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Editorial

Posing Significant Research Questions

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In 2002, the National Research Council (NRC) released *Scientific Research in Education*, a report that proposed six principles to serve as guidelines for all scientific inquiry in education. The first of these principles was to "pose significant questions that can be investigated empirically" (p. 3). The report argued that the significance of a question could be established on a foundation of existing theoretical, methodological, and empirical work. However, it is not always clear what counts as a significant question in educational research or where such questions come from. Moreover, our analysis of the reviews for manuscripts submitted to *JRME*[1] suggests that some practical, specific guidance could help researchers develop a significant question or make the case for the significance of a research question when preparing reports of research for publication.

Building on the *JRME* archive of nearly 50 years of research articles, this issue marks the beginning of a series of editorials aimed at discussing how to conduct and report high-quality research in mathematics education. In this first editorial in the series, we discuss what counts as a significant research question in mathematics education research, where significant research questions come from, and how researchers can develop their manuscripts to make the case for the significance of their research questions. Although we are beginning a new series of editorials, we will continue to draw on the ideas from our editorials over the past 2 years (e.g., Cai et al., 2018; Cai et al., 2017). In particular, we consider what significant research questions might look like in the aspirational future world of research that we have described in those editorials—a world in which mathematics education research is carried out by widespread, stable partnerships of teachers and researchers and in which research both takes root in and shapes the everyday practices of mathematics teaching and learning.

Significant Research Questions

It is difficult, if not impossible, to judge the significance of a research question just by reading the question. Certainly, significant research in mathematics education should advance the field's knowledge and understanding of the teaching and learning of mathematics (Heid, 2010; Simon, 2004). We believe this implies that the characteristics that make a research question significant are dependent on

[1] We analyzed the reviews for every manuscript that underwent full review and received a decision in 2017. For those manuscripts that were ultimately rejected, not a single reviewer stated that the research questions were particularly relevant or insightful. In contrast, for those manuscripts that ultimately received a revise and resubmit decision or were accepted (pending revisions), only one reviewer raised the concern that the research questions would not make a contribution to the field.

context and specifically on assumptions about what kind of knowledge is useful. Research can advance our understanding of teaching and learning mathematics in ways that are more distant from the classroom or more connected to practice. Although we acknowledge the value of research that is more distant and might eventually have an effect on classroom teaching or learning, we have developed the argument in our previous editorials that significant research in mathematics education can, and perhaps should, be much closer to the classroom and aim to directly impact practice. From this perspective, we begin by asserting that a research question that addresses teachers' shared instructional problems and one whose answer helps the field (students, teachers, policy makers, researchers) understand why and how the answer is a solution to the problem is likely to be a significant research question.

Addressing Instructional Problems

We focus on teachers' instructional problems because they provide a strong basis for connecting the work of research to the challenges of teaching and learning mathematics. Confrey (2017) argued that mathematics education research is grounded in a "practical wisdom" that reflects the challenge of operating in complex decision-making environments like classrooms and schools. Because of this, significant research questions can and do arise directly or indirectly from teachers' problems of practice.[2] Within the idealized portrait of a future world of mathematics education research described in our previous editorials (Cai et al., 2017a, 2019), significant research questions arise from interactions between researchers and teachers about challenges that teachers face in establishing and helping students achieve well-defined learning goals. Grounding a research question in instructional problems that are experienced across multiple teachers' classrooms helps to ensure that the answer to the question will be of sufficient scope to be relevant and significant beyond the local context.

Significance is also drawn from the importance of the mathematics that is investigated. Instructional problems that lead to significant research questions are problems related to teaching and learning powerful mathematics—mathematics that is valued by the mathematics education community (broadly conceived). Our vision of important mathematics is inclusive. It includes mathematics content, the nature and practices of mathematics as a discipline, beliefs about mathematics and affective perceptions of mathematics as a powerful and useful tool, and the role and use of mathematics in addressing inequities (Cai et al., 2017b).[3]

For an example, we revisit the instructional problem faced by the fourth-grade teacher, Mr. Lovemath, described in our earlier editorials (Cai et al., 2017a, 2017b). Mr. Lovemath intended for his students to explore multiple strategies for completing a fraction comparison task, but the students were unable to make

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[2] This point also finds support in *Scientific Research in Education* (NRC, 2002) in its discussion of Pasteur's quadrant—the intersection of the quest for fundamental understanding and considerations of use (Stokes, 1997).

