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Editorial

Research Pathways That Connect Research and Practice

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We concluded our November editorial (Cai et al., 2018b) with a promise to consider research paradigms that could bring us closer to the new world we have envisioned where research is intertwined with practice. We will call the paradigms we have in mind *research pathways* to avoid the range of complicated connotations often applied to the term *paradigm*. By research pathways in education, we mean the collection of assumptions that define the purposes of educational research, the principles that differentiate research from other educational activities, and the guidelines for how research should be conducted.

Calling for Alternative Research Pathways

In 2002, the National Research Council (NRC) issued a consensus report on the principles that define "scientific inquiry" in education. Many research pathways, representing distinctly different approaches to research, could be equally scientific based on these principles. For us, this means that the decision about which pathway to adopt is not a choice between pathways of different scientific rigor and integrity but rather a choice among pathways best suited to solve the targeted research problem. In addition, researchers need to consider the costs and benefits of different pathways. All research pathways have trade-offs. Researchers must decide which research pathway is best suited to solve the educational problem of interest and which has the most favorable benefit-to-cost ratio.

One long-standing research pathway to improve educational outcomes is composed of a sequence of research studies that, in a linear fashion, move from basic, exploratory research that identifies a promising educational intervention in a small controlled setting all the way to large-scale studies that demonstrate the efficacy of the intervention in real classrooms (Institute of Education Sciences [IES] & National Science Foundation [NSF], 2013). This well-known sequence promoted by the two major U.S. funding agencies of education research includes six different types of studies, each with a different purpose. The concept for this kind of research pathway was outlined years ago in a report to the president by Vannevar Bush (1945), a report that led to establishing the NSF. Bush argued that, in the physical and medical sciences, basic research must be protected from immediate application, with basic research preceding application research. Educational researchers have traditionally endorsed the basic-to-applied sequence, with research first identifying promising interventions in small settings and then scaling up to test the interventions in larger scale, more realistic settings.

Although this linear pathway has been the dominant approach for the last several generations of educational researchers, there has been an ongoing nagging sense that, as the studies scale up, they become too distant from the particular contexts of teaching for their findings to have a direct impact on individual classrooms. Moreover, they require ever-increasing resources to carry out. Thus, the costs of this pathway might outweigh its benefits.

Pathways thought to be better suited to informing classroom teaching with usable research data have been proposed along the way. Even before the current linear pathway became dominant, John Dewey (1929) proposed an approach to improving classroom practice by repeatedly creating and testing promising ideas, an approach illustrated in detail at the University of Chicago Laboratory School (Tanner, 1997). There was no linear sequence in Dewey's approach; rather, there were recursive cycles of hypothesizing, testing, refining, rehypothesizing, and so on. Decades later, a similar research pathway was implied in Gage's (1989) vision for a future in which research on teaching would make incremental but steady and lasting progress. Cronbach's (1986) call for more replications of small studies as well as calls for including implementation (a final stage in the linear sequence pathway) from the beginning as an integral part of the research process (Fullan & Pomfret, 1977; NRC, 2004; Schoenfeld, 2016) echo similar themes.

Many educational researchers in other countries have also followed pathways different from the linear pathway dominant in the United States. Two examples can be placed under the label *design research* (Cobb, Jackson, & Dunlap Sharpe, 2017). One is Gravemeijer and van Eerde's (2009) work on teacher and student learning using a series of teaching experiments on elementary arithmetic. A second example, detailed in our July 2017 editorial (Cai et al., 2017c), is a series of experiments in which Gu, Huang, and Gu (2017) searched for ways to help students develop a concept of perpendicularity. In both cases, the researchers engaged in cycles of testing instructional hypotheses and refining them; no linear sequence, as described above, was involved.

These alternative perspectives remind the reader that the current emphasis on a linear sequence of studies of different kinds with different purposes is not the only scientific research pathway that addresses problems of teaching and learning. If alternatives are possible, how do researchers choose between them? If scientific rigor and integrity are not the distinction, what criteria should be used to make these consequential decisions?

