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<td>Published by</td>
<td>Institute of Education (Singapore)</td>
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Research on Teacher Education

The title of a lecture or address is often the least important part of it. Sometimes, indeed, it is almost an after-thought. In the case of my address today, however, the words of the title are important. What one puts between 'research' and 'teacher education' — on, or and, or in — makes a great deal of difference to the subject matter.

Research on teacher education I take to be systematic attempts to collect facts about, to interpret, and to develop theories concerning the formal efforts of education systems to educate and train teachers for their tasks in classroom and school.

Research in teacher education would, as I see it, be about how we might help future teachers to appreciate the values and limitations of research — in their own specialist subjects, not just in Education — how to use research findings, and how to carry out and participate in research that will improve their professional practice.

Research and teacher education has to do with all the kinds of research — psychological, sociological, historical, philosophical — that are relevant to the study and improvement of the organisations, processes, and outcomes that constitute teacher education programmes.

So, to a greater extent than usual in the title of a lecture, the choice of words defines rather precisely the focus and limitations of what follows. I shall be concerned with research on teacher education. Some of this may be relevant to the part that research can play in the education and training of teachers, and to a better understanding of the relation of research and teacher education. But these are separate and larger subjects. I make no claim to deal with them today.

The Scale of the Effort

The sharp contrast in the volume of materials relevant to a lecture entitled 'Research on Teacher Education' as compared with one that has to do with research and teacher education deserves comment. I have recently been examining, for purpose of review, the latest (fifth) edition of the Encyclopedia of Educational Research. Sponsored by the American Educational Research Association, (AERA), the Encyclopedia first appeared in 1941. There were new editions in 1950, 1960, and 1969. Now we have the fifth. It comes in four large volumes, containing 2,156 double column A4 (8" x 11") size pages. It is, I would stress, an encyclopedia of research, not of 'Education' as such. Nearly all its many hundreds of authoritative articles contain material relevant to how we go about the education and training of teachers. A very much smaller number are relevant to a lecture about research on teacher education. Perhaps in one sense both you and I should be grateful for that. But in another, it is perhaps rather disturbing that, in relation to the great body of literature that exists on other aspects of Education, and on subjects other than Education, rather little time and effort has been devoted to the systematic study how we educate and train the men and women whose work makes for the success or the failure of our schools.

Why is this so? Why, when one can find a massive research literature on so many other aspects of Education, are the pickings in respect of teacher education so meagre? Some might allege that it is due to the low status of work in the specialism, low even by the modest standards of educational research in general. Others would point to the small numbers of those engaged in universities, colleges and institutes of education who are directly concerned with the process of teacher education itself, as distinct from the school subjects that they profess, or the educational specialisms — psychology, sociology, and so on — that they have been hired to teach.

It might be argued that really high quality research needs early commitment, as is made by first-class honours graduates in physics, chemistry, languages or the social sciences. Research on teacher education does not fit in to any of the existing disciplinary paradigms, and is unlikely to appeal as a subject to bright young graduates. Instead, much of the work that does get done is the product of the part-time labours of ex-school teachers who have acquired higher qualifications and been appointed to posts in schools and institutes of education. It is easy to point to the lack of funds available for research on the topic, and the urgent priorities that governments and research bodies have attached to other matters, such as the improvement of reading, mathematical skills, the education of the handicapped, the development of positive attitudes towards industry and productive work. All these points are indeed relevant to understanding why comparatively little systematic research on teacher education has been undertaken but
they do not in themselves suffice.

There are two principal factors that interact together to account for our lack of systematic research effort in this field. They are, first, the nature of our beliefs about the possibilities and limitations of teacher education, and second, the reaction of disciplinary specialists to large, messy, multi-disciplinary problems, involving very large numbers of disparate variables, such as the teacher education scene presents. These beliefs and perceptions interact and reinforce each other.

In nearly all systems, there is at least some measure of scepticism about the very concept of trying to 'train' a teacher. The higher the status of the teaching activity involved, the greater the scepticism. It shows itself at the one extreme in the old saw that 'teachers are born and not made', and, at the other, in the weight that researchers themselves are often willing to attach to 'situational variables' in the analysis of teacher performance of learning outcomes. Scepticism, and a necessarily unpredictable 'situation' reflect and reinforce pessimism about our ability to disentangle the complex interactions between personality, disposition, acquired knowledge and pedagogic skills on the part of the teacher, and motivation, response, attitudes to schooling and capacity for learning that characterise students in classroom and school settings.

Such scepticism and pessimism do little to encourage large-scale expenditures on research. If the outcomes of training are so much less important than the effects of, for example, personality and prior experience, and the contexts in which the teacher's task is performed are so variable and unpredictable, what is the point of spending a lot of money to obtain facts and findings that may be of little practical value and which are incapable of application in the real world of schools and classroom?

