Towards a National School Research Agenda

Geoff N Masters

Australian Council for Educational Research

Almost two decades ago, Benjamin Bloom observed that, in education, we seem particularly susceptible to fashions and fads:

> The libraries and basements of our schools still store the forgotten relics of fads and nostrums which were purchased because they promised to solve our educational problems. In education, we continue to be seduced by the equivalent of snake-oil remedies, fake cancer cures, perpetual-motion contraptions, and old wives' tales. Myth and reality are not clearly differentiated, and we frequently prefer the former to the latter. (Bloom, 1981, 15)

Twenty years on, it is interesting to reflect on whether we have become less susceptible to fashions and fads in education. Is decision making now better informed by systematic research? Do our schools and education systems still lurch from one fad to the next, depending on the prevailing political agenda or current educational philosophy? To what extent are recent initiatives in Australian school education based on solid research evidence?

By way of example, we might consider the areas of language and literacy learning. There has been no shortage of new initiatives in these areas in recent years: early literacy programs, new approaches to teaching reading, national literacy goals, whole language programs, national surveys, phonics teaching, statewide literacy tests, literacy benchmarks, international surveys, literacy targets, the list goes on. The good intention of all these initiatives is the improvement of literacy learning. But what works? What is the research evidence that setting benchmarks and targets for literacy attainment will improve literacy levels in schools? By what mechanism do statewide tests improve children's literacy learning, and how can their effectiveness be enhanced? Among the multitude of early literacy programs, are all equally effective? What are the characteristics of the most effective programs? How are schools and teachers to make informed choices of one literacy program over another?

My own perception is that much practical decision making in education continues to be driven by political agenda, by models adopted from other countries and industries, and by philosophical beliefs about what *should* work. While education systems and schools are focused on the implementation of each new initiative, and teachers are busy judging whether or not to respond, we only occasionally pause to question the research evidence that a new initiative will lead to improved student learning.

It would be easy for those of us in the education research community to be cynical about the inadequate research base for much educational decision making, and to be critical of the apparent inability or unwillingness of many decision makers to distinguish fads and beliefs from solid research evidence. We could point to the limited attention paid by practitioners to research findings, to the limited confidence many decision makers have in educational research, and to the limited support the education industry provides to research in general.

We might argue that, in an ideal world, all education policy makers, managers and classroom practitioners would be hungry for reliable information about what works to improve learning; quick to eschew untested fads; keen to work with researchers to identify and address pressing research questions; and eager to collaborate to develop a national research agenda for education.

But in my view, the limited role research findings appear to play in setting new directions for educational policy and practice is attributable in large measure to the way in which we conduct and communicate educational research. If educational research is to play a greater role in informing policy and practice in the 21st Century than it has in the 20thCentury, then there are some hard questions that we should now be asking ourselves:

a clear sense of purpose

What is the purpose of educational research? Are we happy with the idea that educational research has a purpose? Or is our field of work more complex than that? If we do have a purpose as researchers, what is it?

focused research questions

Are we clear about the questions requiring answers in school education? Can we write them down? Can you write down the research question or questions you are attempting to answer in your research? Can you write down some possible answers to this question? Can you write down the implications for educational policy or practice of each of these possible answers?

Do we believe that we can find clear answers to educational research questions? Have we given up on the possibility of providing clear, unambiguous advice to policy and practice? Have we used the complexity of the phenomena with which we are working as an excuse for not providing clear advice?

useful research syntheses

What do we know about what works in education? Are educational phenomena so complex and so context-specific that it is not possible to develop useful syntheses of what we know? Are research findings so equivocal and contradictory that we cannot extract meaningful generalisations? To what extent does our research build on to previous research to provide a growing corpus of knowledge? Or are our research projects and methods so varied and idiosyncratic that the cumulation of knowledge is difficult or impossible?

improved dissemination

Are we so concerned to publish our research in refereed journals that we have given insufficient attention to its practical utility or to ways of maximising its take-up and impact? Are we content with the conclusion that the phenomena with which we are dealing are complex and that there may be no clear implications for practice? Does our concern for academic rigour lead us to obscure significant findings with cautions, caveats, and journalese? Do we believe that the dissemination and popularisation of research findings is a job for somebody else?

a national research agenda

What are the most pressing research questions in Australian school education at the present time? Can agreement be reached on a small set of questions deserving an intense and concerted effort? What are the research questions that Australian school systems want answered? Are those systems working with the education research community to ensure that these questions are addressed? Which questions, if we had answers to them, would have the greatest potential for improving learning in our schools?

