# Untying the Mind's Knot

*Barry A. Cipra*

NORTHFIELD, MINNESOTA

> *Listen and learn,*
> *for what I shall now say*
> *will be a gift of lofty consequence.*
>
> —*The Paradiso, Canto VII, 23–24 (Ciardi).*

> *Opinions too soon formed often deflect*
> *man's thinking from the truth into gross error,*
> *in which his pride then binds his intellect.*
>
> —*Canto XIII, 118–120.*

Not unlike the poet Dante, undergraduate mathematics has lost its way in a dark woods. Many who set out as teachers find themselves wandering aimlessly through the groves of academe, waiting for a Virgil or a Euclid to guide them to the light. Some can't see the forest for the trees. Some seek salvation in the roots, others in the canopy. Some see nothing but danger lurking, and a steep, accelerating descent into the nested circles of Hell and Damnation.

Among the many voices in the wilderness are some crying "Research, we need more research!"

Others retort, "Like hell we do!"

What's all the shouting about? Is Research to be our Beatrice—or is the call for careful study another Siren's song, sweetly luring scholars to their doom? The answer depends on who you ask.

This report will examine some of the issues and controversies surrounding research in collegiate-level mathematics education. It is based on interviews with a number of mathematicians and mathematics educators who hold a variety of opinions on the subject. The purpose here is not to survey or update the latest studies, but rather to bring into focus the central concerns of those working in the area and of those who wonder if it's worthwhile.

> *You will certainly come to know your view*
> *is steeped in falsehood if you listen well*
> *to the counter-arguments I shall offer you.*
>
> —*Canto II, 61–63.*

## A Diversity of Definitions

To start with a seemingly innocuous question: What is research in collegiate-level mathematics education? That gets quickly to at least one source of the cacophony of opinions, because there does not seem to be any general agreement on the meaning of the term.

The spectrum of interpretations as to what such research is, not to mention what it's good for, makes the fable of the blind men and the elephant look like a model of consensus and agreement.

"The term means different things to different members of different communities," says Jim Leitzel, a professor of mathematics at Ohio State University currently at the MAA headquarters in Washington, DC. The diversity of definitions makes dialogue difficult.

One way to view such research is as pure inquiry into the processes of human understanding: The learning of mathematics poses numerous fundamental questions in cognitive science; the job of the mathematics education researcher is to follow those questions wherever they may lead. If the journey happens to bring something useful back to the classroom, then so much the better. But that's not necessarily the reason for doing the research.

That point of view should be familiar to mathematicians, who smile knowingly at G.H. Hardy's peculiar boast, "I have never done anything 'useful.'" But when it comes to educational research, mathematicians tend to think in terms of their own needs. They become the client, with a client's demands for service. From that point of view, mathematicians see educational research in purely practical terms.

"For the most part the mathematical community, when it comes to research on mathematics education, is a user community. That is, the generic mathematician says, no different from the high school teacher, tell me what I can do in my classroom on Monday," says Alan Schoenfeld of the University of California at Berkeley. "It is appropriate for mathematicians to ask that of educators, but it is not appropriate for that to be what defines mathematics education research."

Schoenfeld points to three broad categories of research. The first he calls social and epistemological "engineering." This work is based on the view of mathematics as a human activity—a view mathematicians are likely to stick with even after computers have taken over completely. The operative pedagogical assumption is that students develop this view of mathematics only if they experience it by engaging in the "give-and-take of mathematical sense-making," rather than the customary note-taking approach in which the student copies a neatly packaged version of the subject. What goes on in this type of setting and how to make it work are questions of interest to a number of researchers.

The second category Schoenfeld calls "product-oriented" work. This work is closely related to curriculum development and includes many such efforts, but it differs by placing an emphasis on foundational aspects and research. The difference could be likened to the difference between an applied mathematician who's interested in the general conditions under which a particular numerical technique will produce an approximate solution to a differential equation and an engineer who just wants the damn thing solved.

Schoenfeld's third category is "basic" research into cognition. Like basic research in any subject, this work often has no obvious or immediate applicability, but presumably will exert a long-term influence. Much of Schoenfeld's own work falls into this category. In one project, for example, Schoenfeld and colleagues analyzed seven hours of videotape showing a single student interacting with a tutor and a computer program. "It goes without saying that I see this work as being deeply connected to some of my more 'applied' mathematical work," says Schoenfeld, "but I must stress that on its own terms this is basic work pursued independent of potential applications: our huge investment in analyzing seven hours of videotape was aimed at yielding a better understanding of mathematical thinking and learning processes."