Raths and Katz (1982) on the basis of a recent survey of 88 teacher education instructors, came to the following somewhat gloomy conclusion:

'One response suggested in our findings is that instructors believe that what contributes significantly to successful teaching are dispositions and other personal qualities which, in fact, cannot be taught in methods classes. This puts them in the position — no doubt a moralising one — of believing that which is more useful in the repertoire of the teacher cannot be taught by them, or perhaps by anyone, and conversely that what can be taught is alright, but tangential and insignificant. If this characterisation of the thinking of pedagogues themselves is close to the truth, they may be caught in a recursive cycle of events and behaviour something like the following: an instructor believes that what she can achieve is not very useful or significant, which in turn leads to low or diminished effort in attempting new techniques or experimenting with alternative approaches, which in turn leads to low impact, followed by low effort, and a recursive cycle of low effort — low impact — low effort that is difficult to break. What may be missing, among other things, is educational and intellectual leadership within the institution and profession which would attempt to break the cycle, clarify the actual reward structure, change it if necessary, and confront some of the issues implied in trying to please students, colleagues and the ultimate clients, i.e. the schools, in a period of diminishing resources and decreasing public confidence' (p. 282-283).

Such scepticism on the part of teacher education practitioners, if general, raises serious questions, which deserve a considered response.

The Scope of Research on Teacher Education

Whilst useful and important work has been done in recent years, particularly in the United States, which identifies and quantifies relationships between what have been called 'presage', 'process', and 'product', variables, we should not take such work as defining the field of enquiry of teacher education research, nor should we be too optimistic about its capacity to influence the organisation, content and outcomes of teacher education programmes. Drummond and Andrews (1980) are among many authors who have noted how little changed teacher education programmes are today from those which existed half a century ago. Houston and Newman (1982) state:

'Despite the wide range of institutions within which teacher education programmes were conducted, they were amazingly similar and relatively unchanged during the past fifty years. . . . This similarity might be attributed to teacher certification requirements, common requirements for the Baccalaureate degree, expanding governmental mandates for social reform, and the extensive communication network among educators' (p. 1881).

This is, of course, a judgment about the overall structure of the programmes, about the proportions of time and attention given to e.g. general education, the teaching specialisation, educational foundations, field experiences and pedagogy.
An interesting recent development is provided by the claims now being made by *subject* staff in institutes and departments of education for the significance of research in their own area of teaching as a way of enhancing the psychological, sociological, and other Foundations knowledge of students. This also, of course, enables the staff concerned to claim a larger proportion of available teaching time for their subject.

'... much of the work in teacher training that was undertaken in the general educational theory context can now be approached much more meaningfully through the mathematics-education context. One would therefore expect, and hope, to see the future mathematics teacher spending more training time in specifically mathematics-education activities and less in the areas of the individual educational disciplines' (Bishop, 1982 p 132).

Given that, in general purpose teacher education institutions, students must acquire some knowledge of what they are going to teach, must develop an understanding of children, of the learning process, and of what works well and what doesn't as far as teaching methods are concerned, must try out their knowledge and ideas in actual classrooms, and must make some kind of selection from the range of things they can specialise in, it is unsurprising that a teacher educator fifty years ago would have no difficulty in recognising most of the programmes on offer today. I have argued elsewhere that it is a mistake to base perceptions of stability and change on organisational appearances. Oxford and Cambridge Universities are still structured today very much as they have been over the past 500 years. But the organisation and content of their teaching and research have changed out of all recognition.

In teacher education a great deal has changed as far as content and approach are concerned. But how much of what has changed is a response to research on teacher education? In some places, not a lot. Elsewhere, particularly in some institutions in the United States, where there has been a systematic attempt to incorporate the outcomes of, for example, behaviouristically-inclined studies into learning modules for specific teaching skills, such as questioning, motivating response, and diagnosing learning difficulties, the direct impact of research is more visible. Such research efforts, however valuable, do not provide us with a base on which to design, organise and conduct programmes of teacher preparation. They do not pretend to do so. Our model of what constitutes research on teacher education needs to be a good deal broader, taking in work on people — staff and students — on curriculum, (including practical teaching), on questions of teacher demand and supply, on the relation of in-service work to initial training, on the means of assessment we use in selecting and certifying students, on the analysis of institutional cultures, and on the part that teachers play in strengthening, sustaining — or even undermining — particular orientations and emphasis within pluralist societies. The research agenda is as broad as the activity of teacher education itself.

To take such a position might seem to favour the view of those who throw up their hands in despair at the number and complexity of the variables involved, or who even denigrate the role that research-based knowledge might play in the improvement of teacher preparation. This is a risk that must be taken. To recognise that the scope of research on teacher preparation is much broader than work on carefully selected and narrowly defined elements in performance and outcome, if it makes even more obvious the large gaps that exist in the knowledge base of the activity, does at least offer the prospect of findings that can be read and understood by those who administer and teach within the system.