Over the past twelve months, ACER has engaged in two separate but related activities that have been important to our thinking about many of these questions.

The first activity was a round of consultations with all State and Territory school education authorities and the Commonwealth to identify their research priorities for the next three years. The second was a survey of 85 leaders of the school education community by Dr Jane Figgis as part of ACER's review of its research dissemination strategies. (The interviewees included Directors-General, senior system bureaucrats, representatives of Independent and Catholic education authorities, principals' associations, other professional associations, school-based leaders, heads of school-related agencies such as Boards of Studies, education unions and curriculum agencies, and university researchers.)

The input from these two activities, while specifically addressing ACER's research program, no doubt has relevance for Australian educational research more generally. In their discussions with Jane Figgis, school education leaders encouraged ACER to:

- communicate a clear sense of its mission/purpose;
- provide a strong, independent, authoritative voice in school education;
- share research knowledge and create partnerships; and
- develop research-based tools that can be used in schools.

These challenges could usefully be taken up by all of us who work in educational research in this country. If educational research is to be more effective in the 21st Century, then we must clarify and communicate our shared mission. We must develop a strong and respected voice as a research community on issues of educational policy and practice. We must find ways of concentrating effort in research teams and partnerships and of sharing and disseminating our expanding research knowledge. And we must work harder at developing practical tools and services made possible by our research.

A Clear Sense of Purpose

> The lack of consensus on education research goals is the result of a weak field that cannot make tough decisions to do some things and not others, so it does a little of everything. (Kaestle, 1993, 29)

Since taking up my appointment as Director of ACER late last year, I have had cause to reflect again on the nature and purpose of research in education. What is it that distinguishes *educational* research from other forms of social, organisational and behavioural research? What is the fundamental question that educational research seeks to answer?

I have a rather simple view of educational research. It follows from my rather simple view of education itself. I see education as being fundamentally about the development of individuals. We use a variety of words to describe this core business of teaching—student growth, progress, improvement and learning, to name a few. But whichever term we use, our central purpose as educators is to facilitate and support the development of individuals' understandings, attitudes, values, knowledge and skills.

The purpose of educational research is to find ways to improve student learning. In my view, educational research has no other purpose, and research that does not have this as its ultimate motivation and objective is not educational research. (It may well be legitimate and useful research, but I would not describe it as *educational* research.) Strangely, in my twenty-five years as a researcher, I cannot recall anybody ever giving me such a simple statement of the purpose of educational research. Perhaps my old teacher at the University of Chicago, Benjamin Bloom, came closest. I suspect that some of my teachers never told me because they thought it was obvious, and many never told me because they did not know.

It follows from my simple definition that anybody engaged in a systematic search for ways to improve student learning is engaged in educational research. A classroom teacher experimenting with alternative ways of introducing a text, or of motivating a particular individual, and a full-time educational researcher comparing the effectiveness of different early literacy programs in addressing children's reading difficulties are both engaged in a search for ways to improve student learning, and, in this sense, are both engaged in educational research.

It also follows from my simple definition that educational research is undertaken for the ultimate benefit of learners. The ultimate beneficiaries of all educational research are students, regardless of who initiates, undertakes or pays for the research, and regardless of the nature of the research or of the questions the research is attempting to answer.

It is interesting in thinking about the beneficiaries of educational research to reflect on parallels with other forms of research. The purpose of medical research is to create and disseminate knowledge and tools that can be used to improve human health. Although medical practitioners and medical institutions benefit from the availability of new knowledge and new techniques, medical research is conducted in the first instance for the benefit of patients, or in the interests of public health generally. The criterion in terms of which medical research must be judged is its success in creating knowledge and tools that can be used to improve the quantity and quality of human life.

