Commentary on the Final Report of the National Mathematics Advisory Panel

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Foundations for Success: The Final Report of the National Mathematics Advisory Panel (2008) excludes the view of students learning the social practices of mathematical reasoning and the use of mathematics in understanding and modeling situations. The authors argue that by filtering research to only "statistically significant individual effects, significant positive mean effect size, or equivalent consistent positive findings," the report misrepresents the resources that education research affords for improving mathematics education and education in general. The authors argue that only offering research results of statistical comparisons is inappropriately limited. They recall a strategy developed by the National Academy of Education in which researchers and educators collaborate to strengthen educational practice in local settings and to provide analyses and develop resources intended to support travel of their innovations to other sites.

Keywords: learning processes and strategies; mathematics education; research methodology

The National Mathematics Advisory Panel's (2008) final report, *Foundations for Success,* presents a competent summary, with suitably qualified recommendations for mathematics educators, of a highly selected subset of research findings and interpretations. We expect that if the 45 recommendations of the report were taken seriously across the nation, the average net effect would be marginally favorable.

Even so, as researchers we find the report profoundly disappointing. We agree with the educational aim that the Panel expressed: "The national workforce of future years will surely have to handle quantitative concepts more fully and more deftly than at present. So will the citizens and policy leaders who deal with the public interest in positions of civic leadership" (p. xii). Although we find the goal of a mathematically capable citizenry and workforce entirely admirable, the potential contribution of the Panel's report is severely limited by the omission of findings of much research that can provide important resources for efforts to strengthen mathematics education.

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What Are "High-Quality Studies" and "Strong Evidence," and by Whom and How Are These Determined?

The Panel's Subcommittee on Standards of Evidence applied the following filter in order to select research that was used in reaching its conclusions: "All of the applicable high-quality studies support a conclusion (statistically significant individual effects, significant positive mean effect size, or equivalent consistent positive findings)" (p. 2-2). The report does not attempt to justify this choice of a methodological constraint. Education researchers know that this view of research quality is held by some of our colleagues, but it by no means expresses a consensus of the field.

We dispute the validity of a decision made by a presidential advisory panel to adopt a methodological criterion that reflects a view held by some researchers but that is far from representing a consensus of the field. The methods of every scientific field evolve, and one of the most important functions of a scientific community is to continually examine, criticize, and improve its methods. Education researchers are vigorously engaged in this process of examination and debate as is reflected by the American Educational Research Association's (AERA) *Handbook of Complementary Methods* (Green, Camilli, & Elmore, 2006). The development and evaluation of methods of design experiments and design-based research is a particularly important issue in this process (e.g., Barab, 2004; Kelly, 2003). By selecting a criterion that represents the methods of the field only partially, the Panel's report misinforms the president and the society about the scientific resources that the field offers.

The report, and the methodological filter that determined its content, could be interpreted as being only about applied research, and it might be thought that a government-appointed body should have a more active role in declaring what methods are appropriate for studies relevant to the government's mission to assist the nation's schools. According to the report, "The primary interest of the Panel is experimental and quasi-experimental research designed to investigate the effects of programs, practices, and approaches on students' mathematics learning and achievement" (p. 2-3). We have no quarrel with a report intended to inform the president and the public about research "designed to investigate the effects of programs, practices, and approaches on students' mathematics learning and achievement." On the other hand, the decision by the Panel to limit its interest to "experimental and quasi-experimental research," in the narrow sense of these terms adopted by the Panel, resulted in a gross misrepresentation of the body of research findings that could and should inform the nation's efforts to improve mathematics education and education in general.[1] According to the Panel's report, the only way for research about "programs, practices, and approaches" to contribute to improvement of educational practice is to produce a statistically significant difference between an innovative treatment and something else. This simplistic view does not represent the variety of ways in which researchers and professional educators are experimenting with arrangements to find ways to interact productively, including developing methods and evidentiary criteria for design-based research (Barab, 2004; Kelly, 2003).