An illustration is provided by work on the origins, qualifications, experience and attitudes of staff of colleges, departments and institutes. It is a matter of importance that we have the fullest possible information about the men and women who are engaged in the process of preparing teachers. Without such information, based upon careful systematic research and frequently updated, we are likely to act upon misconceived hunches and impressions. This is not just a matter of surveying the number of years classroom experience that lecturers in Education possess, or the number of occasions during the last twelve months in which they have taught a class under normal conditions, or the level or the qualifications in relation to the courses for which they are responsible, and so forth, although even on these obvious aspects of the matter we have all too little up-to-date information. Interesting general questions also arise, such as the extent to which the knowledge and skill required to be an effective educator of teachers are specific to that task, and develop and become more effective through experience as a teacher educator, rather than simply as a classroom teacher. In other words, what is the trade-off between recent and continuing class teaching experience on the one hand, and the forms of understanding that an experienced lecturer acquires through years of experience in a college or institute on the other?

In discussing such issues, there is a tendency to employ metaphors from other professions that
may or may not be valuable. The teaching hospital is often cited. Professors of surgery, it is contended, would soon lose credibility if they ceased to carry out operations. Surely, therefore, those who claim to prepare teachers to work in classrooms need to be active practitioners with children of relevant ages, not simply with tertiary students? Perhaps so. And then again, perhaps not. I raise this question, not to offer an answer, but to suggest that simplistic metaphorical comparisons, between occupations which are very different in terms of their technologies, their knowledge bases, their status and their organisation, are not necessarily the best way to inform sensible decisions.

Research on teacher educators is an essential aspect of research on teacher education. Its pursuit implies a model of teacher education research with a wider range than, for example, that specified some years ago by Ned Gage (1972 p 141) in his valuable study *Teacher Effectiveness and Teacher Behaviour*:

'Research on teacher education is that in which teachers' behaviours and characteristics are dependent variables, and ways of recruiting, selecting, and educating teachers are independent variables'.

The somewhat looser definition offered by B O Smith (1971 p 2) at about the same time would encompass more of the work with which I am concerned, but is still too narrow for my purpose.

'What is research on teacher education? In a sense this question is naive, for everyone must know already what teacher education is, and that research on it is simply the systematic study of problems that arise in the course of carrying it on. Generally speaking, research on teacher education attempts to answer the question of how the behaviour of an individual in preparation for teaching can be made to conform to acceptable patterns'.

We do need carefully defined, closely focussed research, directed to identifying relationships between inputs made during programmes of teacher preparation, the development of teacher competencies, teacher performance and student outcomes, such as have been developed by a number of individuals, research projects and university departments in the United States and elsewhere. But if research on teacher education is to benefit the process that it seeks to analyse, it must take in a much broader range of work, some of it necessarily much looser, more impressionistic and wider in scope, designed to extend and deepen the base of systematic knowledge on which judgments and decisions about numbers and supply, selection and recruitment, curriculum and assessment, certification and induction, in-service refreshment and training for specialised roles, are organised and conducted.

**Research and Curriculum Development in Teacher Education**

What place is there within the concept of research on teacher education adopted in this lecture for large-scale development work such as went on in a number of American universities during the late 'sixties and early 'seventies, and which, on a more modest scale, is still being pursued today in the United States, Australia, Great Britain and elsewhere in the English speaking world?

It may be helpful to refresh our minds as to the content of some of the developmental models involved. They focussed upon the improvement of teaching in primary schools, and were initially supported by the US Office of Education. It had originally been intended to design a variety of innovative models, and then to test each of them with a view to selecting those for implementation that satisfied stringent success criteria. The testing state was not in fact undertaken, although many of the models received wide publicity, and exerted influence beyond the programmes of the institutions from which they originated. Joyce (1972) (quoted by Houston and Newman (1982)) summarised the different approaches as focussing upon the teacher as an instructional manager (Florida State); objectives-based teacher education (Georgia); human relations, teaching skills and content-based teacher education (Mass.); the teacher as a behavioural scientist (Michigan State); the teacher as a producer of learnign (Northwest Regional Laboratory); individualised instruction (Pittsburgh); intent, action, feed-back and modification in the teaching cycle (Syracuse); the teacher as innovater (Teachers College, Columbia); and the teacher as a team member (Toledo).

Houston and Newman (1982 p 1187) suggest that there were several common elements in these models:

'Teachers were viewed as clinicians (in the medical sense), applied behavioural scientists, and members of a co-operative team. The models assumed that teacher competencies and behaviours could be defined, and programmes were proposed to teach mastery of the objectives largely through modularised instruction. All relied on the use of simulation laboratories and proposed long periods of training on an pre-service — in-service continuum'.

It seems likely that the distinctiveness of these models has been diluted by time, staff mobility,
and experience. The popularity of some of the methods of instruction used by these programmes waxed and waned over a relatively short period in the late 'sixties and 'seventies. In the early part of the decade, it was estimated that 77 per cent of teacher education programmes in the United States included a component of micro teaching, invented by Allen and Ryan at Stanford ten years earlier. Competency and performance based teacher education (C/PBTE) caught on fast and were mandated in a number of States. By 1977 only just over a quarter of all teacher preparing institutions were not involved in some way in the implementation or exploration of CBTE programmes. This uninvolved proportion had grown to 41 per cent four years later. Projects were funded to develop 'protocol materials', whereby classroom situations were represented on tape, film, in print and by other means as a way of focussing student attention on actual classroom problems and providing opportunities for tutor and students to apply analysis and interpretation in accordance with their other work in foundations, curriculum studies and pedagogy. Many institutions made widespread use of simulation and 'critical incident' techniques, presenting open-ended problems, derived from practice, in case form. Here again, whereas more than 60 per cent of teacher education programmes were using simulation in the early 'seventies, by the middle of the decade the proportion had fallen by nearly half.

