Editorial

Improving Education Research: Ideology or Science?

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In the United States these days, there is a surprising amount of attention being paid to the improvement of research in education. Calls are heard across the land for greater rigor in educational research so that *scientific evidence* and *research-based practices* can guide educational improvement. (For more on this, see my editorial in the March 2002 issue [Silver, 2002].) My colleagues in other countries tell me that this rhetoric is also beginning to seep across the borders and oceans surrounding the United States and is finding its way into political and professional discourse regarding education around the world.

There is little disagreement within the profession that the quality of education research needs to be enhanced. Ellen Lagemann (2000) has detailed the torturous history of education research. In her analysis, Lagemann notes that, since its inception about 100 years ago, education research has rested on a foundation of work in the social sciences, such as psychology and anthropology. Yet the validity of the social sciences themselves as *scientific* enterprises has been debated numerous times in the past century. Efforts to cast education research as a scientific field have been challenged almost continually, both from within and from outside the field. As a consequence, education research does not hold high status in the academic community, nor is it viewed as persuasive in political debates. A recent effort to examine the status and quality of education research was undertaken by a committee of the (U.S.) National Research Council [(NRC) 2002], and its report outlines some critical features of scientifically credible education research and several design principles to guide the creation of an improved education research enterprise. But efforts to improve education research from inside the field are running headlong into initiatives launched by politicians and others outside the field.

Seeking a simple way to improve education research, some politicians in the United States have turned to medicine. Ignoring, or perhaps dismissing, the penetrating analysis of the complexities of education research offered by the NRC report, they have turned to the compelling model offered by the National Institutes of Health (NIH). As a consequence, one now hears frequent calls for education research that meets the so-called gold standard of research supported by the NIH; namely, double-blind, experimental designs with random assignment to trials. According to the current prevailing view among policymakers, educational progress will not be made unless and until we have such research in hand to guide decisions.

There are many reasons one might offer for being skeptical about such a simplistic solution to the problem of education research. For example, Howard

Gardner (2002) has noted that many excellent educational programs around the world, ranging from the Reggio Emilia preschools in Italy to colleges and universities in the United States, are not themselves based in any rigorous way on the kind of scientific evidence in current demand. Moreover, David Berliner (2002) has observed that there are many views of science and scientific inquiry, and by at least some of these views, educational research may be the "hardest science of all" (p. 18) because it must deal with complex contextual elements and interactive human concerns. "A science that must always be sure the myriad particulars are understood," Berliner argues, "is harder to build than a science that can focus on the regularities of nature across context" (2002, p. 19). Ironically, many of the politicians who now demand experimental evidence based on randomized trials that control for variations across contexts are the same ones who argue for the importance of local (rather than federal) control of educational decision making because of the need to attend to the nuances of local needs and preferences. Berliner's point about the diversity of sciences is important, especially when one considers the growth of scientific knowledge in fields such as astronomy or geology, in which experimentation of the sort done in medical studies is hardly possible. Knowledge grows in these fields through a combination of theory development and empirical observation. Even if one does not agree with Berliner's assertion that educational research is more difficult than medical research, one can find other reasons to doubt the wisdom of a wholesale adoption of the medical model.

Medical research, even when done well, is often not conclusive, either because of conflicting findings or because of the impossibility of conducting definitive experimental studies. Consider, for example, the medical evidence currently available to middle-aged women regarding the efficacy of hormone replacement therapy or the necessity of annual mammograms. Moreover, even when evidence is available it is not always used by medical professionals in clinical practice, as is evident from a recent study of care provided to Medicare recipients. That study (Jencks, Huff, & Cuerdon, 2003) found on average that patients had less than a 75% chance of receiving the appropriate, proven treatment their condition required. Thus, it seems that medical research also suffers a disconnection from the world of practice, which has, of course, also been a longstanding critique of education research. The current pressure on educational practitioners to produce scientific evidence to justify their work will no doubt cause them to have greater interest in education research. Although it is unlikely they will consistently find what they need in what we now have to offer, it is also not clear that adopting the medical model will necessarily remedy the situation immediately or even in the future.

