Review

Theories That Gyre and Gimble in the Wabe

Mathematics Education as a Research Domain: A Search for Identity. (1998). Anna Sierpinska and Jeremy Kilpatrick (Eds.). Dordrecht, The Netherlands: Kluwer. xiii + 576 pp. ISBN 0-7923-4599-1 (hb., 2 vols.) $250. ISBN 0-7923-4600-9 (pb., 2 vols.) $99.

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Most mathematicians consider themselves, at least to some degree, to be both educators and researchers. Yet few embrace or even respect the subject at the intersection of these fields: research in mathematics education. Mathematicians rarely apply their own logical acumen to assessment of teaching and learning, nor do they often read the vast and growing literature in educational research. They are conflicted about evaluating the professional work of researchers in mathematics education and often resist pressure to publish (or even review) educational research in mainstream professional journals. Few look to educational research for insight when seeking to improve their own teaching.

Why is this? Many researchers in mathematics education believe their field is the victim of prejudice by a conservative old guard who reject anything that does not fit the paradigm of classical scientific research. Mathematics departments have a long history of resisting new interdisciplinary fields (e.g., statistics, operations research, applied mathematics, computer science). Indeed, according to a recent NSF review committee, mathematicians still disdain connections with outside fields, even when such isolation hurts their self-interest. Research in mathematics education may well be just the latest victim of mathematicians' hubris.

On the other hand, perhaps the phrase "research in mathematics education" is simply a pretension, if not an oxymoron. Is mathematics education even susceptible to research? What might be the aims of such research? What are the objects of study? What are the chief questions? What are the main theories? What are the key results? What are the criteria? What are the important applications? Critics of research in mathematics education point to the paucity of answers to these questions as evidence that what we are dealing with here is more like a political movement than a scientific discipline.

In response to these and related concerns, the International Commission on Mathematical Instruction (ICMI) convened experts in mathematics education research from around the world and asked them to clarify the identity of their discipline. The purpose of this study conference, held in May 1994 at the University of Maryland, was not to describe the state of the art but to identify perspectives, goals,

problems, and research methodologies. The two volumes under review contain reports from this study conference together with several dozen papers expanded from conference presentations.

So what do these international experts tell us about their own field? Pretty much the same thing as the skeptical mathematicians. Indeed, the most striking conclusion of this ICMI study is that in spite of much thoughtful work by individual researchers, there is no agreement among leaders in the field about goals of research, important questions, objects of study, methods of investigation, criteria for evaluation, significant results, major theories, or usefulness of results. The papers in these two volumes—five working group reports, 33 expert papers, and the editors' summary (significantly titled "Continuing the Search")—document a field in disarray, a field whose high hopes for a science of education have been overwhelmed by complexity and drowned in a sea of competing theories.

The title of the volume virtually proclaims a crisis in identity. What other academic field would devote such effort to investigating whether or not it is a research discipline? The standards are set forthrightly by one of the study's working groups: Research is "a craft practiced by scholarly groups whose members have agreed in a broad sense on what procedures are to be followed and on the criteria for acceptable work" (Working Group 2, p. 16). Judging from the papers in this volume, research in mathematics education falls far short of meeting these two standards.

Despite arguments about the relative importance of different subdisciplines, mathematicians for the most part agree on procedures of research and criteria of validity for mathematical results. For theorems, logical proofs are required; for applications and algorithms, demonstrated utility suffices until rigorous proofs can be found; and for theories, broad explanatory power is expected. Scientists operate in a domain in which theoretical models must be tested against worldly events. Yet all scientists agree on the requirements of reproducible procedures and empirical verification.

In contrast, researchers in mathematics education not only disagree on criteria for acceptable work, they even disagree on the need for criteria. Some refuse to adopt any criteria. They propose instead "to define specific approaches and methods for each problem separately" (A. Sierpinska & J. Kilpatrick, p. 540). Some urge adoption of traditional criteria (K. Hart, p. 411), whereas others support a "practical perspective" that recognizes the primacy of idiosyncratic personal views of editors and reviewers (G. Hanna, p. 401). "The study has shown," report the despairing editors (p. 540), "that mathematics educators generally feel uncomfortable with the idea of establishing ... a set of criteria for assessing the quality of mathematics education research."