These models and techniques gained their rapid penetration from four main sources.

First, generous funding was available, usually from Government sources, based upon recognition of the need to up-grade teacher preparation and development in line with expansion of educational provision, a thrust to achieve greater educational equality, especially for hitherto under-privileged groups, and curriculum change.

Second, great hopes were invested in the possibilities of instructional technologies based upon behavioural psychology, free of long-standing and inhibiting disputes about values and purposes.

Third, education authorities found themselves under pressure to be more accountable to their publics, and tended to respond favourably to approaches that offered measurable and quantifiable 'results'.

Fourth, the availability of cheap audio reproduction and the video camera, plus the greater portability of equipment, and cheaper reprographics, permitted and encouraged the development of such techniques as micro-teaching, simulation and protocol materials in ways that hardly would have been possible a generation earlier.

Even if the number of institutions that consciously base their work on one or more teacher education 'models', or make systematic use of particular techniques is probably smaller than in the middle 'seventies, all of these developments have left their mark on the teacher education scene. What were formerly seen and claimed by some as major break throughs have been re-evaluated as partial, but nonetheless useful, methods. Programmes have become more eclectic, but many still show signs of the systematic development work that went on from the mid 'sixties to mid 'seventies. Much of this work must be seen for what it was — development, rather than research. In a recent article on what has survived of C/PBTE in New York, Wendel (1982) has argued that six specific facets remain.

"(1) Criterion-referenced assessment with extensive use of behavioural objectives remains. (2) The assessment of student learnings in the "methods" courses is accomplished more frequently with performance measures than with paper and pencil tests. (3) Student rewards of "passed" competencies are maintained in an appropriate office. (4) Professors of education are working closely with classroom teachers during field experiences. (5) Competencies in the affective domain are not ignored by education faculty members. (6) Education faculty members are in regular communication with each other for planning, conducting, and evaluating student experiences." (p30)

Apparently the concept of student self pacing utilizing modular approaches has not been widely adopted; there is less individualized instruction than had been hoped, and schools have not adopted C/PBTE on the basis of college student field experiences.

Evaluation was always recognised as one of the greatest problems of innovations of this kind, and was seldom attempted on a scale and with adequate thoroughness to validate one approach or method against another. In particular, the impact of much of this development work was blunted by the absence of generalisable criteria of teacher effectiveness, a subject on which a great deal of research effort has been expended.

Teaching effectiveness research is really a sub-set of the larger category of 'research on teaching'. Dunkin and Biddle (1974 p 3) are rigorous in their definition of what research on teaching comprises.

For them, no study is included that does not involve 'systematic observation of teaching in classrooms'. This excludes: 'Investigations conducted with white rats, planaria, or pre-school children, observa-
tions of teacher and pupil behaviour in non-
classroom contexts, studies of the back-
grounds and characteristics of teachers, 
pupils or others concerned with education 
except as that have been related to class-
room behaviour; field surveys of school 
characteristics such as size of classrooms 
or per pupil expenditures unless they have 
included observations of classroom 
behaviour; experiments in which an innova-
tion is introduced into the classroom (such 
as a new curriculum or teaching device) 
and evaluated against the criterion of pupil 
achievement without study of the actual 
teaching process with which it was 
mediated; investigations in which teachers, 
pupils, school principals, or others are 
asked to report or rate classroom events 
which they saw — but the investigators did 
not'.

Earlier work on teacher effectiveness was 
heavily biased in the direction of listing personal 
characteristics deemed by large numbers of 
observers to be associated with 'effectiveness', 
such as the qualities of adaptability, considerate-
ness, enthusiasm, good judgement, honesty and 
magnetism that emerged as the top six charac-
teristics from the Commonwealth Teacher Train-
ing Study (Charters and Waples, 1929), or were 
based upon rating scales applied by different 
observers in large numbers of classrooms.

The unreliability and lack of validity of such 
procedures was increasingly recognised. In the 
late 'fifties and early 'sixties efforts were made to 
refine the categories employed and to use what 
Gage (1972) and others have called 'low in-
fERENCE' observation schedules, relying upon 
clear instances of specific teaching behaviours, 
capable of being recorded with a high degree of 
reliability.

A great number of studies published in the 
'seventies used these schedules as part of what 
has come to be known as 'process-product' 
research and which classified the elements of 
teaching and learning in a much less ambiguous 
and more objective fashion than had hitherto 
been the case. Medley (1982 p 1899) has 
suggested that the definition of teacher effective-
ness requires systematic attention to be given to 
nine important types of variables, and the rel-
ations between them. The five that are labelled 
'pre-existing teacher characteristics', 'teacher 
competencies', 'teacher performance', 'pupil 
learning experiences', and 'pupil learning out-
comes' are influenced at particular stages by the 
processes of teacher training, the nature of the 
external context of performance, the internal 
classroom situation and the characteristics of 
individual pupils.