Scientific criteria for the quality of research studies and the strength of evidence are not determined by governmental edict. They are developed, enacted, debated, and settled in processes of scientific inquiry where competing claims for validity and significance are contested. The kinds of research that can best contribute to the improvement of educational practice and policy, and to the advancement of fundamental understanding in the learning sciences, are the subject of intense inquiry and debate in the research fields and in efforts to develop collaborative interactions between researchers and professional educators. An authentic report to the president and the American people should have reflected this dynamic condition. Instead, the Panel chose to pronounce a narrow methodological doctrine that has adherents in the scientific community but is by no means settled ground.

Questions About the Nature of Knowing and Learning Are Not Scientifically Settled

The Panel did not provide a discussion of its assumptions about knowing and learning. Currently among researchers there is considerable work that seeks to develop, understand, and evaluate alternative framing assumptions for the study of knowing and learning (e.g., Greeno, Collins, & Resnick, 1996; Moss et al., 2008). This is not just a theoretical enterprise, although it includes significant work on broad theoretical issues. One line of this theoretical work, variously called sociocultural, activity-theoretical, or situative,[2] is developing a framing for studies of learning that aims to be more comprehensive than the behaviorist and individual-cognitive perspectives that have framed much previous research in education, including mathematics education.

The more comprehensive view of learning that this effort is working to develop includes but is not limited to the kinds of domain knowledge and understanding that are easily measured with current tests. It also includes growth by students in productive participation in the social practices of mathematical reasoning and use of mathematics in understanding and modeling situations as well as for solving problems.

The assumptions about learning that are being developed in the sociocultural (or activity-theoretical or situative) framing have serious consequences for policies and the conduct of education, including mathematics education. According to this view, achieving the goal of a mathematically capable workforce and citizenry requires more than an increase in the number of students who take and succeed in Algebra II. The technical capabilities that have traditionally been taught, and learned by a minority of students, are only one aspect of what students need to develop to be productive and knowledgeable users of mathematics in their work and social lives.

In this view, students need to develop capabilities of interpreting and adapting mathematical representations, concepts, and methods as they construct their mathematical knowledge and understanding and use mathematics generally. To develop these capabilities, students need to be positioned in learning activities as effective agents of their learning and understanding, not as passive recipients of authorized knowledge who are taught simply to follow instructions and succeed on tests.

Neglected Issues and Research

The Panel's neglect of issues involving learning to participate in mathematical activity is illustrated by its discussion of understanding and skill, on which it wrote,

> Debates regarding the relative importance of conceptual knowledge, procedural skills (e.g., the standard algorithms), and the commitment of addition, subtraction, multiplication, and division facts to long-term memory are misguided. These capabilities are mutually supportive, each facilitating learning of the others. Conceptual understanding of mathematical operations, execution of procedures, and fast access to number combinations together support effective and efficient problem solving. (p. 26 and, with slight differences, p. xix)

Although the Panel discourages debate, the document seems to us to emphasize growth of skills. For example, "fluency with whole numbers," "fluency with fractions," and "particular aspects of geometry and measurement" are listed as the "critical foundations of algebra"; that is, they are scheduled to precede any instruction that involves functions. Six of the 11 "benchmarks for the critical foundations" read that "students should be proficient with <>," where <> is some set of computational procedures.[3] The report includes "understand key concepts" in its definition of proficiency (p. xvii) and admonishes teachers to "emphasize these relationships [between understanding, fluent execution, and fast memory access]" (p. xix), but the report does not try to characterize these relationships, other than to say that they are "mutually supportive" (p. 26) or "mutually reinforcing" (p. 3-40). There is a large research literature concerned with conceptual understanding of elementary school mathematics that the Panel apparently did not consider. For example, J. Moss and Case (1999) provided a model of conceptual structures involved in understanding fractions and developed and studied a curriculum in which students learned concepts of numerical percentage and fraction quite successfully.

However superficial the Panel's consideration of the issue of concepts, skills, and memory retrieval, the Panel neglected issues of participation in learning almost entirely. The report, therefore, presents a gross misrepresentation of the resources that exist now and that will be strengthened by future research and development. Studies of these issues have almost entirely been done using methods of design-based research and case studies rather than experimental or quasi-experimental comparisons between treatments. This may account for the Panel's omission of studies that could inform professional educators regarding aspects of classroom activity that fundamentally affect students' learning, knowing, and understanding mathematics.

