, communication, information retrieval, and so on. To take only one example, information retrieval, the Educational Research Board brought together the representatives of fifteen different organisations to agree on a common format for abstracts of research; and with feasibility studies begun by the National Foundation for Educational Research in England and Wales and the Scottish Council for Research in Education, working in collaboration with the Council of Europe EUDISED thesaurus of educational terms, we have the basis for a possible information retrieval service covering the whole European scene, to match the ERIC system on the other side of the Atlantic.

To summarise, I think that recent years have seen a move away from the naive idea that problems are solved by educational research; that is the old educational science' idea, and it is a myth. Educational research can strengthen the information base of decision-making; its procedures of inquiry and evaluation can inject rigour into the flabby educational thinking which has satisfied us for too long in the past. The most important contribution of research is, I suggest, indirect. This is important: in one sense, educational researchers are the unacknowledged legislators of the next generation. As Taylor says in his book *Research Perspectives in Education* (1973) a primary function of research in education is to sensitise - to make people aware of problems. Also, in assessing the achievements of educational research, we have to consider its effect on the attitude of those who teach. Vigorous research activity or, to use a less pretentious title, investigation into teaching and learning, sharpens thinking, directs attention to important issues, clarifies problems, encourages debate and the exchange of views, and thus deepens understanding, prevents ossification of thinking, promotes flexibility and adaptation to changing demands. Research of this kind aims to increase the problem-solving capacity of the educational system, rather than to provide final answers to questions, or objective evidence to settle controversies. On this view, educational research is a mode of thinking rather than a shortcut to answers. In the long run, the real influence of educational research is through its effect on the attitudes of those who teach.



## REFERENCES

- ASHBY, E. (1958) *Technology and the Academics*. London: Macmillan.
- BAIN, A. (1879) *Education as a Science*. London: Kegan Paul
- BURT, C. (1921) *Mental and Scholastic Tests*. London. King and Staples.
- BUTCHER, H.J. and PONT, H.B. (eds) (1968, 1970, 1973) *Educational Research in Britain*, Vols 1, 2 and 3. London . University of London Press.
- CONRAD, A. (1960) See Radford (1973) footnote, p. 6.
- CUNNINGHAM, C. (1972) 'A research policy for economics', *SSRC Newsletter*, 14 March 1972.
- FLEMING, C. M. (1946) *Research and the Basic Curriculum*. London: University of London Press.
- HALSEY, A.H. (1972) *Educational Priority*, Vol. 1. London: HMSO
- HOLMES, E. (1972) Book review in *Br. J. educ. Psychol.*, **42**, 92.
- NISBET, J.D. and ENTWISTLE, N.J.(1966)*The Age of Transfer to Secondary Education*. London: University of London Press.
- NISBET, J. D. and ENTWISTLE, N. J. (1969) *The Transition to Secondary Education*. London: University of London Press.
- RADFORD, W. C. (1973) *Research into Education in Australia, 1972*. Canberra: Australian Government Publishing Service.
- RICHARDSON, E. (1975) *Authority and Organisation in the Secondary School*. London: Macmillan.
- RUSK, R. R. (1913) *Introduction to Experimental Education*. London: Longmans Green.
- SANFORD, N. (1964) *The American College*. New York: Wiley.
- TAYLOR, W. (1972) 'Retrospect and prospect in educational research', *Educ. Res.* **15**, 3-9
- TAYLOR, W. (ed.) (1973) *Research Perspectives in Education*. London: Routledge and Kegan Paul.
- THOULESS, R. H. (1969) *Map of Educational Research*. Slough: NFER.
- VERNON, P. E. (1 940) *The Measurement of Abilities*. London: University of London Press.
- WALL, W. D. (1962.) 'Educational research today: **IV**.-Resistances, organisation and costs', *Brit. J. educ. Psychol.*, **32**, 223-33.
- WARD, A. V. (1973) *Resources for Educational Research and Development*. Slough: NFER.
- WHIPPLE, G. M. (1912) *Manual of Mental and Physical Tests*, Vol. 1. Baltimore: Warwick and York.
- YATES, A. (1971) *The Role of Research in Educational Change*. Palo Alto: Pacific Books.
- YOUNG, M. (1965)*Innovation and Research in Education*. London: Routledge and Kegan Paul.