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Reflections on Problem Solving Theory and Practice

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Abstract: In this article, the author reflects on the current state of mathematical problem solving, both in theory and in instruction. The impact of the book Mathematical Problem solving (Schoenfeld, 1985) is also discussed, along with implications of problem solving today with the advent of 21st century technologies.

Keywords: Mathematical problem solving; Mathematics teaching; Mathematical learning

Introduction

My book *Mathematical Problem Solving* (Schoenfeld, 1985), which I shall refer to as *MPS*) was published more than 25 years ago. *MPS*, which was fundamentally concerned with research and theory, had been developed in dialectic with a course in problem solving at the university level. The book provided a theoretical rationale for the course, and evidence that it worked; the course was an existence proof that, with the "right" kinds of instruction, students could become more effective problem solvers. The book-plus-course addressed a series of theoretical and pragmatic questions, some of which they answered, some of which they suggested answers to, and some of which they left unaddressed. Either directly or by logical extension the ideas in the book had the potential for significant curricular impact, if the "lessons" in them were taken seriously.

The question is, what has been the fate of the ideas that the book and the course embodied? Which ideas survived, which flourished? Which evolved in unpredictable

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ways, which withered with unfulfilled promise? I am grateful to the editors for the opportunity to reflect on the past and to think about future opportunities.

I begin by describing what, in my opinion, were the achievements, failures, and potential of that early work (which, of course, built upon and reflected the state of the field in 1985). This is followed by a characterization some of the main outcomes of the evolution of problem solving research and development. There is, of course, a huge literature on problem solving. It is impossible to do justice to that literature, and my comments will be selective. My most general comments are based, in part, on the volume *Problem solving around the world – Summing up the state of the art* (Törner, Schoenfeld, & Reiss, 2008). That volume provides a recent overview of theory and practice (and to some degree, curricular politics) in a wide variety of nations. This article will update my article in that volume (Schoenfeld, 2008), characterizing recent and potentially significant events in the U.S.

Problem Solving as of 1985 – a retrospective view

In theoretical terms, what *MPS* offered in 1985 was a *framework* for the analysis of the success of failure problem solving attempts, in mathematics and hypothetically in all problem solving domains. "Problem solving" at its most general was defined as trying to achieve some outcome, when there was no known method (for the individual trying to achieve that outcome) to achieve it. That is, complexity or difficulty alone did not make a task a problem; solving a system of 100 linear equations in 100 unknowns without the use of technology might be a real challenge for me, but it is not a *problem* in the sense that I know how to go about getting an answer, even if it might take me a very long time and I agonize over the computations.

The core theoretical argument in *MPS*, elaborated slightly in Schoenfeld (1992), was that the following four categories of problem solving activity are necessary and sufficient for the analysis of the success or failure of someone's problem solving attempt:

a) The individual's knowledge;
b) The individual's use of problem solving strategies, known as heuristic strategies;
c) The individual's monitoring and self-regulation (an aspect of metacognition); and
d) The individual's belief systems (about him- or herself, about mathematics, about problem solving) and their origins in the students' mathematical experiences.

Regarding (a), little needs to be said; one's mathematical knowledge is clearly a major determiner of one's mathematical success or failure. Regarding (b): In 1985 I singled out heuristic strategies for special attention, because my major intuition when I began doing research on problem solving was that, with the right kinds of help, students could learn to employ the heuristic problem solving strategies described by Pólya (1945/57, 1954, 1962,65/81). Regarding (c): research over the course of the 1970s and early 1980s had revealed that how well problem solvers "managed" the resources at their disposal was a fundamental factor in their success or failure. When working complex problems, effective problem solvers monitored how well they were making progress, and persevered or changed direction accordingly. Unsuccessful problem solvers tended to choose a solution path quickly and then persevere at it, despite making little or no progress (see, e.g., Brown, 1987; Garofalo & Lester, 1985). Finally, regarding (d): by the time that *MPS* was published, many counterproductive student beliefs, and their origins, had been documented. For example, students whose entire mathematical experience consisted of working exercises that could be solved in just a few minutes came to believe that "all problems can be solved in five minutes or less," and ceased working on problems that they might have been able to solve had they persevered.