Attending to aspects of learning involving participation in mathematical practices involves focusing on the social organization of learning environments. The potential efficacy of instruction that changes the social organization of mathematics learning has been demonstrated in design-based research and in case studies. Examples include Stein and Lane's (1996) study of middle school classrooms in the QUASAR project. The project developed a characterization of mathematics classroom tasks in terms of the cognitive demands on students as they participated in these tasks. They showed that in schools where there was a higher frequency of high-level demands, students performed better on an assessment that was designed to measure student understanding. Another example was Boaler's (2002) study of mathematics teaching and learning in two secondary schools in England. In one of the schools the students worked on investigations; in the other they learned by working on traditional problem sets. Students in the two schools developed what Boaler called different forms of mathematical knowledge. When asked to work on an open-ended problem, such as designing an apartment, students who had learned by working on investigations used mathematics as a resource for making inferences and evaluating possibilities, but students who had learned by practicing procedures were stymied by the lack of direction as to which procedures should be performed.

Although the cases that are studied in analytical research of this kind can include comparisons (as these examples did), the important findings of the research are not of the form Treatment A was better then Treatment B. Instead, design-based research and case studies provide empirical grounding that can expand the space of possibilities for education resources and practices. The conclusion is not just that something worked or worked on average better than something else, but that a particular kind of learning outcome is possible, along with detailed information about how that learning can be brought about.[4] Through these studies, and through detailed analyses of patterns of interaction in classrooms, the field is developing a more comprehensive theoretical understanding of learning and expansion of the space for designing and utilizing improved learning resources and practices. By ignoring this research, justifying its selectivity with its narrow criterion of evidentiary quality, the Panel's report neglected the most important issues that need to be addressed in order to improve mathematics education significantly.

Rethinking the Mathematics Curriculum

The Panel's report reflects the traditional view of the mathematics curriculum that predates the development of computational devices that can carry out all the algorithms that are taught up through graduate school. In fact, the charge to the Panel assumed that the traditional curriculum leading to algebra would remain in place, as do the standardized tests that are used to evaluate students' mathematical learning both in the United States and in other countries that the Panel used as benchmarks.

We think it is time to reconsider what should be taught in the mathematics curriculum in light of the changing needs for mathematics expertise in the modern world. Certainly the need for such expertise is increasing, as the Panel's report makes clear, but at the same time the nature of the needed expertise is also changing. As one example, it seems fair to say that practically no one, including scientists and engineers, ever has occasion to compute the area of a triangle or trapezoid, use the quadratic formula, or add and multiply fractions. Yet these are all included in the Panel's recommendations for the prealgebra and algebra curriculum. The development of computers has focused mathematical thinking on decimals (given their digital basis) and on the manipulation and use of functions, so it would make sense to spend more precious school time on these topics. We do not presume to say what exactly a new mathematics curriculum should include, only that it is time for society to rethink what mathematics it is teaching.

Furthermore, if we want to have a dramatic impact on students' ability to use mathematics in the real world, we think it will be important to embed real-world mathematical tasks into mathematics teaching. The kinds of problems that currently permeate the mathematics curriculum are not real problems but, rather, problems designed to illustrate the use of different algorithms (Lave, 1988). However, there are a variety of interesting mathematical tasks, such as representing different populations, designing buildings and artifacts, and modeling phenomena, that students can understand and that can be engaging for students to carry out. The efforts by Lehrer and Schauble (2006) to engage very young students in creating mathematical representations and models demonstrate what is possible in order to deeply affect students' understanding of mathematics. But their work is outside the purview of the Panel's report.[5]

A fundamental problem of the mathematics curriculum is that students are learning a large body of knowledge with practically no understanding of how that knowledge might be used in the world. We would argue that in order to make students' mathematical knowledge more useful, mathematics education needs to go back and forth between an emphasis on teaching particular skills and teaching how those skills can be used in the real world. That is to say, mathematics teaching should obey the "interweaving principle" that coaches use when they train someone in real-world activities, such as sports or theater (Collins, 1994). Coupling knowledge and its use is critical to deep learning.

