Mathematics Education Research, Its Nature, and Its Purpose: A Discussion of Lester's Paper

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Abstract: This commentary discusses the framework for mathematics education researchers outlined in Lester's (2005) paper. The author reacts to (a) Lester's concern about the current political forces in the U.S. to define scientific research in education rigidly, and offers a possible reason—apart from political ideology—for the emergence of these forces; (b) recapitulates Lester's outline and model for theory-based research in mathematics education, and interprets Lester's paper as a call to the MER community to respond to the current political forces that (inappropriately) shape our field, and (c) addresses the role of mathematical context in MER, a topic absent from the paper's narrative.

ZDM Classification: B10, D20, E20

Introduction

Lester's paper is a significant contribution to mathematics education research (MER) because it sets a vision and provides a framework for mathematics education researchers to think about the purposes and nature of their field. My reaction to the paper is organized into three sections. In the first section I react to Lester's concern about the current political forces in the U.S. to define scientific research in education rigidly, and offer a possible reason—apart from political ideology—for the emergence of these forces. In the second section I recapitulate Lester's outline and model for theory-based research in mathematics education, and I interpret his paper as a call to the MER community to respond to the current political forces that (inappropriately) shape our field. Also, in this section, I describe reasons implied from Lester's paper as to why graduate programs in mathematics education must strengthen the theory and philosophy components of their course requirements. The third, and final, section addresses the role of mathematical context in MER, a topic absent from the paper's narrative. Despite the absence of such explicit discussion, I found in Lester's paper important elements on which to base the argument that theory-based mathematics education research must be rooted in mathematical context. An implication of this argument is the need to strengthen the quality of the mathematics component in graduate programs in mathematics education. I will illustrate this argument and its implication with an example concerning the proof-versus-argumentation phenomenon.

Rigid Definition of Scientific Research in Education

Lester's paper starts with a discussion of the current political forces in the U.S. to define *scientific research* in education as an area that employs a research paradigm whose primary characteristics are randomized experiments and controlled clinical trials—what is referred to in the No Child Left Behind Act as *scientifically-based research* (SBR). This rigid definition seems to be based on the assumption that the ultimate purpose of research in education is to determine "what works," *and* that, to achieve this goal, SRB methods must be employed. Lester points out that experimental methods underlying the SRB approach were dominant in MER until the 70s but were abandoned primarily because they were found inadequate to answer "what works" questions. It should be clear that what was abandoned was not the experimental research design methodology, which, undoubtedly, is needed for answering certain research questions. Rather, what was abandoned is the principle that this methodology must be applied uniformly to all MER investigations. The MER community realized that answering questions dealing with complexities of human thoughts and actions—specifically those concerning the learning and teaching of mathematics—requires adopting and even inventing other research methodologies. For example, the emergence of the modern "teaching experiment" methodology (Steffe & Thompson, 2000) was driven by questions concerning the development of mathematical knowledge in authentic classroom settings.

This raises an obvious question: Why is it that those who insist on adopting SBR methods indiscriminately in all areas of educational research and independently of the research question at hand seem to have ignored the reasons for their abandonment in MER? Could this be a result of lack of confidence in the quality of educational research—MER included—among lawmakers and the public at large? Feuer, Towne, and Shavelson (2002) argue that evidence exists to support the contention that educational research is perceived to be of low quality, even among educational researchers themselves! Reports lamenting the lack of value of research in education are not unique to the U.S.; similar critiques have been advanced in many other countries (Levine, 2004). One of the reasons given by Feuer et al. for this situation is that theory in educational research is often weak or absent, which is precisely the topic of Lester's paper.