[3] Indeed, Confrey (2017) points out that the fourth of Flyvbjerg's (2001) questions that characterize research in social science—Who gains and loses from the intervention?—puts questions of equity squarely in the sights of mathematics education researchers.

progress on the task and ended up employing a single procedure (using common denominators) to perform all the comparisons. The students' difficulties with the task led Mr. Lovemath and Ms. Research, a mathematics education researcher, to identify several relevant questions: Why did the students encounter difficulties? Why did the intended opportunity to learn mathematics not materialize? What prior knowledge do students need to take advantage of this learning opportunity? These questions are grounded in an instructional problem that is likely shared by many teachers who are trying to help their students achieve this learning goal. Answering these questions would generate insight into students' learning of important mathematics and would also shed light on ways to make the learning opportunities in the task available to all students.

Understanding How and Why

Research questions that focus on teachers' instructional problems gain additional significance when they move from only finding answers to the problem to also understanding how and why the answer is a solution to the instructional problem. This distinguishes significant research from many other educational activities (e.g., conducting a successful professional development). Understanding how and why builds knowledge of a type that enables the solution to the initial instructional problem to be adapted for a related problem or a different context. Research questions that aim to understand often ask about the conditions under which the solution to a problem will work rather than simply asking about the nature of the solution. Years ago, Cronbach (1986) noted the value of these kinds of questions, using the classic Brownell and Moser (1949) study[4] as an example of research that went beyond a simple comparison of treatments (a what-works-best horse race) to examine how different treatments operated under different conditions. More recently, Maxwell (2004) argued for the importance of causal explanation in educational research, specifically highlighting the explanatory significance of underlying causal processes and the importance of the context in shaping those processes in particular situations. Studying the conditions under which a solution works allows teachers and researchers to generate further hypotheses about the changes in students' learning that other instructional choices might produce.

For example, in the case of Mr. Lovemath's instructional problem, Ms. Research might ask what prior knowledge the students need to take advantage of the learning opportunity in the fraction comparison task. An answer that reflects the potential significance of this question would be more than just a list of prerequisite concepts. Ms. Research's answer should address the field's theoretical understanding of the role that earlier concepts play in new learning. Furthermore, the answer's description of prior knowledge should, itself, attend to important mathematics—the conceptual structures that underlie fraction comparison and how particular concepts are needed at different points to engage productively with the task.

[4] Brownell and Moser (1949) studied two approaches for teaching subtraction (*meaningful* and *mechanical*) under two different conditions (using the regrouping algorithm for subtraction and using the equal additions algorithm). By crossing the two instructional approaches with the two different algorithms (regrouping and equal additions), Brownell and Moser found, among other results, that the meaningful approach produced better outcomes than the mechanical approach for the regrouping algorithm but not for the equal additions algorithm.

Making these conditions explicit would allow Mr. Lovemath and Ms. Research to make and test new predictions about how students would engage with the task after making specific changes to the instruction leading up to the task. The thinking behind these predictions could also then inform research and practice in other classrooms (perhaps using different curricula) in which teachers encounter a similar instructional problem.

Looking across the problems of practice discussed in our previous editorials, we can identify additional types of significant questions that might arise from problems of practice. These include questions about the resources (in addition to the prior knowledge discussed above) that students bring with them that would help or hinder them in taking advantage of a learning opportunity, questions about the arrangements of learning goals and subgoals into learning trajectories, questions about the kinds of data that would usefully inform teaching, and questions that focus on teacher–researcher partnerships and their work. Fundamentally, our message is that significant research questions can be generated by addressing problems of practice while striving to understand underlying mechanisms and their interactions with the context. Although we have not described every significant research question that can be posed in mathematics education, we believe that the kind of knowledge produced by answering research questions like these is useful and likely to have an impact on practice.

Communicating the Significance of Research Questions

Perceiving and formulating a significant research question is both a science and an art. The mathematician Jacques Hadamard (1945) wrote that "this delicate choice is one of the most important things in research" (p. 126). Einstein and Infeld (1938) claimed that "to raise new questions, new possibilities, to regard old problems from a new angle, requires creative imagination and marks real advance in science" (p. 95). Indeed, Klamkin (1968) claimed that "among professional mathematicians, asking questions rates almost as high as answering them" (p. 132). However, formulating a significant research question does not ensure that audiences will perceive its significance. It is still necessary to communicate that question in such a way that the field appreciates its significance. This communication, often embedded in research papers, depends on clearly formulating the research question for readers and making convincing arguments for its significance.