OK. Perhaps it is too much to expect widespread agreement on the criteria for research in a field as complex and amorphous as mathematics education. But mathematics educators cannot even agree on the nature of mathematics. Although mathematics is at the heart of mathematics education, it turns out that educators' mathematics is not mathematicians' mathematics. Indeed, the question of "what is mathematics" is debated throughout these two volumes almost as if Courant and Robbins (and their many successors) had never answered the question. The consequences for educa-

tion are profound. "If it is unclear as to what is mathematics, what are its main achievements, and what constitutes performance in it, then what hope is there of teaching it in a clear way ... or more effectively?" (R. Brown, p. 459).

To a mathematician, mathematics is singular—a Platonic paradigm in which there are simple, unquestionable criteria for distinguishing right from wrong and true from false. But to mathematics educators, mathematics is plural. Mathematics, among other things, offers a lens through which one can look at the world. In mathematics education the direction is reversed—one looks at mathematics through the lens of learners (and teachers). Thus "a multitude of views on mathematics will result," even a multitude of "personal mathematics" (W. Dörfler, p. 13). In mathematics education, post-modernism has replaced Platonism, contextual interpretation has replaced absolute truth, and utility has replaced correctness as the standard of value (A. Sfard, p. 491). No wonder educators repeatedly cite non-Platonists such as Imre Lakatos, Reuben Hersch, and Thomas Tymoczko as authorities on the nature of mathematics rather than, say, Keith Devlin, Morris Kline, or Ian Stewart, each of whom has written extensively on this topic.

This difference in the conception of mathematics has important consequences for mathematics education. Mathematicians view their mathematics as the real thing, in contrast to other people's versions that mathematicians believe are but imperfect imitations. Nonmathematicians, especially students, harbor nonstandard ideas ("misconceptions") that, "like road accidents," could be avoided through better teaching. In mathematicians' Platonic world view, "idiosyncratic student conceptions" are the result of "failed attempts to convey ... what true mathematics is all about" (A. Sfard, p. 496).

In contrast, mathematics educators' view of mathematics is mediated by the oftentimes strained relation between mathematics and its learners—what one author called the "pragmatics" of mathematics. This relation of mathematics to learner "requires us to broaden the notion of mathematics to encompass mathematical practices that are relevant for society at large but [are practices] other than those of university mathematics" (Working Group 1, p. 11). The inherent incompatibility between this pluralistic post-modern view and the naive Platonism of working mathematicians yields systems of beliefs "as distinct as those which separate incommensurable scientific paradigms ... or different religions" (A. Sfard, p. 505). "Judging one from within the other is absurd."

The endless cycle of fashion in mathematics education—first new math, then back to basics, then problem solving, now constructivism—leads many observers to question whether the subject harbors any enduring principles or convictions. The debate in these volumes does little to mitigate this concern. Dramatically opposing views on almost every topic are more the norm than the exception.

For example, some argue that all mathematical knowledge comes into being "in contexts that are real, concrete, or experimental" (J. Confrey, p. 12), whereas others hold that important parts of mathematics must be learned "not in authentic but in artificial contexts" (G. Vergnaud, p. 12). Although many believe in the scientific basis for research in mathematics education, others argue that such knowledge is

just "a combination of statements of belief and their justification which relate to the [environment] in which the student is operating" (S. Lerman & R. Lins, p. 342). In fact, some are so concerned about the disharmony of the English term "mathematics education" that they prefer using *didactique des matématiques.*

A chief worry of many authors is whether their field is worthy. Is it a true scientific discipline (whose products are theories of learning) or a design science (whose products are curricula, texts, and software)? Believing in the former as the path to both knowledge and stature, many U.S. mathematics educators undertook comparative statistical research that they expected would lead to "grand theories that would enable mathematics education to become a science" (N. Ellerton & M. Clements, p. 157). This strategy, advocated by Ed Begle in 1966 at the First International Congress on Mathematics Education, was based on a view of mathematics education as an experimental science similar to medicine or agriculture. Unfortunately, not long after the challenge was laid down, "doubts were being expressed about the capability of education to become a science at all" (R. Mura, p. 114).