The purpose of educational research is to create and disseminate knowledge and tools that can be used to improve learning. Improvements to learning can take many forms, including quantitative improvements in achievement (eg, increased literacy levels among disadvantaged students); qualitative changes in the kinds of learning taking place (eg, greater emphasis on higher-order thinking skills, attitudes and values); and the development of more efficient or cost effective ways of achieving learning outcomes.

The improvement of learning is the objective that should drive all educational research. Research into teacher practices, curriculum materials, ways of organising and managing educational institutions, teacher professional development, assessment and reporting practices, successful transition from school to work—indeed any educational research topic one wishes to name—has as its ultimate purpose the improvement of learning. If it did not, it is difficult to see how it could claim to be *educational* research. All educational research ultimately must be judged in terms of its success in creating knowledge or tools that can be used to improve learning.

In his 1997 article in *Educational Researcher*, Michael Scriven argues that educational research has a 'duty' to society to create knowledge that can be put to practical use. He, too, draws a parallel with medical research:

> If medical research had only contributed explanations of disease but had neither identified nor developed any successful treatments, we would surely say that it had failed in its principal duty. (Scriven, 1997, 20)

If Scriven's argument is correct, then educational research not only has a clear purpose, it also should be driven by a sense of duty to go beyond simply describing and understanding educational practices to have an impact on improving practice. If this 'duty' is taken seriously, then some forms of research will be given higher priority than others. For example, research into the most effective ways of using information technology to enhance learning probably will be given higher priority than a study designed only to establish and document the prevalence and uses of computers in classrooms.

Perhaps the reason why decision making in education is not as research-based as it might be is that we have come to view the findings of much research as equivocal and contradictory. We have come to doubt the generalisability of research findings and have seen much research as distant from the day-to-day workings of classrooms.

Again, it is interesting to reflect on how research is seen in other areas of human endeavour, such as agriculture, and on the relationships between research and practice that have developed in those areas. Research in support of Australian rice farming, for example, has a clear purpose: to create knowledge and techniques that can be used to make qualitative and quantitative improvements in rice production (eg, to increase crop yield, to improve farming efficiency in terms of water usage, to create new strains of rice for niche markets).

In this research, the role of the researcher is clear. The practitioner (farmer) looks to, and depends on, research for innovations to improve farm output. The researcher and farmer work together to achieve this result, for example during farm visits by the researcher to analyse local farm conditions. And the criterion for judging the success or failure of the researcher's efforts is unambiguous.

It is instructive to consider what a relationship of this kind might look like in education: a relationship in which the researcher is clearly focused on creating knowledge to improve practice; in which the practitioner looks to and expects research to be of obvious practical benefit; and in which the researcher visits and works with the practitioner to design strategies to improve local output.

As we commence a new century of research in education, it will be important, I believe, to commit ourselves to a more focused conception of the purpose of educational research: a conception in which improved student learning is our shared ultimate goal. This sharper focus should lead us to commit energy to some things and not others, rather than doing a bit of everything.

A clearer understanding of the purpose of educational research may lead us in the 21st Century to a clearer understanding of what we are and are not prepared to call educational research. To continue the parallel with other forms of research, writing a history of the Royal Melbourne Hospital would not usually be described as medical research. Are we similarly clear about what does and does not constitute *educational* research? (In asking this question, I am not questioning the ability of historical method to contribute to both medical and educational research. I am simply observing that medical research appears to have a relatively clear mission or purpose, and speculating that educational research would benefit from a similarly clear understanding of its ultimate purpose.)

Focused Research Questions

> Education research does not provide critical, trustworthy, policy-relevant information about problems of compelling interest to the education public.
>
> (Sroufe, 1997, 27)

Perhaps a key to improving the usefulness of much educational research would be a sharper focus on the *research questions* being addressed. Many research studies in education proceed without an explicit specification of the questions they are attempting to answer. Instead, they are conducted as data gathering exercises in some area of educational activity, presumably in the hope that the data, when carefully analysed, will yield useful insights. And, if sufficient data are collected, they frequently do. But too often the reported 'findings' of this form of research are the one or two most interesting bits of information caught in the research net.

A sharper focus on research questions requires an intention to do more than map and document the *status quo*. It requires more than the investigation of broad research 'topics'. It involves an active search for better answers to the question 'What can be done to improve learning?'