The Role of Theory

Lester's paper can be interpreted—and this, I believe, is one of its values—as a call to the MER community to respond to the perception that led to the SRB movement by promoting better research in mathematics education. A critical task for promoting better research is "nurturing and reinforcing a scientific culture, [defined as] a set of norms and practices and an ethos of honesty, openness, and continuous reflection, including how research quality is judged" (Feuer at al., 2002, p. 4). Lester addresses one crucial weakness of the current scientific culture in MER—the lack of attention to theory and philosophy. He argues that "the role of theory and the nature of the philosophical underpinnings of our research have been absent" (p. 457). Lester identifies three major problems that contribute to this weakness. The first, relatively new problem is that the current pressure from governmental funding agencies to do "what works" research has likely decreased the researchers' attention to theory building. The other two problems are: (a) the widespread misunderstanding among researchers of what it means to adopt a theoretical or conceptual stance toward one's work and (b) a feeling on the part of many researchers that they are not qualified to engage in work involving theoretical and philosophical considerations. These two problems, unlike the first, are internal to the MER community in that they are the result of the state of graduate programs in the U.S, which are "woefully lacking in courses and experiences that provide students with solid theoretical and philosophical grounding for future research" (p. 461). As internal problems, argues Lester, they can and should be addressed from within the MER community. Accordingly, Lester calls for the MER community to "do a better job of cultivating a predilection [among graduate students] for carefully conceptualized frameworks to guide our research," and he gives compelling reasons for the need to advance this cause. For example, he argues that "without a [research] framework, the researcher can speculate at best or offer no explanation at all" (p. 461). Other scholars have made a similar call: "One of the crucial points for the development of theoretical foundation of mathematics education is, without doubt, the preparation of researchers in the field" (Batanero, Godino, Steiner & Wenzelburger, 1992, p.2).

Lester's call to promote theory-based research in mathematics education is accompanied with (a) an outline of the role of theory in education research and (b) a discussion of the impact of one's philosophical stance on the sort of research one does. Regarding the first of these items, he offers a model to think about educational research in general and MER in particular. Lester's model is an adaptation of Stokes' (1997) "dynamic" model for thinking about scientific and technological research, which blends two motives: "the quest for *fundamental understanding* and *considerations of use*" (p. 465). The value of Lester's model is precisely its emphasis on merging theory and practice in MER. Essentially, this is a cyclical model where existing understanding (of fundamental problems) and existing products (such as curricula and educational policies) are inputs (to-be-investigated phenomena) for "use-inspired basic research"—research whose goals are, in turn, improved understanding and improved products.

Regarding the second item, Lester illustrates Churchman's (1971) typology of inquiry systems—Leibnizian , Lockean, Kantian, Hegelian, and Singerian—by considering how these systems might be applied to a significant research question in mathematics education. The question, which has generated major controversy among educators, is: Which curricula, the "traditional" or the "reform," provide the most appropriate means of developing mathematical competence? Lester's point in this discussion is not that the application of Churchman's framework can, in principle, resolve this or any other controversy in the education community. Rather, his point is that Churchman's framework can be very useful for researchers to think about fundamental questions concerning their research.

Lester's discussion of these two items implies strong reasons for why graduate programs in mathematics education must strengthen the theory and philosophy components of their course requirements. I highlight three reasons: First, adequate grounding in philosophy is needed for researchers to address fundamental questions about the nature of inferences, evidence, and warrants of arguments one brings to bear in one's research, as well as the morality and practicality of one's research claims. Second, and entailed from the first, to address such questions competently one must have adequate preparation in theory. For example, in applying the Kantian enquiry system, one must know how to design different studies with different theoretical perspectives, and one must understand why each such design might necessitate the collection of different sets of data and might lead to very different explanations for the results of the studies. Hence, a solid knowledge of different theories and their implications regarding the learning and teaching of mathematics is essential. Third, to develop a disposition to reason theoretically and philosophically, novice researchers must engage in problematic situations involving theoretical and philosophical considerations. Graduate students can benefit greatly from problem-solving based courses—as opposed to descriptive courses often offered—where the problems are designed to help students understand different theories and inquiry systems relevant to MER. More important than understanding a particular theory or inquiry system, however, is the goal to help our graduate students develop the ability to inquire about theories and about inquiry systems—what Lester, after Churchman, calls *Singerian*:

> Such an approach entails a constant questioning of the assumptions of inquiry systems. Tenets, no matter how fundamental they appear to be, are themselves to be challenged in order to cast a new light on the situation under investigation. This leads directly and naturally to examination of the values and ethical considerations inherent in theory building. (p. 463)

For these reasons, I identify in Lester's paper a suitable structure for a series of graduate courses, whose collective goal is to develop among students a predilection to reason theoretically and philosophically about research in education. Absent from the paper's narrative, however, is the role of mathematical context in theory-based research in *mathematics* education.

