Mathematics Education Research: A Guide for the Research Mathematician. By Curtis McKnight, Andy Magid, Teri J. Murphy, and Michelynn McKnight. American Mathematical Society, Providence, 2000, xi + 106 pages, $20.

Reviewed by **George E. Andrews**

This book describes the methods of mathematics education research, with the aim of increasing respect among research mathematicians for educational research. This goal is plainly and poignantly summarized in one sentence in the final paragraph:

> ...we hope that [research mathematicians] have an appreciation that mathematics education research is a serious field of scholarship and that research mathematicians and mathematics education researchers can enjoy mutual respect as fellow scientists.

In the first two chapters, we discover some of the problems that have interfered with a full appreciation of mathematics education research. On page 3, we are told that conflicts over "reform" have produced a discussion that

> ...has been healthy for the mathematical community, and has contributed positively in terms of a renewed focus on the importance to mathematicians of improving the quality of undergraduate mathematics instruction. Nonetheless, it may also have had the unfortunate side effect of identifying mathematics education research, in the perception of many research mathematicians, with advocacy for a reform agenda.

On page 15, the authors further explain how not only antipathy toward "reform" but also provincialism among research mathematicians makes it more difficult to understand mathematics education research:

> Some readers start with an underlying assumption that all mathematics education research is "soft" and does not provide for objective evaluation. If one begins with that assumption, one can easily find bases for declaring such results invalid. Certainly this may come easier for those used to proof-based deductive conclusions rather than evidence-based (more inductive) conclusions. Evidence-based conclusions should flow from empirical investigations in which "the preponderance of the evidence" supports the conclusion or in which any reasonably objective and unbiased evaluator of the evidence provided finds their doubts allayed. Careful specification of methods, results, and interpretations can aid the rational evaluation of the evidence and the conclusions supported by it. Nothing can aid a fair evaluation of evidence by a person with an irrational bias or with an unwillingness to admit that there are other bases for drawing conclusions than the deductive conclusions of mathematics.

While research mathematicians apparently suffer from their narrow-mindedness, we find on page 7 that mathematics education researchers are much less constrained:

> Mathematics education researchers need not be disinterested observers. And just because the researchers have an agenda doesn't mean that the evidence produced by their studies is unusable by others not sharing their orientation. Typically such authors are not shy about disclosing how they hope their studies help advance their cause, of course. Similarly, studies that were supported by publishers or technology manufacturers, and that use their products, need not be dicarded by readers who prefer other brands or equipment. Moreover, potential conflicts of interest of this latter type are almost always prominently disclosed. *Tendentiousness of both these sorts is a standard part of the human sciences enterprise, and usually doesn't have any effect on the data collected or its analysis.* [Emphasis added.]

This, to my mind, is an incredible paragraph. Can anyone believe that "Tendentiousness ... doesn't have any effect on the data collected or its analysis"? When we visit the Texas Instruments web site, we find study after study extolling the virtues of calculators in the classroom. What a surprise! It is not necessary to accuse anyone of intentional dishonesty to note that human beings often see what they want to see. This is why so much effort in laboratory science is devoted to promoting disinterestedness.

The authors wish to distinguish mathematics education research from *mere* discussion:

> Not all mathematics education discussions are research... The aim is more descriptive or analytical rather than aimed at confirming hunches and conjectures.
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> There is, of course, nothing wrong with this decriptive discussion. Most sciences move from careful description of common phenomena in the science to findings supported by evidence and, eventually, to theories based on careful descriptions and supported findings. It would be an impoverished discipline that only allowed fully formed, conclusive results into conferences and even into some journals.

The implication here is that descriptive discussion must yield eventually to the loftier achievements of mathematics education research. My theme, put succinctly, is that mathematics education research as described in this book is, in fact, inferior to descriptive discussion because it *is* descriptive discussion without humility. In other words, I believe that no sensible study of teaching will involve "confirming hunches and conjectures", aim at "fully formed, conclusive results", or try to pass itself off as a science.

Not only is the enterprise "soft", but it is unavoidably soft because it completely misunderstands what goes on in teaching. Research in mathematics education will almost always produce underwhelming results because it confuses convergent and divergent problems, an issue I have raised previously [**1**]. To explain convergent and divergent problems, I can do no better than to quote E. F. Schumacher from his *A Guide for the Perplexed* [**5**, pp. 121–123]. Schumacher begins by noting that some problems are solved and some are unsolved. He asks:

