Twenty Questions about

Research on Undergraduate Mathematics Education

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"A pump, not a filter" is the banner of reform in undergraduate mathematics. Almost everyone agrees that the current system is not working as well as it might. Some pipes in our educational system are virtually clogged, others leak badly, while many are disconnected from their destination. For far too many able students, undergraduate mathematics fails to inspire, to motivate, or to lead. Put simply, our educational system often fails to educate.

In many arenas of human endeavor—e.g., engineering, medicine, technology—the typical approach to a challenge of this sort is an intensive R&D effort. Basic research, leading to applications, yields necessary insight for the design of new systems that respond to the challenge. Mathematicians know this paradigm well, since our discipline is the language in which most R&D takes place. But when confronted with problems of our own making—of undergraduate mathematics education—mathematicians rarely recognize research as a valuable or natural part of the response.

Many claims are made to explain this anomaly—claims that appear to some as sound reasons, to others only as lame excuses. These conflicting claims must be addressed by both advocates and critics if research in undergraduate mathematics education is to achieve either legitimacy or utility. Some of the issues reverberate around mathematics departments either as rhetorical questions or as skeptics' challenges. These questions define the challenge and frame the debate.

1. What are the goals of educational research? What, indeed, are the goals of education? Is the purpose of educational research to understand education or to improve it? This is, of course, the fundamental dichotomy between basic and applied research. The answer, perhaps, is "both." But then one might ask a more difficult question: Are the insights from basic research ("understanding") useful for applications ("improving")? Does the transfer from theory to practice ever work in education?

2. Are there any enduring results? This challenge is repeatedly raised by mathematicians and other traditional scientists. Where are the scientific laws or "theorems" of this field? Have we learned anything that was not already known to good teachers everywhere? Are there any results from educational research that are as credible as the best of traditional scientific research? Has the RscienceS of education advanced at all since Plato? Is educational research merely a rediscovery of known truths—or worse yet, a camouflaging of wisdom in obscure language?

3. Is educational research significant? One test of significance is replicability. Indeed, the credibility of science rests on the ability to reproduce results under identical conditions. In education, one wonders if the variables can ever be controlled sufficiently to meet this stringent test. If no two situations are ever identical, how can one be sure that any result is reliable? What are the characteristics of reliable educational research? Must replicability be one of them? How can research be vested with meaning and significance in the absence of replicability?

4. Is educational research predictive? The ability to predict events is another test of scientific research, especially valuable in fields such as astronomy and geology where controlled, replicable experiments are rare. Where are the results of educational research that predict results? More

challenging, are there any surprising predictions? Is educational research any better than intuition as a predictive tool?

5. What does educational research explain? Some parts of science (e.g., string theory) are judged not so much by their empirical results as by their contributions to grand theories that help explain how things fit together. Much of mathematics fits this paradigm: the deepest results are those with greatest explanatory value. So one must ask of educational research: what does it explain that we didn't know beforehand? Does it teach us anything new? Are there any Raha!S insights?

6. Is educational research about teaching or learning? Do the insights from what is learned about teaching have any connection with student learning? Do insights about how students learn actually help teachers improve their teaching? More fundamentally, can educational research verify any meaningful relationships between teaching and learning?

7. Is educational research useful? Is it believable, reliable, persuasive? Is the body of educational research sufficiently strong to convince skeptics? Can the results of research, if properly communicated, change people's instincts and beliefs? We know that university faculty mathematicians especially—do not include educational research among the repertoire of relevant and important knowledge required of a practicing university or college faculty member. But even if they had this knowledge, the question remains: is the evidence powerful enough to change habits of mind?

8. Does it work? This is, after all, the bottom line. The purpose of education is to educate, so it is not unfair to ask of educational research whether the students of those who read and understand all that is written about educational research learn more mathematics than those who study with mathematicians who disdain educational research. To answer this may require educational research on the effects of educational research. Who would believe that?