By these categories of behavior being "necessary and sufficient" for the analysis of problem solving success or failure, I meant that:

> They were necessary in the sense that if an analysis of a problem solving failed to examine all four categories, it might miss the cause. It was easy to provide examples of problem solving attempts for which each of the four categories above was the primary cause of success or failure.
>
> They were sufficient in that (I posited that) no additional categories of behavior were necessary – that the root cause of success or failure would be found in categories (a) through (d) above.

In *MPS* I claimed that the framework described above applied for all of mathematical problem solving; I had ample evidence and experience to suggest that that would be the case. I conjectured, on the base of accumulated evidence in other fields, that the framework applied to *all* problem solving domains, broadly construed. If you take problem solving in any of the sciences, there was a face value case for the framework. The relevant knowledge and strategies would be different in each domain – knowledge and heuristic strategies are different in physics or chemistry than in mathematics – but it was easy to se that the framework fit. But the potential application was broader. Consider writing, for example. Someone who sits down to write an essay, for example, is engaging in a problem solving task – the task being to create a text that conveys certain information, or sways the opinion, of a particular audience. Various kinds of knowledge are relevant, both factual and in terms of text production. Writers use heuristic strategies for outlining, using topic sentences, etc. They can profit from monitoring and self-regulation; or they can lose track of their audience or argument, thus wasting time producing text that will ultimately be discarded. Finally, beliefs are critically important: the writer who believes that writing simply consists of writing down what you think will produce very different text from the writer who believes that crafting text is a challenging art requiring significant thought and multiple edits.

In sum, *MPS* offered a framework for analyzing the success or failure of problem solving, potentially in all problem solving domains. At the same time, the work reported in *MPS* had significant theoretical limitations. My analyses of problem solving all took place in the lab: one or two individuals sat down to work on problems that I had chosen. In various ways, this represented very significant constraints on their problem solving, and thus on my analyses. First, they were given the tasks. In most real-world problem solving, the tasks emerge in practice and have a history or context of some sort. Second, the goals were pre-determined (the students were to solve *my* problem) and the problems themselves were fixed. In problem solving "au naturel," goals and the problems themselves often change or emerge in interaction. Third, the timescale was relatively short. Fourth, social interactions were minimal. Fifth and most important, *MPS* offered a framework, highlighting what was important to examine in order to explain success or failure. What *MPS* did not offer was a *theory* of problem solving – a characterization that allowed one to explain how and why people made the choices they did, while in the midst of problem solving. All of these were limitations I wished to overcome. My ultimate theoretical goal has been to provide a theoretical explanation that characterizes, line by line, every decision made by a problem solver while working on a problem (trying to achieve one or more complex goals) in knowledge intensive, highly social, goal-oriented activities. In 1985 that goal was far beyond what the field could do.

Let me now turn to issues of practice. First and foremost, *MPS* was an existence proof, at multiple levels. At the macro level, the book provided evidence that my problem solving courses really worked – that my students became much more effective problem solvers, being able to solve more and more difficult *unfamiliar* problems after the course than before. At a finer level of grain size, examining students' work after the course showed that it was indeed possible for the students to master a range of problem solving heuristics; that they could become more effective at monitoring and self-regulation; and that on the basis of their experiences in the course, students were able to evolve much more productive beliefs about themselves and mathematics. At a yet finer level of grain size, *MPS* offered a methodological blueprint for developing problem solving instruction.

The challenge in 1975, when I began my problem solving work, was that heuristic strategies "resonated" – when mathematicians read Pólya's books his descriptions of problem solving strategies felt right – but, it had not yet been possible to teach students to use such strategies effectively. A major realization was that Pólya's descriptions of the strategies were too broad: "Try to solve an easier related problem" sounds like a sensible strategy, for example, but it turns out that, depending on the original problem, there are at least a dozen different ways to create easier related problems. Each of these is a strategy in itself; so that Pólya's name for any particular strategy was in fact a label that identified a *family* of strategies. Once I understood this, I could "take apart" a family by identifying the main strategies that fell under its umbrella. I could teach each of those particular strategies (e.g., solving problems that had integer parameters by looking at what happened for n = 1, 2, 3 , 4 . . . ; looking at lower-dimensional versions of complex problems; etc.), and when the students had learned each of these, they had mastery of the family of strategies that Pólya had named. What that meant was that understanding and teaching Pólya's strategies was no longer a theoretical challenge, but an empirical one. One could imagine a purely empirical, pragmatic program: take the main heuristic strategies identified by Pólya; consider each as a family of strategies and decompose them into their constituent parts; and work out a straightforward instructional program that enabled students to learn each of the constituent strategies. In this way, it should be possible to make problem solving accessible to all students. I hoped that some such work would take place.