This American infatuation with statistical research has now been moderated by an infusion of observational and inferential methodologies drawn more from anthropology than from agriculture. Children, it turns out, are more like people than like plants. Problems in mathematics education are inherently too complex for simple statistical techniques, requiring instead methodologies from many disciplines including epistemology, psychology, sociology, philosophy, neuroscience, and mathematics itself.

Mathematicians tend to believe that only when mathematics education research produces a "theory of mathematical thinking that convincingly explains observed phenomena" (S. Amitsur, p. 455) will it become a true academic discipline on a par with other sciences that produce robust theories with broad explanatory power (e.g., evolution in biology, thermodynamics in chemistry, relativity in physics). But are such theories likely to emerge in a field that defiantly refuses to accept even its own past accomplishments? "There is no way to resolve [questions about research in mathematics education] once and for all in the way, for example, one might prove a theorem. Each generation must address anew what doing research in mathematics education is all about" (A. Sierpinska & J. Kilpatrick, p. 527). Such sentiments more aptly describe pop culture than a "research domain" of the title quest.

One reason research experts agree on so little is that education is socially and culturally situated. Thus, for instance, the energy that U.S. researchers devote to problem solving is of little interest to Japanese educators because problem solving is already a routine part of their educational practice (Y. Sekiguchi, p. 392), whereas the individualistic focus of the Piagetian psychological literature that has so dominated mathematics education in the West has little in common with Vygotsky's "radically distinct" social constructivism that has influenced Eastern Europe (S. Lerman, p. 347). The international community of mathematics education research is not like that of mathematics or science. In the latter, "research is internationally shared and international interests are overriding," whereas in the former "each country has its domestic educational problems and interests, and they shape its research practice significantly" (Y. Sekiguchi, p. 391).

Several study participants noted that most references in the research literature in mathematics education point to the research school to which the author belongs. This intellectual myopia, they argue, is due not to language barriers or lack of good will but to "true incompatibilities" between different research traditions. "Results obtained by different research schools are very difficult to compare, and researchers prefer to stay within a strictly homogeneous reference system" (P. Boero & J. Szendrei, p. 207). The result is intense intellectual parochialism in which arguments over epistemology or methodology that rage among researchers in one country may be looked on with "amusement, disdain, or incomprehension" by researchers elsewhere (A. Sierpinska & J. Kilpatrick, p. 546).

Not surprisingly, study participants offer widely differing goals for their field, ranging from basic knowledge to practical applications:

- To study. "Research in mathematics education is the intentionally controlled examination of issues within and related to the learning and teaching of mathematics through a process of inquiry that leads to the production of (provisional) knowledge both about the objects of the inquiry and the means of carrying out that inquiry" (G. Hatch & C. Shiu, p. 297).
- To understand. "Developing understanding satisfies both fundamental (theoretic) and practical aims simultaneously. Research efforts that do not aim to understand satisfy neither of these goals and are not worth making" (J. Hiebert, p. 141).
- To invent theories. Basic research questions emerge from a theoretical framework "of the nature and meaning of mathematical objects (concepts, propositions, ...) which takes into account their epistemological and psychological dimensions" (J. Godino & C. Batanero, p. 178).
- To create knowledge. "The goal for research in mathematics education should be to produce new knowledge about the teaching and learning of mathematics" (T. Romberg, p. 379).
- To improve education. "Among the more pragmatic aims would be the improvement of teaching practice as well as of student understanding and performance." (Discussion Document, p. 5).

Opinions about the desired results of research in mathematics education are equally diverse, if not sometimes incompatible. Mathematicians expect verifiable studies that offer the possibility of improving teaching and learning. Some educators argue that change in people—transformation in the "being" of the researchers themselves—is the most important product. "It is their questions that change, their sensitivities that develop, ... their perspectives that alter. In short, it is their being that develops" (J. Mason, p. 357). Others argue for empirically tested teaching units based on "fundamental theoretical principles. ... Such units are the most efficient carriers of innovation and are well suited to bridge the gap between theory and practice" (E. Wittmann, p. 99).