9. What findings of educational research are relevant to student learning of mathematics? What has been learned about cognition that explains how students learn mathematics? What has been learned about social factors that influence the way students learn mathematics? Are there cognitive, developmental, or social factors in learning that are unique to mathematics?

10. Is education susceptible to research? Is education more an art than a science? The many examples of students who succeed despite "bad" teaching and students who fail despite "good" teaching fuel suspicion that education is less a science than an art, where beauty is as much in the eye of the beholder as in the mind of the creator. Can this suspicion be addressed by evidence? Can research help determine just what part of education is really scientific?

11. What are the right questions to ask? Much educational research is primarily a sophisticated analysis of particular students' learning under particular conditions. Other research probes the mind of individuals in order to learn how students learn. Should educational research inquire not only into whether students learn, and how, but also into what they should learn? Should it inquire into what teachers need to know in order to lead students to learn? (Do teachers who know more mathematics make better mathematics teachers? Do Ph.D.'s make better teachers of undergraduate mathematics?)

12. Is statistical theory the right tool for educational research? Students are, after all, not as predictable as plants growing in variously treated patches. Why is a methodology of analysis and significance testing that was developed as a tool for agricultural research the most widely used tool for educational research? Is it even possible to carry out double blind experiments in education? Might newer methods (ethnographic, philosophical, historical) provide better insights? Are statistical methods in education provably more replicable, more predictive, or more explanatory than other methods?

13. Is it possible to fairly assess educational innovation? A common question for educational research is the question of comparison: is treatment A (calculus with calculators) better than treatment B (calculus with chalk)? But what is the basis for comparison? Does one measure both groups on their use of both chalk and calculators? Just how can one fairly compare groups educated in classes with different objectives? Without fair comparison, is it possible to assess innovation? How can we determine whether the innovation will survive transplantation to a new environment?

14. Dare we do educational research on college students? Undergraduate courses represent the last chance for students to learn mathematics. How can faculty justify subjecting such students to unproven methods for the sake of research? Will students and parents tolerate having their taxes and tuition used to turn courses into experiments and students into guinea pigs?

15. Does the act of research change the outcomes of research? The reputation of the "Hawthorne" effect has dogged educational research much as the uncertainty principle has limited the horizons of quantum physics. Since the efforts put forth by students are known to be influenced by the degree to which someone pays attention to them, the very act of research will almost surely introduce a significant confounding factor in observed results. As important as this technical difficulty may be, perhaps even more important is the degree to which widespread belief in the Hawthorne effect, whether warranted or not, undermines credibility of research no matter how carefully done.

16. Can one hear the signal amid the noise? The linkage between teaching and learning is mediated by numerous factors whose variability is enormous and largely beyond the control of any researcher. In this environment, the observed effects of the few variables we can control are quite likely to be indistinguishable from the many we cannot control. Can we be sure that effects we observe are due to the causes we have created?

17. Who is qualified to conduct research in undergraduate mathematics education? There are specialists in educational research, and specialists in mathematics research. But what qualifies one as a specialist in mathematics education research—particularly at advanced levels where the nature of mathematical practice becomes more dominant? Is it necessary to have advanced degrees both in education and in mathematics? Can one be credible in both worlds without such credentials?

18. Are the results publishable? More to the point, will the research still make valuable reading ten or twenty years later? Might reports of practice, no matter how carefully (e.g. statistically) documented, be more like "news" than like "research"? Is educational research primarily of ephemeral value, useful for the moment but not for the ages? If so, does that diminish its value for the moment?

19. Will anyone read it?. How can the mathematical community persuade mathematicians to read and learn from published research reports? Research on undergraduate mathematics education is a scattered literature, with no flagship journal and no editorial traditions. Research will be read only if it becomes de rigueur, if lack of knowledge of major papers yields professional embarrassment. For that to happen, one must answer a prior question: will one miss anything of vital importance by remaining ignorant of educational research?

20. Does it count for tenure? At last, the only question that really counts.