Some feel the time is ripe for the development of theories of understanding. Others are not convinced. "There's a continuing distrust, or skepticism, about any *theoretical* explanation of how the brain processes information concerning mathematical concepts," says Ramesh Gangolli of the University of Washington. On the other hand, Gangolli notes, there's a growing willingness on the part of mathematicians to take a chance trying out some of the educational practices suggested by results from cognitive science. "I think there are some encouraging signs that professional mathematicians are now coming to regard some of the results of cognitive work as useful," he says.

Schoenfeld acknowledges that "research methods are not well established at this point." However, he says, the situation is much better than it was. "About 10 or 15 years ago, the field finally came to the realization that the methods that were pretty much standard were essentially worthless," he says. Those methods were "scientistic" misapplications of statistical methods such as comparison studies and factor analyses which, while appropriate for agricultural studies, were out of place in the classroom. "The problem is that human behaviors—and humans generally speaking—are just a lot more complex than fields of corn."

The field has gotten away from that, Schoenfeld says. Instead, "we started asking: Can we build some models of thought processes? Can we get a handle on what those things actually are and talk about the way that cognitive structures grow and change? That's a jargon way of saying, can we really describe learning in a precise way?" The change in thinking has left the field "in pretty much an exploratory phase." Nevertheless, Schoenfeld maintains, "I think we've come close to identifying the salient aspects of mathematical thinking. We at least know what it isn't, which is the reductive view that everybody had of it 15 or 20 years ago, that the mathematics you know is the sum of the mathematical facts and procedures you know. We have a much richer view [now]."

*I approve what has emerged thus far, but now*
*it is time you should explain what you believe,*
*and from what source it comes to you, and how.*

*—Canto XXIV, 121–123.*

## 500 Papers

Traditionally, mathematics education has concerned itself with children before they get to college, and especially with youngsters in the elementary grades. Researchers look into how children learn to count, what happens—or goes wrong—when they add and subtract, how various societal problems affect achievement in mathematics, and a vast number of other questions.

Research along the same lines at the collegiate level is less common and has a much lower profile. Most members of the mathematics community, even when they have an abiding interest in educational issues, are unfamiliar with the research literature. To the extent that they've heard rumors of it, many are suspicious.

The amount of research in collegiate-level mathematics education is relatively small by modern standards. A dedicated scholar (or masochist) could conceivably read the entire extant literature, provided he or she had access to the various sources. Joanne Rossi Becker and Barbara Pence at San Jose State University surveyed the most prominent sources for the years 1975–1989, and compiled a list of approximately 500 published manuscripts. (They chose 1975 as a starting date because there was already a bibliography, compiled by Marilyn N. Suydam, for 1900–1974.) Becker and Pence also identified over 750 dissertations involving college-age populations in the same period.

Becker and Pence classify research under the general headings of student learning, methodology, equity issues, academic preparation for college, and teacher education. There is necessarily overlap among and between them. Student learning includes research into learning styles, problem solving, concept formation and misconceptions, and specifically into the learning of algebra and calculus and the impact of computer technology. Under methodology, they cite studies of effective teaching, the use of tests and assignments, and experiments with cooperative learning.

"Equity issues" includes both gender and minority issues. It also includes studies of "affective variables"—e.g., "math anxiety" studies. Research on college preparation—much of which is now done by the colleges themselves—focuses on predictors of success (such as the use of placement exams) and the development of remedial courses. Teacher education refers to research on how well teacher education programs do at preparing pre- and in-service teachers, an issue of increasing importance in light of the "sweeping changes" called for by such documents as the NCTM *Standards* and the MAA *A Call for Change*.

Compared to the thousands of papers published every year in traditional research areas of both mathematics and mathematics education, the several hundred papers identified by Becker and Pence constitute a manageable amount of material. There are signs, though, that this will change. The growing educational reform movement—itself still a fairly young enterprise in spite of its long history—has led many teachers to reconsider the basic tenets of their craft, and some are inclined to do so in systematic fashion.

"Things are very different from the way they were three or four years ago," says Ed Dubinsky at Purdue University. "There are a number of people who are now actively involved in research in mathematics education at the post-secondary level."