Understanding the Problem Research Is Trying to Solve

The first step in selecting or developing a research pathway is to understand deeply the problem the research is trying to solve. The better the problem is understood, the better the chance of using a research pathway especially suited to solving it—a pathway with more benefits than costs. The problem we have been addressing in past editorials is the disconnect between research and practice. This has consequences for both learning and teaching. From a view focused on student learning in the United States and many other Western countries, the major concern with a wide variety of mathematics education outcomes (cognitive and noncognitive, short term and long term) is that the mean performance of students is too low and the variance is unnecessarily high. The solutions that researchers are searching for are instructional methods for increasing the mean performance (using appropriately ambitious criteria for student learning) and decreasing the unnecessary variance (Bryk, Gomez, Grunow, & LeMahieu, 2015; Raudenbush, 2009).

From a view focused on classroom teaching, the major concern is the general malaise of unambitious mathematics teaching punctuated by pockets of excellence. Classroom teaching—the core of schooling under the control of educators—has, in most cases, remained stubbornly focused on a narrow, unambitious set of learning goals and has continued to use mostly recitation techniques with few rich learning opportunities (Cuban, 1993; Hiebert et al., 2005; Hoetker & Ahlbrand, 1969). The high levels of variance in teaching quality are often created by pockets of excellent teaching that are frequently found in local contexts with resources of all kinds, both inside and outside of school (Kristof, 2009; Valentino, 2018).

Understanding why research has had so little impact on classroom learning and teaching is the key to identifying research pathways that could have a more direct impact. In previous editorials, we have unpacked our view that there is not just one reason for the disconnect between research and practice but a combination of interrelated reasons: inattention to teachers' actual instructional problems (Cai et al., 2017); ignorance of the grain size of information that teachers need to improve their practice (Cai et al., 2017b); insufficient understanding of the influence of local contexts on the implementation and effectiveness of particular methods of teaching (Cai et al., 2017c); absence of a mechanism to build a shareable knowledge base for teaching (Cai et al., 2018a); institutional constraints that disincentivize researchers and teachers from building productive, sustainable partnerships (Cai et al., 2017a); and a culture that defines the professional roles of teachers and researchers that push them to adopt the traditional research pathway and discourage them from exploring others (Cai et al., 2018b). Given these legitimate concerns, it is not difficult to see why research has had little impact on practice.

Inevitable Limitations of the Traditional Linear Research Pathway

Although the linear research pathway has not been the only approach to studying the problems of teaching and learning, the general logic of exploratory, basic research followed by large-scale demonstrations of efficacy followed by implementation of the winning intervention is the most common sequence in funded U.S. education research, including mathematics education research (Coburn & Stein, 2010; IES & NSF, 2013). We are concerned about its continuing dominance because of what we believe are its limitations for improving mathematics teaching and learning. We will illustrate two limitations of this pathway.

A first limitation becomes visible when we imagine the challenge of addressing all the reasons cited above for the disconnect between research and practice. The challenge becomes especially daunting when noticing that many of these issues must be resolved by addressing them as related parts of a whole rather than as independent issues. For example, building a useful knowledge base for solving problems of classroom teaching requires knowing what kinds of information are most useful for teachers, which, in turn, requires knowing their most pressing instructional problems, which, in turn, requires knowing key details of the context in which they work and knowing the grain size of information that will address their problems. Consider building a knowledge base for teaching at a grain size that can help teachers plan daily lessons. A summary of the knowledge produced by IES-funded research identifies 28 claims suggested to improve mathematics teaching and learning (Rittle-Johnson & Jordan, 2016). The claims and elaborations will be of interest to many researchers but are not (yet) at a grain size that teachers could use to make lesson-level instructional decisions. There is a considerable gap between the description of what might work and the specific choices that teachers must make. Translations by educational mediators are needed to reach the appropriate grain size for a knowledge base that would be of practical use to teachers, but translations are often filled with pitfalls (Sabatini, 2009; Schoenfeld, 2006; Silver & Lunsford, 2017), one of the costs of this linear research pathway.