I believe that much educational research would benefit from the more explicit specification of the one or two key research questions that each of our research projects is designed to answer. In this way, there would be a better chance of developing and documenting an accumulating body of knowledge that builds on to the findings of earlier studies. And these research questions should be focused on problems of compelling interest because of their potential to significantly improve educational policy or practice.

The specification of clear research questions will be a challenge for researchers accustomed to describing their research only in terms of the areas in which they are working, or in terms of broad research topics. And to be useful, research questions will need to be specified at a finer level of detail than 'What are the factors influencing school to work transition?' or 'What are the best ways to use information and communication technology in the classroom?'.

Useful Research Syntheses

> Meta-analyses show that many generalisations in education do hold up across many replications with high consistency. (Gage, 1997, 18)

> The school education community wants boldness—strong statements about what is known and what is not, what is right and what is wrong, how current practice matches research conclusions. (Figgis, 1999)

To be helpful to practice, the conclusions of past research must be readily accessible by practitioners. What is known about the best ways to improve the health of patients with a particular medical condition? What is known about the best ways to improve the yield and quality of a particular crop under various soil conditions? What is known about the best ways to develop the early numeracy skills of Indigenous children in remote communities? What does research say about the impact of different class organisations and grouping arrangements on student learning? What works in developing students' understandings of the relationship between force and motion?

Just as medical and agricultural practitioners use syntheses of past research to assist in day-to-day decision making, educational practitioners could benefit from much more convenient access to available research findings. Given the many thousands of person-years spent on research in education, it seems surprising that the findings of this research do not play a greater role in the day-to-day decision making of educational practitioners.

In education, there is a pressing need for accessible, up-to-date reviews of our current state of knowledge. Although the findings of educational research often are equivocal and contradictory, it is also true that, in most areas of educational practice, there is an emerging body of consistent evidence about what systems, schools and teachers can do to improve student learning—in short, *what works*. Systematic reviews of the research evidence in language and literacy learning are identifying factors in successful practice and are providing principles for evaluating commercial literacy programs. The recent report *Preventing Reading Difficulties in Young Children* provides one such synthesis.

A challenge in the 21st Century will be to draw together what is known about successful policies and practices in various areas of educational practice and to present those research syntheses in forms that are easily accessible to teachers, schools, parents, system managers and policy makers. In our work at ACER we plan over the next three years to develop a series of 'review papers' on such topics as 'What is the research evidence on improving the achievement of boys? What do we know about effective uses of information technology in improving student learning? What works in early literacy education? What does the research evidence say about the influence of school structures on student learning? What forms of feedback from assessment programs have been shown to be effective in improving student achievement?'

In educational research generally there is a need for better summaries and syntheses of research findings in easily-accessible formats. These syntheses should be designed to communicate to the Australian education community what is already known from research about successful educational policies and practices, as well as providing a base for planning future research. At least one government education system has made an attempt to produce reviews of this kind for schools and has received favourable feedback on their value.

There also will be a need as we look ahead to the 21st Century for research-based papers that are more speculative in nature—'think pieces' that draw on available research to anticipate and suggest future directions for education policy and practice.

Improved Dissemination

> The image persists that education researchers don't know how to disseminate their results to practitioners. (Kaestle, 1993, 28)

Another key to ensuring that research serves educational practice is the development of better methods for making research findings available to practitioners. As in other fields of research, it is essential that advances in educational knowledge are published in relevant research journals. But the most effective way of making research findings available to teachers may be through new procedures, tools or techniques, accompanied by training in their implementation.

A recurring theme in ACER's consultations with State and Territory school authorities earlier this year was the need for better dissemination of educational research. There was a general view that, although we now have a body of solid, well-documented research, there is

a need for shorter, more accessible summaries of research findings to complement traditional research reports. Better dissemination also might be achieved through the use of videotapes, audiotapes, seminars, workshops, and by working directly with practitioners in the implementation of research findings (although working with practitioners often will be for the purposes of providing examples of how research findings can be articulated with the world of practice).