The Panel's Limited View of Relations Between Research and Practice

The model of research and improvement—that, if viable, would justify the report's research criteria—assumes that the contributions of research to educational improvement are findings showing that an instructional treatment with some specified feature resulted in better average performance than an instructional treatment that lacked that feature. The kind of interface that is assumed between research and more effective education is a set of recommendations that specify features of curricular content, teaching and teacher education, instructional approaches, practices, materials, and assessment. The recommended features are said to be chosen on the basis of supporting research that showed that one or more programs with a recommended feature were more effective than programs without it by a statistically significant amount.

We believe that this view of how research and practice should be related has some serious limitations. It seems straightforward, but it neglects a fundamental feature of educational reality. A finding that something produced improvement on the average leaves open the question of what, in some specific situations, produced larger-than-average effects and what, in other situations, produced smaller-than-average or no or negative effects. In her AERA presidential address, Ann Brown (1994) argued (as did John Seely Brown, 2002, for nonschool settings) that learning programs by the same name always vary. So whether a school or district should adopt a program cannot simply depend on that program's having had, on average, a good effect. It also needs to be conditioned on whether the resources the program needs to succeed are available in that setting. The question others face, then, is whether their place is more like the places where this thing worked well as opposed to where it did not work, and what it would take for their place to be like the places where it worked. Helping people make that judgment requires analytical work, not just comparisons, and the Panel's report ignores that.

The difficulty of judging the applicability of a treatment in a local setting may be less severe if the innovation is relatively minor and technical than if it involves fundamental change. We expect that a small, well-specified treatment that can be added to a variety of instructional practices may be studied in a field trial in which its various versions are uniform enough to permit a conclusion that the treatment had or did not have an average positive or negative effect. If a school or district decides to adopt that change in practice, it can probably determine adequately what resources and activities are needed for it to have the effect it affords.

However, our understanding of the implications of education research during the past 30 years is that a deep transformation of our educational system is needed if we are to achieve the lofty goals of having a mathematically (and otherwise) capable workforce and citizenry. Deep transformations are not like amendments. They require changes not only in technical additions to established practice but also in underlying assumptions about the nature of the work that Ann Brown (1994) called first principles and that John Seely Brown (2002) called tacit assumptions. They are not detachable from settings in which they are constructed or importable into current practice in ways that permit them to be evaluated on the average. If our circumstances were different, if we currently had the general kind of educational system we need and were trying to make significant improvements here and there in it, randomized field trials could be productive. We are dubious about the capability of this research-and-development strategy to support the kind of fundamental change that we are convinced is needed.

A Different, More Promising, Model of Research and Improvement

Efforts to aid educational practice need to recognize that it is deeply situated in settings of local resources and constraints. We believe a strategy of developing exemplary models and providing resources for their spread is much more likely to result in improvement of education than general recommendations are.

A study group of the National Academy of Education (NAEd) said this about a decade ago. The NAEd (1999) study was commissioned by the National Educational Research Policy and Priorities Board to provide advice about priorities to the Office of Educational Research and Improvement. A section on conducting research in ways that could inform efforts to strengthen educational practice more effectively was contributed by a study group chaired by Lauren Resnick.

Resnick's group developed a conceptual model of research-and-development activity that the group called "problem-solving research and development." In this model, the relationship between education research and improvement is very different from that assumed (implicitly) in the Panel's report. The Panel report presupposes a traditional pipeline model of the flow of information from research to improvement of practice. According to this familiar, although overly simple, story, the complete pipeline has fundamental research at one end, which passes results to design and development, where instructional materials and resources such as curriculum or interactive software are constructed. These are evaluated in applied research, where the Panel's report picks up the story. Practitioners then receive lists of resources and programs that have been field tested and shown to have beneficial effects in the conditions of the evaluative research. A system of dissemination is needed to ensure widespread use of programs and resources that have been shown to be effective.