> ... are there problems that are not merely unsolved but insoluble?
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> First, let us look at solved problems. Take a design problem—say, how to make a two-wheeled, manpowered means of transportation. Various solutions are offered, which gradually and increasingly converge until, finally, a design emerges which is simply "the answer"—a bicycle, an answer that turns out to be amazingly stable in time. Why is this answer so stable? Simply because it complies with the laws of the Universe—laws at the level of inanimate nature. [Schumacher proposes to call problems of this nature *convergent problems.*]
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> The more you study them, the more—whoever you are—the answers converge. They may be classified into "convergent problems solved" and "convergent problems as yet unsolved". The words "as yet" are important, for there is no reason, in principle, why they should not be solved some day. Everything takes time, and there simply has not yet been enough time to get around to solving them. What is needed is more time, more money for research and development and maybe, more talent.
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> It also happens, however, that a number of highly able people set out to study a problem and come up with answers that contradict one another. They do not converge. On the contrary, the more they are clarified and logically developed, the more they *diverge*, until some of them appear to be the exact opposites of the others. For example, life presents us with a very big problem—not the technical problem of two-wheeled transport, but the human problem of how to educate out children. ... we ask a number of equally intelligent people to advise us. Some of them, on the basis of a very clear intuition, tell us this: Education is the process by which existing culture is passed on to the next generation. Those who have (or are supposed to have)

knowledge and experience *teach*, and those who as yet lack knowledge and experience *learn*. This is quite clear, and implies that there must be a situation of authority and discipline ...

Now, another group of our advisers, having gone into the problem with the utmost care, says this: Education is nothing more or less than the provision of a *facility*. The educator is like a good gardener, who is concerned to make available good, fertile soil in which a young plant can grow strong roots and then extract the nutrients it requires. The young plant will develop in accordance with its own laws of being, which are far more subtle than any human being can fathom, and will develop best when it has the greatest possible freedom to choose exactly the nutrient it needs.

Education, in other words, as seen by this second group, calls for the establishment not of discipline and obedience, but of freedom—the greatest possible freedom...

Freedom vs. discipline and obedience—here is a perfect pair of opposites. No compromise is possible. It is either one or the other, in any real situation. It is either "Do as you like" or "Do as I tell you"...

"What is the best method of education?" in short presents a divergent problem *par excellence*. The answers tend to diverge; the more logical and consistent they are, the greater is the divergence. There is "freedom" versus "discipline and obedience". There is no solution—and yet, some educators are better than others. How do they do it? One way to find out is to ask them.

Schumacher continues with a discussion of "higher faculties" necessary to reconcile freedom with discipline and obedience. His list includes "love, empathy, participation mystique, understanding, and compassion". To obtain a real feeling for these higher faculties in action, grappling with the divergent problem of elementary mathematics education, let us listen to a mathematics teacher in China who has been quoted by Liping Ma [**3**, pp. 135–136]:

What is it that I am going to teach in this lesson? How should I introduce the topic? What concepts and skills have the students learned that I should draw on? Is it a key piece of knowledge on which other pieces of knowldege will build, or is it built on other knowldege? If it is a key piece of knowledge, how can I teach it so students can grasp it solidly enough to support their later learning? If it is not a key piece, what is the concept or the procedure it is built on? How am I going to pull out that knowledge and make sure my students are aware of it and the relation between the old knowledge and the new topic? What kind of review will my students need? How should I present the topic step-by-step? How will students respond after I raise a certain question? Where should I explain it at length, and where should I leave it to students to learn it by themselves? What are the topics that the students will learn which are built directly or indirectly on this topic? How can my lesson set a basis for their learning of the next topic, and for related topics that they will learn in the future? What do I expect the advanced students to learn from the lesson? What do I expect the slow students to learn? How can I reach these goals? Etc. In a word, one thing is to study whom you are teaching, the other thing is to study the knowledge you are teaching. If you can interweave the two things together nicely, you will succeed. We think about these two things over and over in studying teaching materials. Believe me, it seems simple when I talk about it, but when you really do it, it is very complicated, subtle, and it takes a lot of time. It is easy to be an elementary school teacher, but it is difficult to be a good elementary school teacher.

From the Schumacher perspective, the project propounded by the book under review is hopeless from the beginning. It imposes a statistical, scientific paradigm on the study of education when this is completely inappropriate. Consequently, Part I of the book, "Quantitative Research", is egregiously out of place in the discussion of divergent problems. Quantitative research must reduce all its observations to numbers, and the gigantic loss of information required by this reduction should be enough to reveal the inadequacy of this approach.

Part II, “Qualitiative Research”, is more in line with what might be useful, but it also has problems. The authors are anxious to distinguish qualitative research from mere descriptive discussion. However, reading Part II gives the distinct impression that “qualitative research” is merely “good descriptive discussion”. The question is: Why should we call this science? Obviously, qualitative researchers themselves are concerned about this question because by the time we get to the end of Part II we are being supplied with several ways to reduce qualitative research to numbers.