Medley (1982 p 1900-1901) has summarised 
the way in which research designs employing the 
concepts might be applied in the following terms: 
(See Appendix 2)

'The principal aim of research in teacher 
effectiveness is to add to knowledge of 
inter-relationships along the upper line of 
the diagram . . . so that information about 
variables in each cell can be used by 
decision makers to maximise the pupil 
learning outcomes each teacher produces. 
The general strategy for the research is to 
terelate variables in adjacent cells, 
taking into account variables in the off-line 
cell that directly affect the relationship. 
Thus teacher competencies and teacher 
performance variables might be inter-re-
lated, making due allowance for external 
context variables'.

Teacher effectiveness is obviously of signi-
ficance for teacher education. So is the develop-
ment work on models of teaching that went on in 
the US during the early 'seventies. Both can make 
a contribution to, neither defines the field of, 
research on teacher education.

**Audience and Impact**

What impact does research on teacher educa-
tion have? What is the audience for such re-
search?

In principle, research on teacher education 
should enable those who plan and administer 
institutions, organise programmes, write sylla-
buses and teach and assess students, to inform 
their judgments and decisions with adequate 
rebodies of relevant fact, employ more highly 
refined concepts in their discourse about Educa-
tion, and order their perceptions, analyses, inter-
pretations and actions in terms of theories with 
high explanatory power, and appropriate 
consistency and coherence.

In practice, one suspects that life is not quite 
like this. Research does not simply yield 'find-
ings' which can be 'applied'. To think that it does 
is to adopt a simplistic, scientific, over-rational 
model of decision making and change that bears 
little relation to social, political and organisa-
tional reality. If we are wise, we do not look to 
research simply for its results. The existence of 
research projects relevant to the problems of 
teacher education, conducted by competent 
workers, subject to appropriate forms of 
accountability and with adequate means for 
 dissemination, does much more than simply 
yield 'results'. There are several additional 
benefits.

First, the willingness of a system to devote 
resources to research, and to trust those con-
cerned to be rigorous, discriminating, frank and truthful in undertaking and reporting upon their work, even if the overall scale of this enterprise remains modest, is an important earnest of intention. It signifies a willingness to be open to new ideas, to modify practices and procedures in the light of new knowledge, a desire to benefit from the work that has been done and is being done by others with similar interests and commitments. Only a fool believes that all educational judgments and decisions can be determined, eventually, by research-based knowledge. Manifestly they cannot. But there are many arenas in which costly mistakes could be avoided and useful gains made by taking into account the outcomes of educational research, especially when these are of a counter-intuitive nature.

Second, the existence of research effort in particular subjects helps to sensitize planners and practitioners to the existence of phenomena and variables of which they might otherwise have remained ignorant or unsuspecting. Attitudes to secondary school organisation and student support in the United States and Europe have undoubtedly been influenced by the results of studies of the family origins of students, their regional distribution, the relation of their social backgrounds to the kinds of job they attain, and the relative success in the earnings league of people with various levels of qualification.

Third, the existence of research in a given field creates or contributes to a network of men and women with related interests and concern. Given an appropriate institutional setting, researchers themselves are able to try out their ideas on colleagues and to benefit from interdisciplinary discussion and exchange, both formal and informal. Problems are shared, interim reports discussed and findings dissected at seminars, workshops, meetings and conferences, each expanding the number of those familiar with the existence of the work.

Fourth, the experience that an individual or research team has in using particular techniques, of designing research instruments, collecting and classifying data, and interpreting the results of their analyses in terms of earlier and other ongoing work, makes its contribution to the refinement and improvement of methodology, to the avoidance of error, and to the likelihood that further work will yield useful outcomes.

Fifth, small-scale studies, which appear to add little to the sum of our knowledge about educational process and practice, and to be of interest only to other researchers working in a very narrowly defined field — provided that they satisfy appropriate methodological criteria — add another small brick to the edifice of knowledge. By means of what Glass and McGaw et al. (1981) call ‘meta-analysis’, it is possible to draw together large numbers of studies on cognate themes, with a view to determining whether the data indicates the presence of a regularity or tendency, the existence of which could certainly not be sustained on the basis of any one of the studies, but which taken together they may be seen to support. It is simply not possible for every piece of work that is done, whether by an individual or by a team, to be targeted to a specific outcome or improvement. Take for example my own card systems, which I use as the basis for all that I write — including this lecture! For many years I have used 8” x 5” cards to write down notes on all that I read, to provide a record of quotations and references, and to list thoughts and queries that arise and which require further exploration. A good deal of this note taking is done for specific projects — preparing a chapter for a book, writing an address, searching literature in connection with a particular piece of small-scale research. But when I come actually to write the chapter, or prepare the lecture, or write up the literature review, I find that my collection of cards yields much more valuable material than those notes prepared for this specific purpose. There is a need for serendipitous reading and note taking, without any specific purpose in mind, of a kind that yields a back-up store of material, the relevance of which can only be perceived and drawn upon in relation to a later, more specific, project or undertaking.