A quarter-century later . . .

Issues of theory

Here there is good news, both in terms of what has been achieved and how the theoretical horizon has expanded. As noted above, the major challenge with regard to problem solving was to build a *theory* of problem solving, rather than a *framework* for examining it. More broadly, the challenge was to build a theory of goal-oriented decision making in complex, knowledge-intensive, highly social domains. Mathematical problem solving or problem solving in any content area, is an example. The goal is to solve the problem; knowledge (including knowledge of various strategies) is required; and, depending on the context, the problem solving activities may be more or less socially engaged. Mathematics (or other) teaching is another, much more complex activity. The goals here are to help students learn mathematics. Achieving those goals calls for a huge amount of knowledge and strategy, and for deploying that knowledge amidst dynamically changing circumstances: when a student suddenly reveals a major misconception, for example, or it becomes apparent that the class does not have a good grip on something that the teacher thought they understood, the current “game plan” has to be revised on the spot and something else put in its place. In fact, if you can model decision making during teaching, it is straightforward to model decision making in other complex knowledge-intensive domains such as medical practice, electronic trouble-shooting, and more.

By “model” I mean the following. One needs to specify a theoretical architecture that says what matters, and say how decision making takes place within that architecture. Then, given any instance of such decision making (e.g., problem solving or teaching), one should be able to identify the things that matter in that instance, and show how the decision making took place in a principled way (that is, through a structured model consistent with the theoretical architecture), using only the constructs in the theoretical architecture to build and run the model. By way of crude analogy, think of Newton’s theory of gravity as providing a theoretical architecture (the inverse square law) for characterizing the motion of a set of objects. For each object (say the planets in our solar system, plus the sun) the some parameters need to be specified: mass, position , direction, velocity. The model of the solar system is given these data for time T, and the theory is used to specify these parameters for time T+1. The theory, then, is general; each model (whether of our solar system or some other galactic system) is a specific instantiation of the theory. The quality of any particular model is judged by how well the behavior of the objects represented in the model corresponds to the behavior of the objects being represented. (A model of the solar system had better produce motion that looks like the motion of the planets in our solar system!) The quality of the theory is judged by its accuracy and it scope – what is the range of the situations for which it can generate accurate models? (A theory that only modeled two-body gravitational systems wouldn't be very exciting.)

Twenty-five years after *MPS* was published, my new book *How We Think* (Schoenfeld, 2010) builds on the earlier work and lays out the structure of a general theory of in-the-moment decision making. The architecture it describes is straightforward: what one needs for a theoretical account of someone's decisions while that person is engaged in a familiar goal-oriented activity such as problem solving, teaching, or medical practice is a thorough description of:

a) The goals the individual is trying to achieve;
b) The individual's knowledge (and more broadly, the resources at his or her disposal);
c) The individual's beliefs and orientations (about himself and the domain in which he or she is working); and
d) The individual's decision-making mechanism.

These categories represent the natural evolution of the categories in the 1985 framework. Regarding (a), depicting the goals is necessary in that the theory describes a much broader spectrum of behavior than problem solving. Depending on context, one's highest priority goal may be, for example: to solve a problem; to make sure that one's students understand a particular body of mathematics; or to diagnose a patient appropriately and set him or her on a path toward recovery. Regarding (b), the role of knowledge is still central, of course: what one can achieve depends in fundamental ways on what one knows. In my current theoretical view I fold access to and implementation of heuristic strategies into the category of knowledge. I always viewed problem solving strategies as a form of knowledge, of course – but, in the problem solving work I was trying to validate their importance and utility, so they were separated out for special attention. In addition, I add "resources" into the category of "what the individual has to work with": the approach one takes to a problem may vary substantially depending on, for example, whether one has access to computational tools on a computer. Regarding (c), beliefs still play the same central role in shaping what the individual perceives and prioritizes as in my earlier work. I have chosen to use the word orientations (including preferences, values, tastes, etc.) as a more encompassing term than beliefs because, for example, choices of what to purchase for dinner and how to cook it, while modelable in terms of the architecture I specify, aren't necessarily a matter of beliefs.

