

Science Plus: A Response to the Responses to *Scientific Research in Education*

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I come to the task of commenting on these articles with mixed feelings. I was a member of the National Research Council Committee on Scientific Principles for Educational Research, the committee that produced *Scientific Research in Education* (National Research Council, 2002). In other words, I contributed to the work that the articles in this collection critique. I am also an anthropologist and qualitative researcher, so the authors of the articles here are my mentors, colleagues, and friends. They have written that they agree with some, even much, of what appears in *Scientific Research in Education* (SRE), but they also take serious issue with it. In this brief commentary, I wish to give my interpretation of SRE: What it does and does not say (to me) and why what it says is not enough—a point the articles in this collection make very clear.¹

Jim Gee (this issue) is right. SRE is a situated document. As a consensus report created by 16 researchers and a study director, sponsored by several agencies, written in a year's time in an effort to influence congressional legislation, and potentially affecting the lives of thousands of researchers and hundreds of thousands of students and teachers, the production of SRE is situated not in one theory but in multiple theories. It is also situated in politics, group dynamics, history, and context. To say it is a compromise(d) document is an understatement; to say it is not perfect is correct; to say it was not well-intentioned is inappropriate. I hope my commentary makes this clear.

THE CONTRIBUTION OF SRE

SRE is not the document I would have written if it had only been up to me, but it is a document I can support for the following reasons:

1. Whether we like it or not, think it justified or not, congressional leaders and others around the country have a very low opinion of education research (National Research Council, 2001). Their opinion is not new; in fact, it has deep historical roots (Lagemann, 2000), but it is

unfortunate because it leads people not to care whether *any* research gets done on the policies or practices affecting children, teachers, and schools. I think it is important to conduct research on educational practices and policies because we need to know as much as we can about whether the practices and policies we use or promote are doing what we want them to do. Without assuming that scientifically based research is the *only* way to learn about or decide among practices and policies, it is one good way, and it has political resonance for those (e.g., members of Congress) who can provide support for additional education research despite doubts about its value.

2. SRE tries to promote a conception of “science” or “scientifically based research” in the sense of postpositivism (Phillips & Burbules, 2000), not positivism (National Research Council, 2002, pp. 15–16). The postpositivist perspective on science is important for education research (and a conceptual advance over either positivism or interpretivism alone) because it can accommodate the role of *both* patterned behavior and human intentionality in human activity. Certainly postpositivism is not perfect, and there are other conceptions of science that we might have adopted (e.g., critical theory), but compared to others, postpositivism is relatively inclusive and relatively accessible.

3. SRE argues that the disciplines or fields that can provide examples of scientific research and the accumulation of knowledge appropriate to education are biology (National Research Council, 2002, pp. 31–33), political science (p. 56), and cultural anthropology (p. 107), not physics. The accumulation of general laws and the highly controlled experiments expected in physics are inadequate models for knowledge accumulation and scientific research in education because they cannot accommodate the influences of history, intentionality, and sociality that pervade human phenomena including education. Human intentionality, sociality, power, and history are usually considered irrelevant to investigations of physical phenomena, while they are fundamental to social and educational phenomena. This basic difference requires social researchers to ask different kinds of questions, to take different things into account, to need different tools and strategies to pursue their work, and to argue for their designs, results, and implications in much more complex circumstances. In life science, social science, and education, the purpose of scientific research is to develop, refine, and refute empirically based concepts and working theories that have meaning for their time and place, not to discover general laws that apply universally.

4. SRE argues that scientific research is useful for temporarily determining answers to certain kinds of questions, specifically questions for

which empirical evidence is crucial and can be gathered (National Research Council, pp. 24–26). Consistent with this position, scientific research is *not* an appropriate means of answering all questions. For example, it cannot answer questions about the moral value of one educational approach versus another because empirical evidence does not address moral questions. Very important educational questions must be addressed by philosophers (e.g., Is this the right educational policy to promote a democratic society?), critical theorists (e.g., Is this educational policy likely to reduce educational inequalities between rich and poor?), and others outside the purview of science.

5. SRE (National Research Council, pp. 24–26) makes clear that scientific research includes a variety of research designs and methods. It cannot be limited to a specific set of procedures or methods (e.g., randomized field trials) because the methods that are needed or possible always depend on the questions asked and the circumstances in which they can be investigated.

6. SRE argues that when scientific research is desirable, appropriate research methods and standards of quality depend on the question being asked, the state of the field, the context of investigation, and the answers being sought (National Research Council, chap. 5). The report states that the education research community, not politicians or legislators, should decide what constitutes necessary and good educational research at a given point in time.

Some of the things that SRE does not say are also important to me. It does not say that being scientific requires searching for causal explanations. It does not say being scientific means being neutral or value-free. It does not say that being scientific means proceeding in a linear or reductive fashion. It does not say that only experimental or quantitative methods can be scientific. It does not say that conducting scientific research leads to either methods or results that are infallible or unequivocal. The point of including illustrations of research programs from fields as diverse as cell biology, cultural anthropology, and political science was to show the theory- and context-dependence, value-ladenness, partiality, and messiness that characterize scientific research in the natural and social sciences as well as education.