In our consultations, a number of education systems raised questions about the possibility of closer collaboration between researchers, Departments of Education, and school personnel in undertaking research. Among the suggestions for improved collaboration were:

— the possibility of establishing collaborative research projects between teams of researchers and staff in State/Territory departments;

— the possibility of staff from school education authorities spending short periods working as part of a research team to develop skills in research methods, data analysis, and so on;

— the possibility of researchers spending some time working within a government education department;

- the possibility of researchers assisting education systems to make better use of and to interpret data they already collect. Frequent reference was made to the student outcome data that many systems now collect, and to the need for 'meta-analyses' which would use data from different systems to address research questions of Australia-wide significance (eg, in early literacy learning).

From her interviews with leaders in the Australian school education community, Jane Figgis extracted five suggestions for improving the dissemination of educational research findings and for improving the usefulness of research to educational decision making:

1 *Ensure that all communications about research are timely*

> People want to know about research while it is underway. It is important to keep the school education community informed, in an engaging way, about research work from the start (what is being done and why) and as interesting findings emerge, and then to disseminate key results immediately the research is complete. (Figgis, 1999)

2 *Extract and communicate the intellectually interesting 'plums' from research findings*

> What people want are relatively short, effective explanations of key findings and why they are key. This is not a request for summaries. It is a request for the 'good bits': the bits that are salient and intellectually interesting - what ought to be memorable - for that particular audience. (Figgis, 1999)

> 3 *Explain the usefulness of particular research findings*

> Many—perhaps most—researchers find this totally wrong-headed. Our job, they say, is to do the research, and it is the job of professional educators to draw out the implications for themselves. (Figgis, 1999)

4 *Generate two-way communication about research in progress*

5 *Establish research partnerships*

> What they want is somehow to feel part of the whole process. In this, they are in tune with (or ahead of) the growing body of work on the utilisation of educational research which says that the tradition of thinking of educational research as a linear step-wise process—from consultation to research to dissemination to utilisation—is a recipe for irrelevance and *non*-utilisation. What is required is a constant interaction among all the actors and activities.
>
> (Figgis, 1999)

From these two sets of input over the past twelve months, there are clear messages to the education research community about ways in which our work could be shared and disseminated to make it more directly useful to system managers, school managers, policy makers, teachers, students and parents. A strong thread through this feedback is the call for research that is more intimately connected to educational decision making. In the 21st Century, our view of research must continue to change from one in which researchers conduct research largely in isolation from potential users of research, and then make findings available in published reports at the completion of the process. As a research community we must seek to involve educational decision makers and practitioners more directly in the identification of questions requiring answers; we should explore opportunities to work in partnerships with research users; keep a range of interested parties informed about the progress of research while it is in train; and expand our definition of research dissemination to include workshops, popular publications, CD-roms, internet sites, and meetings with users.

A National Research Agenda

> *Develop a short list of priorities in research that are not tied to those of a particular federal agency. Stick with this agenda for a decade. Focus on better ways of disseminating research findings; be sure that such findings are clear and useful to the different consumers of educational research. (Cuban, 1997, 16)*

Each of the general strategies outlined in this paper has the potential to be helpful in enhancing the contribution of research to educational practice:

- recognising that the purpose—indeed duty—of educational research is to create and disseminate knowledge and tools to improve learning;

- attempting to specify clear research questions that build an accumulating body of research knowledge;
- developing syntheses of what is known from research and making these available in highly accessible forms;
- building partnerships to plan, undertake, share and disseminate research findings.

But these general strategies need to be accompanied by a list of national research priorities. What are the questions to which we urgently require answers in education over the next five years? The next ten years? ACER posed these questions in its round of consultations with State and Territory Departments and the Commonwealth in relation to our research program over the next three years.

A Research Focus

The first outcome of those consultations was agreement that ACER's research for the triennium 1999-2002 should be focused around a single, over-arching research question: *What can be done to improve learning outcomes*? Our intention is that all core research projects during this period will be designed to address this general question. An implication of adopting this focus is that priority is being given to research projects focused on identifying:

- ways of improving learning outcomes (eg, achievement of boys; outcomes for Indigenous students; early literacy development);

- the kinds of learning outcomes required for success in future education, training, work and life;

- ways of improving levels of access to, and participation in, education and training;

- ways of supporting teachers and developing professional competence in promoting learning; and

- the most effective and efficient ways of improving learning outcomes, including the most effective ways of using assessment, reporting and information technology to support learning and teaching.