The Role of Mathematical Context

Clearly, Lester's paper does not purport to address all aspects of the training mathematics education researchers should receive to conduct theory-based research. However, absent from the narrative of the paper is an explicit discussion about the role of the disciplinary context in considerations of conceptual, structural foundations of our research. Despite this, I found in Lester's paper important elements on which to base the argument that theory-based mathematics education research must be rooted in mathematical context. With this argument, I believe Lester's call to the MER community to cultivate a predilection among graduate students for theory-based research should be augmented with a call to promote a strong mathematics background among these students.

The first of these elements is the attention in Lester's model to both *fundamental understanding* and *considerations of use*. The second element is the notion of *research framework,* which Lester defines as

> … a basic structure of the ideas (i.e., abstractions and relationships) that serve as the basis for a phenomenon that is to be investigated. These abstractions and the (assumed) interrelationships among them represent the relevant features of the phenomenon as determined by the research perspective that has been adopted. The abstractions and interrelationships are then used as the basis and justification for all aspects of the research. (p. 458)

The third, and last, element is Lester's reference to context:

> ... because [a conceptual framework (a form of a research framework)] only outline[s] the kinds of things that are of interest to study for various sources, the argued-for concepts and their interrelationships must ultimately be defined and demonstrated in context in order to have any validity.

Taking these elements collectively, Lester's paper can be viewed as a framework consisting of three guiding principles for researchers to think about the purpose and nature of MER.

1. The goals of MER are to understand fundamental problems concerning the learning and teaching of mathematics and to utilize this understanding to investigate existing products and develop new ones that would potentially advance the quality of mathematics education.
2. To achieve these goals, MER must be theory based, which means studies in MER must be oriented within research frameworks (as defined by Lester).
3. The research framework's argued-for concepts and their interrelationships must be defined and demonstrated in context, which, as entailed by Principle 1, must include mathematical context.

Remaining untreated is the question: what is "mathematical context?" It goes beyond the scope of this reaction paper to address this question, but the position I present here is based on a particular definition of "mathematics" (see Harel, in press), whose implication for instruction I formulate as a fourth principle:

4. The ultimate goal of instruction in mathematics is to help students develop *ways of understanding* and *ways of thinking*[1] that are compatible with those practiced by contemporary mathematicians.[2]

Collectively, these four principles support the argument that theory-based mathematics education research must be rooted in (contemporary) mathematical context. I will discuss this argument in the context of a particular phenomenon—that of "argumentation" versus "proof."

A major effort is underway to change the current mathematics classroom climate by, among other things, promoting argumentation, debate, and persuasive discourse. The effort involves both theory and practice—fundamental understanding and considerations of use, to use the first guiding principle. I have chosen this area because (a) scholars in a broad range of research interests are involved in the effort to understand argumentation and to make it a standard classroom practice at *all* grade levels and (b) on the surface, this research—situated largely in sociocultural, socioconstructivist, and situative theoretic perspectives—does not seem to require a strong mathematics background. There is no doubt this is a worthwhile, and even essential, effort. However, there is a major gap between "argumentation" and "mathematical reasoning" that, if not understood, could lead us to advance mostly argumentation skills and little or no mathematical reasoning**.** Any research framework for a study involving mathematical discourse that adheres to the above four principles would have to explore the fundamental differences between argumentation and mathematical reasoning, and any such exploration will reveal the critical need for deep mathematical knowledge.

In mathematical deduction one must distinguish between *status* and *content* of a proposition (see Duval, 2002). *Status* (e.g., hypothesis, conclusion, etc.), in contrast to *content*, is dependent only on the organization of deduction and organization of knowledge. Hence, the validity of a proposition in mathematics—unlike in any

[1] For special meanings of these two terms, see Harel (in press a, in press b). It is important to highlight that these terms do not imply correct knowledge. In referring to what students know, the terms only indicate the knowledge—correct or erroneous, useful or impractical—currently held by the students. The ultimate goal, however, is for students to develop ways of understanding and ways of thinking compatible with those that have been institutionalized in the mathematics discipline, those the mathematics community at large accepts as correct and useful in solving mathematical and scientific problems. This goal is meaningless without considering the fact that the process of learning necessarily involves the construction of imperfect and even erroneous ways of understanding and deficient, or even faulty, ways of thinking.