It can be a challenge to formulate a research question clearly in a research report. As Heid and Blume (2011) observed regarding manuscripts submitted to *JRME*,

> the *statement* of the research question is often an issue in submissions. Authors sometimes fail to specify their research question(s), and even when they do, they sometimes report only a general research problem or area of interest rather than a specific research question. (p. 106)

Our analysis of the reviews for the manuscripts submitted to *JRME* that received a full-review decision in 2017 provides empirical data supporting this observation by Heid and Blume. Fully 55% of the reviews for those manuscripts that were rejected in 2017 included concerns about the research questions, including the lack of a clear motivation for the research questions and a failure to appropriately connect the research questions to other parts of the manuscript (e.g., situating the questions with respect to the theoretical framework or describing methods appropriate for investigating the questions). Even for those manuscripts that were ultimately accepted pending revisions or that received a decision of revise and resubmit, 17% and 23% of the reviews, respectively, included concerns about the research questions such as the need to make the statement of the research questions somewhat clearer.

Thus, communicating the significance of a research question involves several considerations. First, the question must be explicitly stated with specificity and precision. It is neither sufficient nor fair to the reader to merely imply the question, to phrase it only as a goal of the study, or to merely describe a general problem, instructional or otherwise. A precisely stated research question should make clear what kinds of data are needed to answer the question and what an answer would look like. Precision in the statement of a research question can pay dividends in terms of how well the data will help the researcher and, ultimately, the readers to understand the phenomenon being studied.

A second consideration is that the research question must be clearly connected to prior research to situate it in the larger field of mathematics education research. The significance of a research question cannot be determined just by reading it. Rather, its significance stands in relation to the knowledge of the field. The justification for the research question itself—why it is a significant question to investigate—must therefore be made clear through an explicit argument that ties the research question to what is and is not already known. Indeed, nearly one quarter of the *JRME* reviews that highlighted issues with the research questions in manuscripts rejected in 2017 specifically called for authors to make this kind of argument to motivate the research questions, whereas none of the manuscripts that were ultimately accepted (pending revisions) received this kind of comment.

Thus, through a research question's connections to prior research, it should be clear how answering the question extends the field's knowledge because it is based on hypotheses suggested by previous research. The argument that there is a lack of research in a particular area is not, on its own, a strong justification. To successfully make the case that a research question extends the field's knowledge, the question must be situated within a theoretical framework that helps readers understand how answering the question informs the field and, consequently, practice or policy. Although an appeal to an external source can be helpful to establish that the field has an interest in the question, it is not a shortcut to making the case for the significance of the question. That case relies on a chain of justification forged from a theoretical framework that draws on the knowledge of the field. From the perspective of the future world of mathematics education research that we described in our previous editorials, that case can rest on whether the question addresses instructional problems shared by teachers and how the question will aim the investigation toward the conditions under which a solution to the instructional problem works (i.e., how answering the question will help the field understand why and how the answer is a solution to the problem).

Finally, a clear and warranted question must be presented in such a way that it can be empirically investigated and such that the methods for investigation make sense and follow logically from the question. Indeed, the research question should be coherent with the methods and data analysis so that, together, they make a strong argument. That argument should be tight but should also flow smoothly like a convincing story or a winning argument in a debate. It should make it easy for readers to be convinced, and readers should not need to fill in part of the argument. On the one hand, the argument for the significance of the research question depends on a theoretical framework. The theoretical framework shapes the researcher's conception of the phenomenon of interest, provides insight into it, and defines the kinds of questions that can be asked about it. On the other hand, there are many possible theoretical frameworks. Choosing among them depends on how productively they allow the researcher to engage with the research problem and to formulate good questions. This mutual dependence means that formulating a significant research question is an iterative process, one that successively moves from a broad, general sense of an idea which is potentially fruitful to a well-specified theoretical framework and a clearly stated research question.

Like formulating a significant research question, the choice or construction of a theoretical framework is also something of an art.[5] In the next editorial, we will discuss in detail how a theoretical framework can be chosen or constructed to justify and communicate the significance of research questions. In addition, we will address how the coherence of the research question, design, data coding and analyses, and presentation and discussion of the findings as a chain of arguments depends on presenting a relevant theoretical framework.

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[5] We see a parallel to formulating good research questions in mathematics, as Hadamard (1945) describes it: "The guide we must confide in is that sense of scientific beauty, that special esthetic sensibility" (p. 127).

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