Editorial

Of What Purpose?

Recently I had a conversation with an Alberta provincial politician, as well as with several employees of Alberta Learning (the provincial ministry of education). Upon discovering that I am Editor of **ajer**, they asked, "Why do people waste their time doing so-called research, since much of it seems to have little practical value, especially dealing with current problems in education?" "I'll bet that most research activity goes on because it is funded by one agency or another," was added for good measure. My initial impulse, which I stifled, was to respond that if worth is measured in terms of practicality, then much of what government does is worthless. Instead I responded that research in and of itself helps ensure that people in education examine the field, and often research leads to greater insights into what we do and occasionally leads to improved practice. My response was met with skepticism; and then I pulled out a big stick. I said, "If only so-called *good* research is worthwhile, then much of what Thomas Edison did was a waste of time and money. Consider his work with partial-vacuum glass envelopes following his development of the incandescent lamp. He discovered that a heated cathode appeared to emit a charge that could be collected at a cold anode (Handel, 1975). In spite of his discovery, Edison was unable to find any practical use for it. So was his research a waste of time?" The response was, "Not completely, because Edison invented many things, so who cared if he came up with some dead ends?" I smiled as I said, "Well, the Edison effect was studied by John Fleming, who, building upon Edison's research, invented the diode tube, originally called the Fleming valve, a fundamental component of electronics before the transistor" (Fisher & Fisher, 1996). The final rejoinder was,

> OK, so you know something about scientific research, but educational research seems to go in an all-or-nothing cycle. For a time a particular approach or theory is "in," and then it's not. Usually it is someone or something outside education that gets the cycle going. People in education get on a bandwagon, and then when the novelty wears off, attention is shifted to something else.

Following that meeting I reflected on what was said about educational research. Is it really the case that the impetus for much educational research comes from outside the field? What about the apparent *bandwagon* nature of research? Are educators engaging in "basic" research (research in an aspect of education that is of interest perhaps only to the researcher), or are we engaging in research that is either trendy or that which is funded? There are no simple answers to such questions, but they are worth considering, because much of education receives public funding, and it is the impressions of the public, through politicians, that often determine the level of funding.

Unlike some other fields, education is often considered by the public, as almost everyone has gone to school, and through such experience there is a tendency to believe that experience equals knowledge. Because grade-school education occupies much of childhood and adolescence, it seems logical to

some people to conclude that many problems or societal shortcomings can be attributed to faults or deficits in education. The question as to whether such a conclusion is based on objective and solid research has rarely been asked by educational researchers or has been poorly addressed by them. For example, *Why Johnny Can't Read* appeared in 1955 (Flesch). It contained much anecdotal information, agitated many people—including citizens of countries outside the United States—and stressed the idea that existing methods of instruction, especially concerning reading, were inferior. Added to this was the launch of *Sputnik* by the Soviet Union in 1957. The questions and panic engendered in the public led many American politicians to conclude that there was no time for further research into US education. Instead the prevalent view was that the state of US (and by extension Western) education was in crisis and immediate action was needed. One result was the US National Defense Education Act of 1958. Among other things, the Act was intended to rectify the alleged deficiency of inadequate mathematics and science education in schools that had enabled the Soviet Union to leapfrog ahead of the US, at least in space exploration (Columbia University Press, 2003). Was such curriculum reform really necessary, and was the solution really addressing the basic problem? Before such questions could be asked, much less researched, other individuals outside education waded into the issue. One notable personality was Admiral Hyman G. Rickover of the US Navy. Rickover (1959, 1963), a competent seaman, also believed his expertise extended into the realm of educational research. His books condemned American education as it existed. He concluded that the reason the Soviet Union was successful was because most North American education was inadequate. Although rattling good yarns even today, Rickover's books are extremely thin on solid research, educational or otherwise.