To their credit, the authors make no grandiose claims for mathematics education research. Far from it. The book is filled with warnings about problems that undermine conclusions drawn from research. For example:

> Every piece of education research is done with specific questions, variables, contexts for the phenomena examined, data gathering methods, and analysis procedures. Conclusions are drawn in the context of these specifics of the research. One common danger in drawing and stating conclusions in mathematics education research is that findings and conclusions may be stated as applying more generally than the specific situation of the research. Certainly such over-generalizations cannot be considered as evidence-based conclusions.

This theme is stressed over and over. It is not surprising that this is necessary. If we impose scientific methods that are eminently germane to convergent problems but not to divergent problems, we are sure to produce unsatisfying, uncertain conclusions. It is human nature to talk oneself into believing that there must be grander implications in there somewhere.

If one takes Schumacher’s dichotomy at all seriously, then it follows that teaching must be an art and not a science. In this regard, we must distinguish between teaching and the subject matter to be taught. It is clearly essential that teachers must master their subject matter, a project that is appropriately initially addressed in a college education and powerfully brought home in Liping Ma’s discussion of the profound understanding of fundamental mathematics [**3**, p. 124]. However, teaching itself is a skill, and indeed, Michael Polanyi’s analysis of skills [**4**, p. 53] in *Personal Knowledge* is quite relevant to teaching viewed as a divergent problem:

> An art which cannot be specified in detail cannot be transmitted by prescription since no prescription of it exists. It can be passed on only by example from master to apprentice...
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> It is pathetic to watch the endless efforts—equipped with microscopy and chemistry, with mathematics and electronics—to reproduce a single violin of the kind the half-literate Stradivarius turned out as a matter of routine more than 200 years ago.
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> To learn by example is to submit to authority. You follow your master because you trust his manner of doing things even when you cannot analyse and account in detail for its effectiveness. By watching the master and emulating his efforts in the presence of his example, the apprentice unconsciously picks up the rules of the art including those which are not explicitly known to the master himself.

It is quite correct to say that, for many years, mathematicians concentrated on research and paid inadequate attention to the art of teaching. As the authors of this book note, the recent conflicts in mathematics education have contributed positively to a “renewed focus on the importance to mathematicians of improving the quality of undergraduate mathematics instruction”. However, if you are seriously interested in improving your own teaching, you will do much better by studying Steve Krantz’s [**2**] or Liping Ma’s [**3**] descriptive discussions of skills than you will by poring through the certified “scientific” studies envisioned by *Mathematics Education Research*.

REFERENCES

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3. L. Ma, *Knowing and Teaching Elementary Mathematics*, Lawrence Erlbaum Associates, Mahwah, N.J., 1999.
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Pennsylvania State University, University Park, PA 16802
andrews@math.psu.edu

Roots of Software

Can anyone remember when "software" wasn't on the tongue of every schoolchild? But it had to start somewhere. And Fred R. Shapiro, a librarian and etymologist at Yale Law School, thinks he's gotten to the source.

Shapiro sifted through the billions of words in JSTOR, an electronic journal archive, looking for the earliest use of "software". Lo and behold, he found first mention in a January 1958 article by Princeton statistician John Tukey, co-inventor of the fast Fourier transform, a mathematical technique. In the *American Mathematical Monthly*, Tukey wrote: "Today the 'software' comprising the carefully planned interpretive routines, compilers, and other aspects of automative programming are at least as important to the modern electronic calculator as its 'hardware' of tubes, transistor, wires, tapes, and the like."

Shapiro, who reported the finding in the April-June *IEEE Annals of the History of Computing*, thinks JSTOR will revolutionize the study of scientific quotations and terminology. Mark Twain's *Autobiography*, for instance, which was published in 1924, is typically listed as the source for the quote "lies, damned lies, and statistics". But a JSTOR search reveals the saying in an 1896 statistics journal—in which an even earlier source is quoted. Shapiro predicts that JSTOR will be the source of new revelations as it adds older scientific journals, such as periodicals of the Royal Society of London going back to the 1600s.

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Editor's Note: A search for "damned lies" through all volumes of all 137 journals archived by JSTOR took about 25 seconds and returned 39 hits. The earliest instance was in the article Parliamentary Representation in England illustrated by the Elections of 1892 and 1895, *Journal of the Royal Statistical Society* 59 (1896). On page 87 the author attributes the following quotation to a Mr. Courtney: "We may quote to one another with a chuckle the words of the Wise Statesman, lies, damned lies, and statistics, still there are some easy figures which the simplest must understand and the astutest cannot wriggle out of."

A similar complete JSTOR search for "software" returned 12,775 hits, of which the oldest was indeed in John Tukey's article The Teaching of Concrete Mathematics, MONTHLY 65 (1958) 1–9; see p. 2.