A second limitation of this pathway comes from its emphasis on large-scale, randomized control trials (RCTs) as the definitive test of whether an intervention works and should be implemented widely (Berliner & Glass, 2015; Foray, Murnane, & Nelson, 2007). The predicament for RCTs is that statistical tests based on probability theory require ignoring differences among individuals. According to Weisberg (2014), Bernoulli was the first to recognize that if individuals were treated as identical events, then probability theory could be applied to study treatment effects on people. This is a costly assumption in education because understanding the reasons for an intervention's effectiveness depends on understanding the variance—understanding how different students or teachers, situated in different contexts, respond differently to the same intervention (Berliner & Glass, 2015; Bryk et al., 2015).

As with all research pathways, the linear research pathway actually creates some of its own costs or side effects. Some of these side effects are educationally significant. One example is the rise in concern about fidelity of implementation, a side effect that has become a research agenda in its own right. If the findings from an RCT are to be interpreted correctly, researchers must have some assurance that the intervention was implemented as intended across all testing sites. Then, if the intervention is shown to be effective (on average) and warrants large-scale implementation, its continued effectiveness depends on further fidelity of implementation across large numbers of different settings and conditions. Many examples in educational research have shown how difficult this is (see Quinn & Kim, 2017). The problem of implementation is a key reason for the dilution, mutation, or abandonment of many promising ideas in mathematics education, including large-scale efforts such as the introduction of "New Math" curricula and Standards-based mathematics (Battista, 1994; Brown & Campione, 1996; Bruner, 1996; Fey, 1979; Kramer, Cai, & Merlino, 2015).

Promises of Alternative Research Pathways

As we have noted, we are certainly not the first to propose alternatives to the dominant linear research pathway. Many researchers have expressed similar concerns, and we do not claim to provide grand new insights beyond those creatively expressed by others (e.g., Bryk et al., 2015; Cronbach, 1986; Dewey, 1929; Popper, 1944/1985; Snow, 2016; Weisberg, 2014). For example, design research follows many of the principles we identify as central to a research pathway that is more connected to practice (Cobb, Confrey, diSessa, Lehrer, & Schauble, 2003; Collins, Joseph, & Bielaczyc, 2004; Kelly & Lesh, 2000; Lamberg, & Middleton, 2009). In addition, professions outside of education have faced similar disconnects between research and practice and have experimented with different approaches (Morris & Hiebert, 2009). Our goal in this section is to draw on these sources to outline the parameters for a research pathway that is consistent with the ideas we have developed in previous editorials for increasing the impact of research on practice.

What research pathway could address, in one integrated approach, the reasons cited above for the weak effect of research on practice? Outside of education, one of the most developed alternative pathways connecting research and practice has emerged in medical practice. Dissatisfied with the lack of information at a grain size useful for clinical practice, a number of physicians have been searching for an alternative to relying on the translation of RCT findings (Kenney, 2008). Building on the quality improvement principles developed by W. Edwards Deming (1982), physicians began a "science of improvement" (Berwick, 2008) devoted to the continuous, incremental, and steady improvement of clinical practice. We propose a *science of improvement* as an alternative research pathway in education.

At the heart of a science of improvement in education is the direct study of classroom teaching with the aim of improving the mean quality of teaching and reducing its variance. The process begins by identifying and attempting to solve users' (teachers') problems (Douthwaite, 2002; Morris & Hiebert, 2011; Stigler, Hiebert, & Givvin, 2018). Teachers' instructional problems are naturally situated in particular contexts, and solving them requires understanding these contexts in detail and producing reliable information at a grain size that teachers can use. To gather this information, the traditional linear sequence of different kinds of studies is replaced by disciplined recursive cycles of generating hypotheses of improvements to current practice, testing the hypotheses across different contexts, analyzing the results, and changing the practice or refining the hypotheses (Bryk et al., 2015). Note that this alternative pathway flips the linear research pathway on its head because studying the implementation of the intervention happens at the beginning as part of the development of the intervention.

New technologies will enhance this process in several ways, including enabling virtual partnerships between teachers and researchers, gathering and processing data on teaching and learning in classrooms, and sharing the professional knowledge that is developed. To test hypotheses about potential improvements to instruction and learning and, in particular, to determine why improvements do or do not work under a particular set of conditions, extensive, detailed data on instruction and students' thinking must be gathered. Moreover, the shared knowledge base allows a large number of teachers and students to be engaged in the testing and refining of shared hypotheses about learning and instruction. This means that each classroom engaged in testing a hypothesis is, in a sense, an exploratory study but also part of a scale-up study—in other words, basic research is integrated with applied research.