Nevertheless, the predominant idea in the US, Canada, and to a lesser extent in other Western countries, was that traditional educational curricula and practices had failed and that research should be directed toward the development of new curricula and methods. Programmed instruction, teaching machines, computer-assisted instruction, and the space-age curriculum were what up-to-date educational researchers should investigate (Lumsdaine & Glaser, 1960). Other research pursuits, whatever they might be, were expected either to be abandoned or to take a back seat to *important* research. Of course, the corollary was that research findings should support these new initiatives and technologies. Strangely, perhaps, not all research findings did support the often outrageous claims made about learning efficiency and the effectiveness of these approaches and methods (Gilbert, 1979; Glaser, 1965; Moore, 1980). Russell (1999) talks about the *no significant difference* phenomenon. This is where particular educational practices initially touted by some as being superior to preexisting practice are found through research to have minimal positive effects on education. Such findings should not be surprising to educators. It should be surprising, however, that the research occurred *after* implementation rather than before. Indeed Hunka (1977) stated that through a lack of basic research, which includes understanding the day-to-day classroom life of teachers, it was thought by many individuals in relation to computer-assisted in-

struction, "that one could simply plug the computer into a power source, and presto, instant education! The computer was expected to accommodate the gifted, the slow, the ghetto child, as well as the average classroom child" (p. 2).

Although what I describe occurred many years ago, can it be that as a group educators rarely modify future behavior based on previous practice and experience? It seems that many educators are again caught up in the problem of agendas and/or funding driving educational research. Although programmed instruction and the early iterations of computer-assisted instruction are gone, there are recent crazes concerning the instructional use of the Internet and of *killer software* such as Microsoft's PowerPoint. Like the use of computers as described by Hunka (1977), particular applications of computer technology were championed by some individuals as being an educational panacea (e.g., Papert, 1993). Of course, when the panacea does not correct the faults, where better to lay blame but at the feet of educators and "the educational system"? Such shifting of blame would undoubtedly be much more difficult if before embracing such approaches educators researched these ideas and practices before widespread (and often expensive) implementation.

One of the factors that contributes to the apparent bandwagon effect in educational research is funding. Often the effect of funding is twofold. First, projects and proposals congruent with current trends are often those that are funded. Second, institutional recognition in the form of promotion and salary increments is frequently focused on funded research. The contribution of the educational researcher who examines some aspect of curricular theory, for example, and who does not desire or need a large research grant is all too often downplayed in favor of the researcher who secures a huge grant to study something that is either a priority of some agency or is popular at the time. Indeed a colleague of mine recently remarked that the fuss made over securing research grants at universities has become a phallic-like preoccupation in some instances, where the person with the biggest grant is considered the best researcher. Although perhaps expressing an extreme view, my colleague does raise an interesting point, which also relates to the questions raised at the outset of this editorial. If the present focus is on obtaining research grants and/or researching areas that are deemed important, then what is the role of basic research in education? Should the purpose of educational research become a quest for funding, or for examining those matters that the agendas of one or another agency deem important?

G.H. Buck

References

Columbia University Press. (2003). *National defense education act*. Retrieved May 13, 2003, from FactMonster: http://www.factmonster.com/ce6/society/A0834940.html

Fisher, D.E., & Fisher, M.J. (1996). *Tube: The invention of television*. San Diego, CA: Harcourt Brace.

Flesch, R.F. (1955). *Why Johnny can't read—And what you can do about it*. New York: Harper.

Gilbert, T.F. (1979). Human incompetence: The autobiography of an educational revolutionist. *National Society for Performance and Instruction Journal, 18*(6) 15-21.

Glaser, R. (1965). *Teaching machines and programmed learning, II: Data and directions*. Washington, DC: National Education Association.

Handel, S. (1975). *A dictionary of electronics*. Harmondsworth, UK: Penguin.

Hunka, S.M. (1977). *Eight years of computer-assisted instruction: Now what? RIR-77-6*. Edmonton, AB: University of Alberta, Division of Educational Research Services.

Lumsdaine, A.A., & Glaser, R. (1960). *Teaching machines and programmed learning: A source book.* Washington, DC: National Education Association.

Moore, O.K. (1980). About talking typewriters, folk models, and discontinuities: A progress report on twenty years of research, development and application. *Educational Technology, 20*(2) 15-27.

Papert, S. (1993). *The children's machine: Rethinking school in the age of the computer.* New York: Basic Books.

Rickover, H.G. (1959). *Education and freedom.* New York: Dutton.

Rickover, H.G. (1963). *American education, a national failure: The problem of our schools and what we can learn from England.* New York: Dutton.

Russell, T.L. (1999). *The no significant difference phenomenon.* Montgomery, AL: International Distance Education Certification Center.