[2] This position, I should acknowledge, may not be a consensus among mathematics education scholars (see, for example, Millroy, 1992).

other field—can be determined only by its place in logical value, not by epistemic value (degree of trust). Students mostly focus on content, and experience major difficulties detaching status from content. As a consequence, many propositions in mathematics seem trivial to students because they judge them in terms of epistemic values rather than logical values. For example, a decisive majority of students taking a geometry course for mathematics majors in their senior year had genuine difficulties understanding why it is necessary to substantiate the proposition "For any double cone, there is a plane that intersects it in an ellipse." Their robust perceptual proof scheme (Harel & Sowder, 1989) compelled them to make epistemic value judgments rather than logical value judgments. Similarly, due to attachment to content, students—including undergraduate mathematics majors—experience serious difficulties with proofs by contradiction and contrapositive proofs when they view the conclusion of the proposition to be proven as self-evident. Specifically, when a proposition $a \Rightarrow b$ is to be proven and the students view the statement b as self-evident, they are likely to experience difficulties with proofs that assume not b. Their main difficulty is in separating the content of b from its status.

Another related characteristic of mathematical reasoning, which is particularly significant and a source of difficulty for students, is that in the process of constructing a proof, the status of propositions changes: the conclusion of one deductive step may become a hypothesis of another. These are crucial characteristics that must hold in any form of mathematical discourse, informal as well as formal (!). In argumentation and persuasion outside mathematics, on the other hand, they are not the main concern: the strength of the arguments that are put forward for or against a claim matters much more.

Thus, a solid mathematical background seems necessary for a researcher conducting a teaching experiment or observing a classroom discussion to determine whether "argumentation" or "mathematical reasoning" is being advanced. Furthermore, it is inescapable that a scholar who is interested in social interaction in the mathematics classroom would confront—implicitly or explicitly—critical questions such as: Does mathematical reasoning grow out of argumentative discourse, and if so, how? Are there relationships between argumentation and proof? If so, what are they? How can instruction facilitate the gradual development of the latter from the former? For these questions and their answers to be meaningful, one has to have a deep understanding of mathematics, in general, and of proof, in particular.

The above differences between "argumentation" and "proof" represent vital and unique aspects of mathematical reasoning relative to reasoning in any other field. Despite this, students—even undergraduate mathematics majors in their senior year—have difficulties understanding these aspects. This suggests that graduate programs in mathematics education should pay special attention to the mathematical content component of their course requirements. Of course, adhering to Lester's notion of "research framework," mathematics education researchers must know much more than proof: they must understand, for example, the constructs of "argumentation," "social interaction," and "norms," *and* they must master essential elements of different theoretical perspectives, such as sociocultural, cognitivist, socioconstructivist, and situative theoretic perspectives, in which these constructs reside. Furthermore, dealing with the learning and teaching of proof inevitably leads to questions about the epistemology and history of this concept, for example in differentiating between *didactical obstacles*—difficulties that result from narrow or faulty instruction—and *epistemological obstacles*—difficulties that are inevitable due the meaning of the concept (see Brousseau, 1997). This is why it is critical that graduate mathematics education programs include advanced courses in mathematics as well as courses in cognition, sociology, and philosophy and history of mathematics.

Schoenfeld (2000) expressed a position on the purpose MER that is consistent with that the four-principle framework presented above. Namely, that the main purpose of research in mathematics education is to understand the nature of *mathematical* thinking, teaching, and learning and to use such understanding to improve *mathematics* instruction at all grade levels. A key term in Schoenfeld's statement is *mathematics*: It is the *mathematics, its unique constructs, its history, and its epistemology* that makes *mathematics education* a discipline in its own right.

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