In addition, the improvement process continues indefinitely as new problems emerge and better solutions are proposed. This stands in contrast to the typical pattern of traditional RCT scale-up research in which there is usually no planned mechanism for an intervention or program to be sustained after the study is complete (Gutiérrez & Penuel, 2014). Teacher–researcher partnerships are a sustainable structure within which a science of improvement can thrive (Bryk et al., 2015; Schoenfeld, 2016). We believe that a science of improvement, as a research pathway adopted by teacher–researcher partnerships, will change the culture and professional expectations of teachers and researchers (Cai et al., 2017a, 2018b). Indeed, a key difference between traditional RCT scale-up research and the kind of scale-up associated with a science of improvement lies precisely in what is expected from the participants. According to the traditional research pathway, as a research program scales up, the researchers must increasingly disengage themselves from the teachers and students as the number of participants grows to increase objectivity. In contrast, scale-up in a science of improvement involves gradually growing the number of teacher–researcher partnerships with both teachers and researchers staying close to the data.

It should be clear that this alternative research pathway defines scientific work suited to addressing the issues that we identified as preventing a close connection between research and practice. Similar pathways (e.g., design research) have already demonstrated their value in providing research-based solutions to problems of teaching and learning (Cobb et al., 2017). However, the alternative research pathway that we describe here, with its continuing lifetime of cycles of improvement and its gradual but steady scale-up of connected partnerships, remains a vision for the future.

Challenges to Enacting This Alternative Pathway

As we noted, all research pathways have costs and benefits. The science of improvement is no different. The benefits are apparent and targeted toward exactly those issues that enable incremental but lasting improvement in teaching and learning. What are the costs? The first is time, both in the short run and in the long run. In the short run, teachers (and researchers) will find it difficult to schedule time to collaborate on cycles of improving teaching. In the long run, the pathway we propose will take years to have an impact on practice at a scale that can be noticed at the national level. The second cost is agreeing on long-term national priorities for educational outcomes. Until enough researchers, teachers, standards, and curriculum materials share the same stable mathematics learning goals for students, continuous improvements will be erratic, isolated, and local. The third cost is a change in professional identities. Providing the training, institutional structures, and incentives to change the professional identities and daily lives of researchers and teachers has never been seriously tried before. In fact, the difficulty of obtaining funding for this kind of work acts as a strong disincentive for researchers to propose and participate in such projects.

These costs are severe, not because they interfere with the connection between research and practice, but because, up to this point in the history of U.S. education research, they have prevented this pathway from gaining traction. For example, coming to a national consensus on priorities for educational outcomes has been elusive in the United States. Although multiple standards documents (e.g., National Council of Teachers of Mathematics, 1989, 2000; National Governors Association

Center for Best Practices & Council of Chief State School Officers, 2010) have significantly influenced mathematics education, they have also been accompanied by much controversy and resistance (e.g., the "math wars" of the 1990s and push-back on the *Common Core State Standards for Mathematics*).

We do not minimize these problems. Thus far, they have prevented the future world that we envision from becoming reality. However, we are optimistic that a growing number of researchers and teachers can see a future where research can have an impact on practice. We hope the series of editorials that we now conclude has provided insights into how this future might function, thereby offering guidelines that move the future closer to the present. We also hope the alternative research pathway that we sketched in this editorial provides a foundation for our collective efforts to make research and practice an intertwined activity in mathematics education.

With the March 2019 issue, we will begin a new series of editorials that will examine some guiding principles for conducting and disseminating research that has an impact on practice. Drawing on our experience as researchers and as members of the *JRME* editorial team, we will discuss issues including the identification and selection of significant research questions, the framing of a study, choices of methodology within and outside of mathematics education, and the crafting of a research report. We hope that by situating these discussions in the practical work of mathematics education researchers as well as in the wealth of studies and historical data contained in the *JRME* archive, we will provide useful perspectives for those in our community who are just beginning to conduct research as well as those who are seasoned mathematics education researchers.

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