In contrast, projects organized as problem-solving research and development have very different relations between researchers and professional educators from those of the traditional model. The goals of the project include both improving educational effectiveness and contributing to the advancement of fundamental understanding of educational processes. This combination of effort toward both practical and theoretical scientific progress was discussed and advocated by Stokes (1997), who referred to work involving such combined goals as occurring in "Pasteur's quadrant." A team of workers, including researchers, professional educators, and designers and developers, takes on a shared commitment to work on a practical problem of improving some aspect or aspects of educational effectiveness, to develop resources that can contribute to the achievement of that improvement, and to study processes of change in educational practice and processes of student learning in order to contribute to fundamental understanding in educational and learning sciences. Although members of the team are all committed to achieving the multiple goals of their joint effort, they retain different professional identities and primary responsibilities and accountabilities for accomplishing those several goals. The problem-solving research and development model was illustrated in the NAEd group's report with examples available at the time. These included A. L. Brown and Campione's (1994) development and study of a program called Fostering Communities of Learners, and the Middle-School Mathematics Through Applications Project of curriculum development and research at the Institute for Research on Learning (Greeno et al., 1999).

Resnick's NAEd study group recognized that the concept of dissemination is inadequate to capture the kind of process that is needed for successful innovations in one setting to inform efforts to improve practice in other settings productively. They coined the term *travel* to refer to that issue. A successful effort to improve education in a setting necessarily takes account of relevant circumstances in that setting. It is too simple to suppose that the accomplishment can simply be scaled up by a process of dissemination. At the same time, we expect that in most, if not all, successful cases the programmatic changes and the experience of the team can provide a model that can inform efforts elsewhere to accomplish similar achievements. We also expect that successful travel—that is, the use of successful cases as models that can inform efforts in many settings—can be facilitated by the development of resources that are designed to aid the adaptation of the cases by others. We believe that questions about the process of travel and the kinds of resources that can facilitate it should be on the agenda of research and development in our field.

Conclusion

We criticize the Panel's report on two counts. First, the filter it applied to select the research it considered resulted in a presentation that misrepresents the resources that research findings and conceptualizations in the field could afford for the improvement of education. Second, the model of the research-improvement connection that underlies the Panel's report has limited utility, especially for bringing about fundamental change, because of the inherent dependence of changes in practice on local circumstances and assumptions about educational activity that are often largely tacit.

A model such as the NAEd study group's problem-solving research and development seems to us to be much more promising. It has two parts: first, designing and developing programs and resources that can support a kind of educational process that addresses the fundamental challenges of educating students to become effective members of our workforce and citizenry and conducting analytical research by studying the processes involved in these developments to advance fundamental understanding of education and learning; second, developing resources and processes that support travel of successful cases. Many who are doing the work of design, development, and research are engaged in the first part of this agenda. This work is far from finished, but much has been accomplished. The work of understanding and developing the kinds of resources that can support the widespread adoption of successful innovations (i.e., travel) is at a much earlier stage. If the Institute of Education Sciences focused resources and support on this problem, rather than the red herring of randomized trials, it could make significant contributions to progress on educational improvement.

NOTES

[1]The Panel did not exclude all research that was methodologically inconsistent with its version of "experimental and quasi-experimental research," including results of surveys and compilations of practice and informed opinion. However, these other methods were used for other "matters," such as "students' mathematical knowledge" and "the mathematical concepts essential to algebra" (p. 2-3). Our objection is with regard to the exclusion of research other than "experimental and quasi-experimental research" from consideration as "research designed to investigate the effects of programs, practices, and approaches on students' mathematics learning and achievement."

[2]The literature that represents this development is voluminous. The recent book edited by P. A. Moss, Pullin, Gee, Haertel, and Young (2008) includes several chapters that review aspects of it, including those by Gee; Greeno and Gresalfi; Lee; Mehan; Mislevy; P. A. Moss; and P. A. Moss, Girard, and Greeno.

[3]We are not aware of any research (let alone research meeting the Panel's stated methodological standards) that shows that comprehensive proficiency with fractions, emphasizing computation, is prerequisite for beginning to understand and learn about functions.

[4]As Schoenfeld (2006) argued, simply reporting that an innovation did or did not result in a learning gain is inappropriate. It is essential to specify what was measured in the assessments that were used to evaluate learning gains.

[5]Our point here is different from the one addressed in the report's Item 29 in its Findings and Recommendations, which referred to "the use of 'real-world' contexts to introduce mathematical ideas" (p. xxiii). We advocate a discussion that would consider reorienting the aims in mathematics education to include developing the skills and, more important, the dispositions of students in activities where mathematical concepts and methods are resources for knowing and reasoning in other domains (Gainsburg, 2007).

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