It is impossible to say, at the time of reading a particular book or filling in a particular card, what use might subsequently be made of it. That does not mean that it is useless. Only by maintaining a wide and catholic range of reading in the literature of Education can one be in a position to place specific issues and incidents in an appropriate context, and to see their potentiality in research terms.

The academic needs ‘leisure’, and freedom from too many explicit commitments, not in order to cultivate his garden, or amuse himself with the sheer enjoyment of books, (though there is nothing wrong with either of those) but to undertake the reading, thinking and testing out of ideas with colleagues that constitutes the essential condition for subsequently undertaking successful research on topics and problems yet to be defined.

What, then, is the audience for educational research in general and teacher education research in particular? A common colloquial criticism suggests that the most frequently encountered of all research findings is that more research on the topic at issue is necessary. The audience for the work of one education researcher is thus other persons with similar interests and involvement. Like all such criticisms, this one contains elements of both truth and
over-simplification. Truth, because there are serious weaknesses in much research in our field, less today in methodology and the manipulation of data (both of which have been much helped by the availability of low cost computing facilities), more in the original statement of problems and the taxonomising of variables. To criticise research on the grounds that too many researchers write only for others of their same calling is, however, an over-simplification. It too readily assumes that the ‘results’ of a research project should in some way be available for ‘use’ by practitioners. It also neglects the vital functions of clarification and replication.

In planning a programme of work individuals or research teams obtain from their literature searches and from close study of earlier related efforts a much clearer picture than would otherwise be available to them of the direction their efforts might take if useful results are to be generated, of pitfalls to be avoided and possibilities to be explored. In these senses, very little research, even bad research, is sheer ‘waste’. The biggest danger from bad research is not its existence, but lack of systematic evaluation to enable us to judge the way in which it is bad. Another weakness of research efforts in Education is the lack of systematic replication. There are simply too many instruments, designs, models, frameworks, — the terms seem almost interchangeable — and too few are subjected to the rigorous evaluation and replication needed to establish validity and usefulness. Unfortunately, when such evaluation does take place, specially in respect of research that has obtained some measure of public attention, it is often then implied that the results are so flawed that they can be disregarded. The likelihood of a sensible appreciation of the strengths and weaknesses of particular findings is not enhanced when public interest establishes a wholly inappropriate context for the reception of the findings. Professor Entwistle (1976) has commented in relation to Neville Bennett’s Teaching Styles and Pupil Progress, the publication of which in the mid ‘seventies attracted a good deal of press comment:

‘It came . . . as something of a shock that after one of the first popular reports of the early findings, newspapers proclaimed the message that primary teachers were moving back to traditional methods. The dangers in moving from research evidence to unfounded conjectures are clearly illustrated here. Our findings suggest that no more than 17 per cent of teachers in our sample were using the progressive methods so enthusiastically endorsed by the Plowden Committee. Our results said nothing about changes in methods, but the newspaper reports had assumed that after Plowden in 1967 there had been a strong move in the direction of progressive methods. There is, in fact, little evidence to support this view.’ (p viii)

There is a prevalent belief in our field that anyone contemplating a PhD or other research project should start from scratch, rather than attempt through replication to confirm or disconfirm earlier studies. Very little of the educational research published in the journals and elsewhere is subject to any form of rigorous systematic appraisal. Its ‘findings’ tend in varying degree to find their way into the thinking and beliefs of the research and education community, often to the detriment of high quality future work, and, when the Emperor is eventually seen to have no clothes, to the status of educational research as an activity.

If the hopes that I have expressed earlier in this lecture that research on teacher education will eventually lead to an improvement in the quality of both pre-service and in-service teacher education are to be fulfilled, effective links have somehow to be established between the work done by researchers and the decisions of classroom practitioners. To believe that this is secured by persuading students in colleges and Institutes of Education to read the Education journals and to ‘apply’ what they read to their practice is not simply naive, it is positively dangerous. If the research community itself is not sufficiently critical about its own products, teacher education students can hardly be expected to apply adequate criteria of their own to assess the reliability and validity of what they read or hear about. They pick up, as do we all, particular findings and outcomes that appear to justify existing or emergent practices, which are then quoted as a scientific justification for pre-existing judgments, for prejudices or for established habitual imitations of other practitioners. Many teachers also keep in memory, for use as appropriate, examples of what because it sharply conflicts with their own beliefs or habitual procedures they regard as ‘useless’ research. This hardly constitutes a considered application of research to practice, of a kind that we might wish to encourage and make more general.

Clearly, students in initial training are not going to have ‘time, quite apart from anything else, to devote to reading research reports over the wide range of problems that fall within their sphere of practice. For the most part, they make no attempt to do so. What does happen is that authors of monographs and textbooks make themselves familiar with the literature on a particular topic, and incorporate this in either raw or processed — sometimes, alas, rather half baked — form into their writings. Staff draw upon these
Using the Findings of Teacher Education Research

A statement by McIntyre (1980), who has himself made some significant contributions to research in our field, indicates the doubts that many now have about the manner in which findings from research on teaching can be employed for the improvement of teacher education.