Of course mathematicians, if asked about the desired results of their own field, would also give a number of different answers (e.g., proofs, theorems, applications,

algorithms, concepts, theories). However, most mathematicians would embrace *all* these options as significant and worthwhile results. In contrast, the papers in this ICMI study give the distinct impression of a field Balkanized by conflicting ideologies in which each researcher argues for idiosyncratic definitions and goals. The result is intellectual nihilism rooted in the "continuing and persistent doubt" of not knowing. "Instead of searching desperately for secure ground on which to stand firm, it is possible to accept tension between knowing and not knowing as a productive and inescapable source of energy and security" (J. Mason, p. 359).

Behind the title of this study is a faint hint of worry that the emperor of educational research may have no clothes. Physicists study energy and matter; chemists, molecules; biologists, life; mathematicians, patterns. But what do researchers in mathematics education study? According to the very comprehensive index to these volumes, they study contexts, cultures, and curricula; discourse, language, and meaning; teaching, learning, and curricula; models, paradigms, and philosophies; situations, practices, and theories; knowledge, understanding, and meaning; radical, social, scientific, and trivial constructivism; as well as such esoterica as acculturation, didactic phenomenology, educational ecologies, epistemological empowerment, hermeneutics, intersubjectivity, metonymy, ontology, semantic fields, semiotic models, and situated cognition. (Mathematics, it seems, does not have a corner on the market of abstruse terminology.) Unfortunately, nothing in these volumes suggests that this profusion of vocabulary is sufficient to clothe the emperor.

From this thicket of meaning-challenged words and related disputations about goals, aims, methods, criteria, and results emerges a single irony that seems to enjoy widespread assent: Research has had essentially no impact on the practice of mathematics education. On this broad indictment, researchers and their critics agree—although, as usual, they do not agree on the cause. Some blame incompatibilities between researchers' aims and the reality of mathematics education (P. Boero & J. Szendrei, p. 198) or the widespread tendency to generalize results beyond their field of validity (Working Group 4, p. 27). Others cite the inclination of researchers to talk only to each other. "The lack of relationship between research and practice is well documented" (A. Bishop, p. 35).

Almost everyone seems to recognize a significant divergence of interest between researchers and teachers. "Mathematics teachers tend to expect ... specific prescriptions to solve problems they experience in their classrooms, whereas researchers tend to address problems raised in research communities that are not always directly related to teachers' concerns" (Y. Sekiguchi, p. 391). "Failure is the only possible outcome for any approach ... in which researchers hand their results to curriculum developers and teachers who are then expected to apply them in their practices" (J. Mason, p. 375).

Even those outside the relatively inbred community of mathematics education researchers have picked up the scent of irrelevance. Mathematicians, struggling to cope with the "democratization" of teaching, "do not find in the results of didactic research the means to remedy the problems that, in their opinion, this research

should provide" (M. Artigue, p. 484). University presidents talk about closing their schools of education; colleagues find most researchers' papers uninteresting ("they deal with marginal details," S. Amitsur, p. 448); and teachers cannot synthesize scattered results into useful forms (A. Bishop, p. 42). Even the public now sees the activities of the mathematics education research community to be "at best marginal and at worst subversive in relation to public concerns" (M. Brown, p. 263). Without consensus on goals or criteria, without compelling results or powerful theories, none of the rhetoric in these volumes can hope to resolve researchers' continuing search for identity.

Thirty years ago the wonderland of category theory was in its heyday, promising a "theory of theories" to unify mathematics. Floating in a cloud far above equations, groups, and topologies—the substance and texture of mathematics—category theorists promised powerful tools and unifying theories. Traditionalists, skeptical as always, lampooned the field as "generalized abstract nonsense." Category theory enlisted a lot of machinery in the service of abstractions that were either nonsense or perhaps every sense. No one knew for sure, but many had their suspicions.

Research in mathematics education as reflected in these volumes is much like category theory: a profusion of concepts, relations, and neologisms brought to bear on abstract notions of mathematics education that have none of the texture of real students in real classrooms. Many readers will feel just like Alice: "Somehow it seems to fill my head with ideas—but I don't exactly know what they are!" Whether real educational improvement can emerge from these inchoate theories is far from clear. But many have their suspicions.