In adopting this overarching focus on improving learning, it is recognised that learning outcomes are not limited to knowledge, skills and understandings in the key learning areas, but include the full range of outcomes valued by educational institutions, including the social objectives of schooling, student attitudes and values, cross-curricular competencies, higher-order thinking skills, and improved vocational outcomes and lifelong learning skills.

It is also recognised that a research focus on *what works* to improve learning needs to address the questions *for whom?*, and *under what conditions?* and requires an emphasis on research in classrooms, addressing, among other things, questions of effective pedagogy.

A Set of Priorities

Having established this overall focus for our core work in 1999-2002, we next sought advice on areas of research priority. During this second round of consultations, we had little difficulty in homing in on five broad areas or research priority:

- teaching practices to improve learning
- improving literacy and numeracy learning
- vocational outcomes and lifelong learning
- improving outcomes for Indigenous students
- assessment and reporting to improve learning

The general question to be addressed in each of these five priority areas is: *What can be done in this area of educational practice to improve learning outcomes?* An important first question is: What is already known about the best ways to improve learning through policy and practice in this area? The second task is to identify from an analysis of the current state of knowledge in each area, the research questions most likely to lead to improvements in learning if answers to those questions were available. The final step is to decide which questions are answerable in practice and to design a research program to seek answers to those questions.

Looking Ahead

The five broad research priorities listed above come close to mapping a national agenda for school education research in this country for the start of the 21st Century. The Commonwealth Department of Education, Training and Youth Affairs has a similar and overlapping list of national research priorities. But these proposed priorities have been the subject of consultation and negotiation with only a limited number of stakeholders in school education.

In the case of ACER's research program, priorities were identified through a process in which researchers themselves went to policy makers and system managers and encouraged them to talk about their research needs. An alternative would have been for those responsible for school education in this country to take the initiative in communicating their research needs and in collaborating with the educational research community to establish a national research agenda.

As we make the transition to the 21st Century, the time has come, I believe, for greater clarity about the purpose of educational research; for higher expectations of the significance and usefulness of research findings; and for much greater collaboration between policy makers, practitioners and the research community to identify urgent research priorities and to establish significant, ongoing, research teams capable of building an accumulating body of knowledge. If we can achieve these objectives, then perhaps education in the 21st Century will be less at the mercy of fads and fashions and better able to make improvements in student learning.

References

Bloom, BS (1981). *All Our Children Learning*. New York: McGraw-Hill.

Cuban, L (1997). The Vision Thing: Educational Research and AERA in the 21st Century. Part 2: Competing Visions of What Educational Researchers Should Do. *Educational Researcher*, 1997, 12-18.

European Commission (1999). Forward Planning in Education in the Member States of the European Union. Brussels: Eurydice.

Figgis, J (1999). *Relevance, Partnerships, Boldness: Expectations of ACER in the 21st Century*. Melbourne: Australian Council for Educational Research.

Gage, NL (1997). The Vision Thing: Educational Research and AERA in the 21st Century. Part 1: Competing Visions of What Educational Researchers Should Do. *Educational Researcher*, 26(4), 18-19.

Hargraves, A (1996). Transforming knowledge: blurring the boundaries between research, policy and practice.*Educational Evaluation and Policy Analysis*. 18(2), 105-122.

Hillage, J, Pearson, R, Anderson, A & Tamkin, P (1999). *Excellence in Research on Schools. Research Report RR74*. London: Department for Education and Employment.

Kaestle, CF (1993). The Awful Reputation of Educational Research. *Educational Researcher*, 22, 23-31.

Scriven, M (1997). The Vision Thing: Educational Research and AERA in the 21st Century. Part 1: Competing Visions of What Educational Researchers Should Do. *Educational Researcher*, 26(4), 19-21.

Sroufe, GE (1997). Improving the 'Awful Reputation' of Educational Research. *Educational Researcher*, 26(7), 26-28.

US National Research Council (1999). *Improving Student Learning: A Strategic Plan for Education Research and its Utilization*. Washington.