Dubinsky is part of that crowd. "I've been doing research for about six or seven years, maybe a little longer if you count the time that I took when I started reading other people's stuff." He began with a study of Piaget, the Swiss psychologist whose studies of children have had a profound influence on educational theory. "I've tried to make myself something of a specialist in the ideas that Piaget had about how people learn, and to see if those ideas can be extended to the post-secondary level," Dubinsky explains. He has also spearheaded the effort to establish educational research as a legitimate professional activity for members of the mathematics community.

Research in mathematics education, Dubinsky asserts, has an appropriate place in the world of mathematics. "In this community of 45,000 people, there is a significant number who are hired by mathematics departments for their interest in education," he says. Those numbers indicate "a pent-up potential for sharp growth" in the area of educational research, he adds.

It's an uphill battle, though. Education researchers are faced with a mathematics community that is generally skeptical, occasionally hostile, and usually uninterested. There are lingering doubts about the quality of research in mathematics education. There is a communication gap separating researchers whose native tongue is educationese from a potential audience schooled in the language of mathematics—a gap that some fear, and others hope, is unbridgeable. And, as always, there is the ogre of priorities: Are the results of research in mathematics education worth an investment of resources?

*Thus [Beatrice] began: "You dull your own perceptions*
*with false imaginings and do not grasp*
*what would be clear but for your preconceptions."*

—*Canto I, 88–90.*

## Quality Time

By far the biggest bone of contention in the whole discussion of research in collegiate-level mathematics education is over the question of quality: Can the educational research establishment convince the mathematics community that its standards are up to snuff? There are some for whom the answer is a resounding "No." Others are willing to take a wait-and-see approach. Still others defend the standards of the discipline as entirely adequate as is, albeit inadequately enforced.

The dispute stems partly from fundamental differences between mathematical and educational research. The nature of the work is different, and the standards for one do not apply to the other. In mathematics, while research is ultimately judged on its significance and depth, there is a first filter of logical (as opposed to political) correctness: Mathematicians prove theorems, and a proof is either valid or it isn't. (This glosses over many shades of gray, of course—or should we say "obviously"? For example, what one person sees as a full-blown proof others may read as the skimpiest of sketches. Also, mathematicians are amazingly forgiving of faulty proofs or even false theorems if the work has been led astray by deep new insights.) There's no corresponding abstract canon of rigor in educational research.

Instead, the criteria for quality in educational research have a more social cast to them. In particular, new research is judged in relation to previous studies—new work is expected to build on what others have found. (That's not to say it can't contradict earlier findings. It just shouldn't add apples to oranges.) One of the main criteria is the presence of a theoretical framework. Another is the presence of an established methodology.

These are admittedly fuzzy criteria, calling for considerable judgment on the part of people in the field. It's a paradigm that many mathematicians are clearly uncomfortable with, and some reject outright. Herb Wilf, for one. As editor of the *American Mathematical Monthly* from 1987 to 1991, Wilf, a professor of mathematics at the University of Pennsylvania, has been involved in discussions of whether or not the MAA should sponsor a journal devoted to educational research (of which more later). Of one batch of papers which were submitted as "good examples of the craft," Wilf says, "My own personal opinion was that very few of them—perhaps one or two—met normal standards for research that have been around in the Western world since the time of Newton."

"Most of the papers—almost all of them–were anecdotal in nature," Wilf goes on to say. That's not necessarily bad, it's just not research, he explains. "Personally I value anecdotal stuff. If an intelligent person tells you something that works and something that didn't work, then you're smarter than you used to be. So that's not so bad. But for some reason it wants to dress itself up as research in undergraduate education." In sum, he says, "I saw some stuff that definitely is in the category of good, intelligent, hardworking people communicating their experiences, and I saw almost nothing of what I would call research—carefully designed experiments with carefully regulated class sizes and comparisons of results, and so on. I saw almost none of that."

Joan Ferrini-Mundy of the University of New Hampshire agrees that many studies submitted as research are "hopelessly anecdotal," but disagrees on the criteria that distinguish valid research. "You look for hypotheses, for example, that are coming out of other work," she explains. "You look for some connection to something else, rather than just someone who decided they'd take a close look at something without worrying about what other people have already seen. You look for some kind of theoretical framework that's going to guide the questions, so that when they get in and start to look around they have something of a framework in which to interpret what they're seeing. And you look for systematic study of whatever it is that's being questioned—some kind of methodology that undergirds what's being done."