Finally, one very important thing about SRE has been missed in commentaries about it: Its discussion of the kinds of research questions that characterize education research (see National Research Council, chap. 5). Commentators have criticized the report for privileging one kind of question—the What works? (or Is there a systematic [causal] effect?) question—and one method—randomized experiments—for addressing it. This

is an important class of education research, but it is not the only important kind. The report discusses two other classes of research questions: What is happening? and Why (or how) is it happening? These two classes of questions are as important, if not more important, than the “what works” class if for no other reason than that both of them must be answered before studies about what works can be valid. This is because accurate assessments of what works presume knowledge of what is going on. We must have careful, thorough descriptions of what’s happening (e.g., of what reading activities are *actually* occurring in the classroom or what reading policy is *actually* being implemented in a school; see also the excellent questions of this type suggested by Erickson, this issue, and Walker, this issue), before we can accurately compare the effects of one set of activities or policies with another. We must also have some explanation for why or how—the means or mechanism by which—something happens as it does. A finding that there is a correlation, or systematic connection, between reading program x and desirable reading outcome y is meaningless as a basis for action unless we can explain why this cause and this effect should be linked. Unless we have a credible explanation for the link, we cannot validly design randomized trials or any other kind of hypothesis-testing investigation. In my view, SRE should be credited for its attention to all three of these question types.

WHAT IS MISSING FROM SRE

As SRE states, both qualitative and quantitative methods can be appropriate for investigations of all three question types. Unfortunately, the report gives too little attention to how qualitative methods can contribute. This is a weakness of SRE that I deeply regret. Suffice it to say here that qualitative studies (e.g., ethnographies, case studies, observational studies) are generally better suited for providing the thorough descriptions necessary to address questions of what is happening and the explanatory processes necessary to address why things happen as they do (see also Eisenhart, in press, and Erickson, this volume). And as Bent Flyvbjerg (2001) writes, “It is often more important to clarify the deeper causes behind a given problem and its consequences than to describe the symptoms of the problem and how frequently they occur” (p. 78). Qualitative studies can also contribute to the question of what works because they permit direct, firsthand observations of what x affects what y (in some cases). For my money, qualitative studies are likely to offer more, not less, than quantitative studies to scientific research in education. I wish SRE had included more about this.

I also regret that SRE was not forceful enough to counteract political pressures to narrow scientific research in education to experimental studies. Despite some positive changes in the Education Sciences Reform Act in

accord with SRE (see Eisenhart & Towne, 2003, for further discussion of this point), the research priorities specified in *No Child Left Behind*, the U.S. Department of Education's current funding targets, and the emerging policies of the new Institute for Education Sciences are narrowly focused on experimental designs. In my view, it is these priorities, not those of the report itself, that are wrong. These federal priorities reflect a mistaken and long-rejected view of what scientific research is, as well as what it can be, and they hold education research hostage to *one* form of scientific investigation that makes sense in only a few, very limited contexts.

SCIENCE PLUS

In light of these federal priorities and the sometime association of SRE with them, what is especially important to me now is to speak out about what SRE leaves out or diminishes. This is a strength of this issue of *Teachers College Record*. We must go beyond SRE in a number of ways, including those discussed in the earlier articles. We need more than traditional science to improve education research:

1. The implications of interpretive science for research on human affairs must be emphasized in education research. Topics such as human intentionality, intentional causation, and sociality that have been the focus of interpretive research must be fully integrated into research on education. The list of questions at the end of Erickson's article highlights this need, as does Walker's call for increased attention to researcher subjectivity and introspection, and Moss's argument for renewed attention to beliefs, values, and cases.
2. As Erickson makes clear, the importance of research in philosophy, history, ethics, and literary criticism for research in education must be stressed. I would not call these types of research "scientific," nor would many who practice them, but they are forms of inquiry absolutely fundamental to educational research, policy, and practice.
3. The importance of critical research (broadly construed) in a free and democratic society, consistent with the messages in both Willinsky's and Moss's articles, must also be stressed. Critics, skeptics, and multiple perspectives should not be silenced but encouraged and taken seriously when education research, whether scientific or not, is being used to influence policies and practices affecting so many.
4. The infeasibility and absurdity of experimental and quasi-experimental designs for many if not most research questions in educational research must be exposed. While these designs are sometimes

appropriate and can perhaps be conducted more frequently than in the past, for the most part I agree with Dick Nelson (2003) when he wrote the following:

For many years, such experimentation has been high on the agenda of scientifically oriented schools of education. But consistently the record has been that what is reported to work in a lab school or another chosen testing locus has been hard to duplicate outside the locus of the original research. . . . [P]art of the problem clearly has been that it is impossible to describe what the experimental treatment was with sufficient precision and detail so that one could know whether one was replicating the key elements of it or not. Part is that the context conditions that enabled a particular treatment to work were not fully known, and not necessarily in existence in other places. And part surely is that evaluation takes time and in many cases does not yield unambiguous results. (p. 917)

5. In education, research must be practically relevant as well as scientifically proficient. The most elegant, sophisticated research designs can easily lead to naught if the results cannot be understood by practitioners, are not relevant to practice, or cannot be put into practice.

6. The need for more agreement about criteria for high quality education research is critical. We need principles that rule things in and out of research, and we need principles of quality that distinguish weak from strong research, depending on the research question and the research design. Unless we are able to tell policy makers and the public what constitutes good research on a given topic, we will continue to have trouble convincing them of the value of our (best) work.

I hope we can find a productive way to move forward on all of these things.

Notes

1 The views expressed here are mine alone. They do not represent the views of other members of the NRC committee or the NRC.

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