'As recently as 1970 some of us, for example those associated with micro teaching, thought that the findings of research on teaching could form the basis of a body of theoretical knowledge which would, more adequately than general social science theories, generate prescriptive principles which could form the core of a theoretical/practical teacher education programme. We were wrong. Not only has research on teaching so far failed to produce any such body of knowledge; but further reflection on this research ... leads to the conclusion that it will not be possible to produce such a body of knowledge. My conclusion, therefore, is that there is not, nor could there be, any systematic corpus of theoretical knowledge from which prescriptions for teaching can be generated'.

In common with other workers in these fields, McIntyre comes to the conclusion that researchers need to move much closer towards teachers' own perspectives and definitions of their situation. Teacher education research here reflects the tensions that developed in the 'seventies between, on the one hand, the positivist tradition of empirical studies of teacher behaviour, and on the other, phenomenologically-inspired efforts to see problems as teachers saw them, and from this to generate research directed towards interpretation and action.

As might perhaps be expected, these two approaches tend to yield rather different findings, even when directed towards the same subject matter. It has been argued, for example, (Brophy 1979) that classroom research indicates that effective teaching is characterised by direct instruction, high expectation, appropriate pacing, and the development of context specific relations. The 'direct instruction' referred to by Brophy is defined by Rosenshine (1979) as academically focussed, teacher directed classrooms using sequence and structured material. The classroom atmosphere is businesslike, task oriented, well controlled. Goals are clear, monitoring and assessment are much in evidence, the teacher is very much in charge. Children spend up to 70 per cent of their time working alone. As Rosenshine admits:

'There is something grim about this picture of direct instruction: large groups, decision making by the teacher, limited choice of materials and activities by students, orderliness, factual questions, limited exploration of ideas, drill and high percentages of
correct answers... The balance of the data until now does not show negative side effects' (p 47).

Such conclusions have not gone uncriticised. Peterson (1979) argues that 'effectiveness' cannot be assessed without reference to the goals of the teacher and the schools. If these are strongly related to achievement, then direct instruction may indeed succeed. But if the school is concerned with 'creativity, independence, curiosity, and favourable attitudes towards school and learning... then a more open and indirect style of teaching seems most appropriate.

It has been clear for some time, at least in the United States and the United Kingdom, that the practice and beliefs of large numbers of teachers do not coincide with those of the advocates of direct instruction. A decade ago, Resnick (1972) identified the difficulties that many young children experience in coping with the environmental complexities of the informal classroom. Studies such as those by Jackson and Belford (1965) had shown that teachers obtained their satisfaction more from short-term classroom feedback than from the subsequent achievements of their pupils:

'... the outstanding elementary teacher does not often turn to objective measures of school achievement for evidence of her effectiveness and as a source of professional satisfaction. The question of how well she is doing seems to be answered for most of these teachers by the continual flow of information from the students during the teaching session. Spontaneous expressions of interest and enthusiasm are among most highly valued indicators of good teaching, although the quality of the students' contribution to daily sessions is also mentioned frequently' (p 276).

A more recent study by Zahorik (1980) found that the teachers studied did not express much interest in student achievement. As the author noted, although the kind of teaching most directly related to student achievement seems to be 'direct instruction', the approach that teachers themselves recommend is in many ways contrary, showing a preference for indirect instructional techniques.

Contrasts between the outcomes of research in one tradition and that in another must not be exaggerated for effect. Studies that initially appear to fall into one or other category are revealed, on closer inspection, to have many common elements. Broad, high inference categories such as 'clarity' in teaching, long regarded both in commonsense and research terms as associated with subsequent student achievement, turn out when analysed in depth to be composed of several significantly different elements. Nonetheless during the 'seventies the ascendancy of the positivist, empirical, number-crunching model of research was strongly challenged by the growth of phenomenological, interpretive, action-oriented, ethnographic and case study approach many examples of which quickly assumed a place in the research citations and in the consciousness of lecturers and teachers. Erikson and Ellett (1982) suggest that there are four main lines of criticism directed at the plausibility of empirical educational research.

The first criticism is methodological, having to do with the lack of opportunities for direct experimentation, the ambiguities of the correlation/causation relationship, and the inability of research in education to yield firmly established causal generalisations.

A second line of criticism relates to the complexity of educational and social phenomena. There are simply too many uncontrolled — and often unperceived — variables in real life situations. This leaves us with the options of research designs in which the number of variables, in order to be manageable, is so reduced that results are trivialised, or of retaining meaning and significance at the cost of imprecision, looseness and sometimes even conceptual confusion.

A third weakness has to do with the dangers of reductionism, in which behaviour is analysed in relation to sociobiological imperatives, and learning becomes in the last analysis a matter of physics and chemistry.