Those criteria can be met even when what's being looked at are interviews with students or examples of student writing or "some kind of data that feels anecdotal," Mundy says. Nevertheless, "it's a pretty fine line. It rests a lot, I think, on how much people are willing to trust the insights of the people doing the work."

What clouds the issue, everyone admits, is the presence of a great deal of research that's of low quality by anyone's standards. "The general feeling among mathematicians that there are a large number of papers that are of really low quality is a valid concern," says Dubinsky. Adds Schoenfeld: "The prevailing attitude is still one of cynicism, and I'd say a fair percentage of it is justified." But it's a bad idea, they argue, to throw out the baby with the bath water.

"It's very similar to an experience I had in the early 1960s, when I was starting out and did my research in general linear topological spaces, in which there was a similar high percentage of low-quality papers put out," says Dubinsky. "It was the sort of stuff that you could take a concept and write down the definition and find another context in which that definition was logically consistent, and go ahead and prove the same old theorems using the same old examples and counterexamples in a new context, and churn out the papers." As a result, Dubinsky recalls, other mathematicians developed a low opinion of the field. But "the appropriate response was not to throw the field out. The appropriate response was for some good people to do some really good stuff, and set the standards, and for people to develop the attitude that we're not going to treat as high-quality work things that are not high quality, but that there are things that are high quality, and we're going to encourage that."

The same holds for educational research, Dubinsky maintains. "The appropriate response is not to say there's too much junk in mathematics education research so let's just ignore the whole thing. I think the appropriate response is to recognize that there is a fair amount of very good stuff, and stuff that could be very useful—first of all, research of a high quality with its intellectual value as such, and in addition things that could be very useful to the practicing college teacher—and to try to find ways to develop standards that help us distinguish between those and to find ways to encourage the good stuff and discourage the weak stuff."

*Then one of them came forward and spoke thus:*
*"We are ready, all of us, and await your pleasure*
*that you may take from us what makes you joyous."*

*—Canto VIII, 31–33.*

## Anybody Here Speak Jargon?

Would it help to have a journal devoted to research in collegiate-level mathematics education? Dubinsky and others argue that it would. Such a journal would help set standards for work in the field, and it would help focus attention on some of the issues in undergraduate mathematics education. Moreover, its existence would attract more researchers to work in the area and help establish educational research as a viable part of the world of mathematics.

But mathematicians are not only skeptical of the standards for quality in educational research, they are also put off by what they see as impenetrable jargon in the literature. How do you expect us to make use of this stuff, they ask, when we can't even make sense of it?

Researchers respond with a mixture of agreement and rebuke. On the one hand, they agree that there's more than enough jargon in their trade and that reading educational research papers is not a particularly pleasant job. "There is nothing more boring than reading good educational research," observes Gila Hanna of the University of Toronto. Hanna should know: she edits the journal *Educational Studies in Mathematics*, which rejects, she says, roughly 80 percent of the papers submitted.

On the other hand, educational researchers point out that they are not necessarily writing for an audience of mathematicians. Every field develops a technical terminology that's often opaque to outsiders. Mathematics certainly has an argot of its own. A mathematician decrying the jargon of educational research is analogous to an engineer complaining how hard it is to find useful information in the *Journal of Differential Equations*.

(One can only speculate on what a mathematician would find readable in a paper on educational research. Perhaps something like the following: Let $L$ be a lecture given to a class $C$. Let $s \in C$ be a student, and let $U_L(s)$ be student $s$'s understanding of lecture $L$. We will show that for almost all $s \in C$, $U_L(s) = 0\dots$.)

Nevertheless, it is incumbent on educational researchers to communicate with mathematicians. "There are two things going on," says Schoenfeld. "On the one hand, mathematics educators need to defend the work that's research *qua* research on its merits. On the other hand, mathematics educators need to take seriously the fact that there is a user community out there, that mathematicians will look for things to be stated in ways that are informative and useful for them. If there's a research finding that might be turned into something useful in curricular terms, the researcher can't just say, 'Here's the finding, mathematics community. Go ahead and do it.'"

At the same time, educational researchers must be careful not to oversell themselves—and mathematicians need to understand what research can and cannot provide. "Nobody is going to come across in mathematics education and say all of a sudden, 'We have a teaching method that's going to have students suddenly learn twice as much,'" notes Schoenfeld. "Actually many people do say that—and they're all charlatans."