The current attraction of policymakers to scientific evidence also seems to be selectively applied. It is difficult not to be cynical when the policymakers who call for greater scientific integrity in education research then turn a deaf ear to biological scientists as they make a compelling case for federal support for stem cell research. The same is true when they demand that education research must be made more scientific, while at the same time purging a national repository of research reports and information thereby preventing current and future researchers from

exhibiting one of the hallmarks of good scientific inquiry—building a cumulative base of knowledge. Government officials require that an educational innovation should not be used on a large scale unless scientific evidence shows conclusively that it works; yet, politicians spend billions of dollars to support a missile defense system, despite a conclusive body of evidence showing that the system does not work (though we are promised that it will work eventually!). What unites all these examples of seeming incongruity is a rhetorical preference for science but an action-oriented preference for ideology among the group of politicians and policy professionals who currently control the decisions. As a consequence, many in the field of education research now worry that ideology and orthodoxy rather than scientific quality and integrity will guide decisions about who receives federal funding for research and development. Such fears are not groundless; some projects with questionable leadership and flimsy premises, but which are deemed to be ideologically appropriate, have been funded without rigorous peer review.

As tempting as it is to greet with cynicism the current policy initiatives regarding education research, we would be wise to consider other responses as well. We should look at the research designs and methods that we employ and recall that issues of deep concern to the field of mathematics education can be studied using quantitative as well as qualitative approaches. Has our love affair with qualitative methods over the past few decades caused us to ignore the value of quantitative research methods employed within reasonable designs and applied to appropriate data sets? We should seize the current interest in scientific quality in our work and turn it into an opportunity to examine the quality of our research methods and the care with which we make and warrant claims in the conduct of both quantitative and qualitative research. We should recall that both qualitative and quantitative research methods can and should be applied rigorously. We should examine the research training provided in our doctoral programs and infuse a new emphasis on the conduct of high-quality research. We must ensure that the next generation of professionals in our field is better prepared to meet the challenges of high quality education research. We should also press our politicians and leaders of our professional organizations to obtain greater funding for rigorous education research using multiple methods.

If we are asked to deliver education research on a par with medical research, then we should demand comparable levels of funding support. In an interesting history of medical education in the United States, Ludmerer (1985) notes that medical research was not very highly regarded in the late 19th and early 20th centuries, and it was not very well supported. In 1891, total endowments to medical schools in the United States amounted to $500,000 in contrast to $18 million in endowments to theological schools. But as we know, support from government and private sources for medical education and medical research grew substantially during the last century, and this is now viewed as a premier professional field of scientific endeavor. Might the same happen to education research if it received comparable levels of support from government and private sources in the next century? Meager funding for education research is not a new problem, but it is often

omitted when critics point to deficiencies and lack of impact. More money will not necessarily improve education research, but it is difficult to foresee major improvements without an injection of additional resources.

It may be possible to reap some benefit from the current interest in improving the quality of education research. But a positive outcome is only likely to occur if the current makers of educational policies and decisions can resist the temptation to let ideology guide their decisions. According to Berliner (2002): "Promoting debate on a variety of educational issues among researchers and practitioners with different methodological perspectives would help both our scholars and our government to make fewer errors. Limiting who is funded and who will be invited to those debates is more likely to increase our errors" (p. 20). Through an open process of deliberation and debate about the quality, as well as the qualities, of education research it should be possible to develop what Feuer, Towne, and Shavelson (2002) call a *scientific culture* of education research within a community of education researchers and practitioners. Thus, they argue, the current demand for scientific evidence in relation to educational practice might be a catalyst for the improvement of the education research enterprise through the development of a "shared core of norms and practices that emphasize scientific principles" (p. 12). This optimistic assessment of the current situation may be actualized if we can keep our cynicism in check and if politicians ensure that ideologically based decisions do not become the norm.

This is a unique moment of challenge, and perhaps opportunity, for those who conduct research on mathematics teaching and learning. I invite all readers of *JRME* to begin a conversation with colleagues near and far on this important set of issues. A key opportunity for such discussion is the Research Presession prior to the NCTM meeting in San Antonio in April 2003; another is the joint international and North American PME conference in Hawaii in July 2003.

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