Finally, the requirement that empirical research should be free of theory and interpretation cannot be met. The very selection of research problems is determined by theories of one kind or another — of society, of human nature, of the possibilities and potentialities of learning. Erikson and Ellett conclude:

'We hope to have altered the image of educational research as providing the firm generalisations on which we should base educational policy. Such an image is both untrue and damaging to educational research. It builds up expectations that no fruitful educational research could satisfy. On the other hand, our interpretations, analyses, inquiries, and theories of education are a rich source of clues and hints that may prove useful to policy formulation. And, in that, educational research may serve educational practice in a far more important and profound way than the attempt to translate research findings into educational policy directives' (p 512).
Agendas for Teacher Educational Research

Having argued that teacher education research embraces much more than the 'application' to the training of teachers of findings on classroom research and studies of teaching effectiveness, it may be helpful in conclusion to look at one or two attempts to define and map the field that are consistent with this approach.

Lilian Katz (1980, 1981) has defined teacher education itself in the following terms:

'Teacher education consists of sets of events and activities which are deliberately intended to help candidates to acquire the skills, dispositions, knowledge, habits, attitudes, values, norms, etc., which enable them to enter the occupation of teaching'.

Research is thus an effort to understand these events, activities and processes in a systematic manner.

Katz goes on to propose a matrix of research themes in teacher education, which contains cells defined by horizontal and vertical matrixes sub-divided into nine 'parameters' — goals, candidates, staff, content, time, ethos, location, regulations and finances. As Katz notes, an almost limitless number of potential variables can be generated in relation to each parameter. The task of assigning priorities to research tasks would be necessarily dependent upon 'persuasive theories' as to possible relationships between variables. Much of this work remains to be done.

In a recent United Kingdom review of research in teacher education, Wragg (1982) has classified the fields of completed and published studies under six headings — general and specific reviews, research into student teachers and their backgrounds, the content of courses and structure of training institutions, teaching practice and school experience, experiments in training and research into in-service and post-experience education. His introduction underlines a difficulty encountered by others who have previously sought to review research in this field (Taylor 1969).

'A computer search of ERIC data basics performing descriptors such as 'Teacher Education' and 'Research' produced well over 500 references. With the inevitable redundancies expected in the computer retrieval of research data, which yields indiscriminately five year, five-volume nationally funded projects, and one-pagers from the Iowa Journal of Bright Ideas whilst shaving, it was possible to reduce this by a half' (p 5).

A comparison between the potential contents of Katz's matrix, and Wragg's list of completed work makes clear just how limited is the volume of research findings in teacher education. In comparison with many fields of scientific endeavour, a few hundred reasonably up-to-date studies, spread across a range of problems as large as those which feature in teacher education, is hardly such as to yield a substantial body of attested knowledge.

The rationale for my own categorisation of completed and on-going work in this field, derives from the structuring of teacher education activity that I used in my Research and Reform in Teacher Education (1978). It is attached as an Appendix to this paper.

Conclusion

It would be foolhardy to argue that the views on research on teacher education offered in this lecture constitute 'British perspectives'. Any such claim would swiftly be contested by those of my countrymen who do not share my opinions about the wide scope of the subject, its lack of co-terminosity with teacher effectiveness and classroom research, the value of retaining an electric methodological base which has room for both positivistic and phenomenological approaches, the need to respect the sensitising, agenda-shaping functions of research and to be cautious about attempts to 'use' research findings as the basis of an instructional technology. It is these opinions that constitute the message of this lecture, and which reflect my own conclusions about the kinds of problems that come within the rubric of research on teacher education. Others in Britain might come to very different conclusions. So might those from Malaysia, the Philippines, Australia and Singapore who are contributing to your continuing consideration of the theme. Like you, I look forward to hearing what they have to say, and engaging in discussion about the future of work in this important field of research.
APPENDIX I

Fields of Teacher Education Research

Teacher Demand and Supply
- Demographic Forecasting
- Phase and subject needs
- Wastage

Recruitment and Selection
- Trends in applications and acceptances
- Qualifications of candidates
- Comparisons with other occupations
- Personality characteristics
- Procedures for screening and selection
- Validation of procedures against performance

Personnel
- Staff qualifications, social and personal characteristics
- Students: social and educational background, attitudes, motivations, development, aspirations, placement
- Supervising staff in schools

Institutions
- History and development
- Structural location
- Finance and administration
- Control and accountability
- Culture, climate and values

Curriculum (Pre-service)
- Structure and organisation
- Pedagogy and assessment
- Epistemology

- Field experience: supervision, theory/practice integration, school involvement

Certification
- Criteria
- Phase/subject specificity
- Portability
- First appointment

Introduction
- Procedures and requirements
- Assessment
- Response
- Monitoring

In-service education and training
- Provision: organisation, cover
- Participation
- Economic considerations
- Outcomes
- Evaluation
- Relations with other stages

Advanced courses
- Type and distribution
- Participation
- Impact upon practitioner and system
- Credentialling

Research
- and/in/on teacher education
- Organisation and finance
- Agenda
- Dissemination and impact
- Trends

Appendix 2

(Medley, 1982 p 1899)
References