"You don't do a single research study and say, 'Well, we've finally solved this problem, now we know the answer,'" says Ferrini-Mundy. "The best you'll get is something that confirms or extends somebody else's study. What you have are building blocks that come together after a while to point you in a direction." The process is not quick. "There's a strong expectation that educational research should give you answers that you can somehow use tomorrow," Ferrini-Mundy notes. "That expects too much of it."

For that matter, even when there are clear implications for classroom practice, it's far from clear that mathematicians will pay attention to the results of research. That's not just due to lingering skepticism. Nor is it merely a matter of making the results readable and available. The final, potentially insurmountable obstacle is simply inertia—or, to use a less obviously pejorative term, tradition. The recommendations of research could well fall on deaf ears. If educational researchers have discovered anything in observing classroom lectures, it's that telling people what to do, no matter how clearly you spell out the algorithm, is not a particularly effective way of getting the message across. On top of which, teachers are going to ask the same question they so dislike hearing from their students: Is this going to be on the test? How's it going to affect my tenure and promotion, how's it going to affect my status in the profession?

If that's the case, then should educational research be a priority of the mathematics community? Not if it detracts from the main effort, which is to get mathematics departments to take teaching seriously, argues Peter Hilton of the State University of New York at Binghamton. It would be "utterly foolhardy" to tell them to do so by concerning themselves with educational research, Hilton says. "We have to concentrate on the difficult problems, and not turn to the easy ones," he adds.

Hilton and others argue that top priority should be given to creative teaching and innovative curriculum development. Educational researchers don't disagree on the goal, but counter that the effort will best be served if it is well informed by the findings of sound research. Indeed, encouraging innovation in the absence of research smacks of irresponsibility: What if the pharmaceutical industry abandoned research in favor of innovative drug design?

*I see now that your mind, thought upon thought,*
*is all entangled, and that it awaits*
*most eagerly the untying of the knot.*

—*Canto VII, 52–54.*

## Second-hand Smoke

Over the long run, the main contribution of educational research may be to change the way mathematicians think about teaching—in a sense, to substitute one set of preconceptions for another. If the research is carefully done, and if it's properly presented, then presumably the new preconceptions will be closer to correct than those they displace. For example, some current research indicates that some of the difficulties students have with calculus are traceable not to poor algebra skills, but rather to conceptual difficulties dealing with functions. It could be a noteworthy advance in the discipline if professors thought in those terms when they prepared their lectures. (It could be an even bigger advance if they dispensed with lecturing altogether, but let's not go into that.)

Schoenfeld likens this kind of effect to the effect that medical research has had on the health consciousness of the American public. The research itself is highly technical, and there's constant controversy, but over the course of time the more clear-cut, consistent findings have managed to sink in. Most notably, for example, the evidence linking tobacco with a whole slew of health problems has helped initiate changes in public attitudes toward smoking, to the point that smokers' "rights" are in serious jeopardy.

If the scientific crusade against smoking is an appropriate analogy, then educational researchers and reformers can expect a long, hard battle. The sheer weight of evidence won't tip any scales, but careful research can serve to inform the educational reform movement and help shape its future. The main benefit of research, say Hanna, is "the feeling that you operate from a position of knowledge rather than ignorance."

*Open your mind to what I shall explain,*
*then close around it, for it is no learning*
*to understand what one does not retain.*

*—Canto V, 40–42.*

## Appendix A: Communicating Among Communities

A REPORT OF A CONFERENCE ON
**Research in Collegiate Mathematical Education**

The Mathematical Association of America (MAA), with support from the National Science Foundation, hosted an invitational conference on Research in Collegiate Mathematical Education in Washington, DC, November 8-10, 1991. Twenty-eight invited participants, representing the mathematics and mathematics education communities, candidly discussed issues relating to the growing interest among faculty and others concerning research in the teaching and learning of mathematics at the undergraduate level. The conference participants focused on four aspects of research in collegiate mathematics education:

- Communicating to college and university faculty the growing body of research in undergraduate mathematics education.
- Improving student learning by stimulating change in collegiate teaching based on the findings of this research.
- Encouraging high standards of research in undergraduate mathematics education.
- Supporting the increasing number of collegiate faculty who undertake research in undergraduate mathematics education.

It is no surprise that the views of those attending the conference varied considerably on these matters. There was general agreement on the urgency of seeking improvement in the teaching and learning of mathematics at the undergraduate level. A distinction was made

This Conference was supported by the National Science Foundation. Opinions expressed in this report are those of the authors, not necessarily those of the Foundation.