Regarding (d), the decision making mechanism in the theory is implemented in two ways. If circumstances are familiar – that is, one is collecting homework or going over familiar content in class – people use various mechanisms described in the psychological literature (scripts, frames, schemata, etc.) that essentially say what to do next. If circumstances suddenly vary from the predictable – e.g., a student makes a comment indicating a serious misconception, an explanation obviously doesn't work – then it is possible to model the individual's decision making using a form of subjective expected utility. (The various options that might be used are evaluated in light of their perceived value to the person being modeled, and the higher a valuation an option receives the more likely the option is to be chosen.) Monitoring and self-regulation, which were a separate category in *MPS,* still play a centrally important role – but here they are placed as a major component of decision making.

To my mind *How We Think* has roughly the same status today that *MPS* had in 1985. The book offers a number of very detailed case studies, showing how a wide range of mathematics teaching can be modeled, and an argument suggesting (by virtue of the breadth of "coverage" in the cases) that the model applies to all teaching. Then, there are suggestions that the theory should suffice to describe goal-oriented decision making in all knowledge-intensive fields. This is a heuristic argument similar to the argument I made in 1985, that the problem solving framework I explicated for mathematics should apply to all problem solving disciplines. Time will tell if the theory holds up.

While *How We think* brings to fruition one theoretical line of inquiry, it also opens up a number of others – lines of investigation that I think will be fruitful over the coming decades. These may or may not strike the reader as falling under the banner of "problem solving" – but, they should, if the question is, what do we need to know about thinking, teaching, and learning environments to help students become more effective mathematical thinkers and problem solvers? (I will revisit this question directly when we turn to practical issues.)

My work to date has examined problem solving through the lens of the individual, at any point in time. That is, the question has been, how and why does the individual go about making decisions in the service of some (problem solving) goals, given what he or she knows? These are serious limitations. First, the focus on what is happening in the moment ignores questions of learning and development. The person who has worked on, and solved, a problem, is not the same person who began working on it. He or she approaches the next problem knowing more than before. So, one question is, how can issues of learning and development be incorporated into a theory of decision making? This is a deep theoretical question, which may not have immediate practical applications – but, if we can trace typical developmental trajectories with regard to students' (properly supported) ability to engage in problem solving, this might help shape curriculum development. More generally, if our goal is to theorize cognition and problem solving, such issues need to be addressed.

Second, individuals do not work, or learn, in a vacuum. As will be seen below, characterizing productive learning environments – and the norms and interactions that typify them – is an essential endeavor, if we are to improve instruction. But learning environments are highly interactive, and the ideas that individuals construct are often built and refined in collaboration with others. At minimum, a theory of learning and cognition that explains how ideas grow and are shared in interaction is critical. There is much to be done on the theoretical front.

Issues of practice

Here, the question is whether one wishes to view the metaphorical glass as being half empty or half full. There is reason to be disheartened, and reason to be encouraged. And there is work to be done.

On the one hand, there are ways in which we could and should be much further along in curricular development (and the research that would undergird it) than we are. As explained above, there was an implicit blueprint for progress in *MPS*: the methods I described for decomposing heuristic strategies into families of more fine-grained techniques, and finding out how much instruction was necessary for those techniques to become learnable, were well enough characterized for others to implement them. That is, 25 years ago it was theoretically possible to begin a straightforward program of development that would result in successful instruction on a wide range of problem solving strategies. "All" that was needed was a huge amount of work! That work did not get done. There are systemic reasons for this, which Hugh Burkhardt and I (Burkhardt & Schoenfeld, 2003) have explored. University reward systems work against this kind of work. There is no theoretical "glory" in working through such pragmatic issues, either for the individual or in terms of promotion decisions at research universities. Making significant progress at the curricular level calls for a team of people, and university reward structures are stacked against that as well – our system tends to reward individual achievements, and to give less credit for collaborative work. Perhaps for those reasons, perhaps because there are fads and fashions in educational research (as in all fields), an area that I considered to be fertile ground for practical development went unexplored. I think that's a shame.

At the same time, some good things have happened in K-12 education. A global summary of developments can be found in Törner, Schoenfeld, & Reiss, 2008. Here I will summarize the optimistic view regarding the past 25 years in the U.S., and then point to the fact that we are at a turning point, where much hangs in the balance.

In contrast to some nations where a ministry of education or its equivalent makes curricular decisions that are implemented nationwide, the U.S. has had what is best described as a "loosely coupled" system. For almost all of its history, each of the states had its own educational system, which was responsible for setting statewide standards.

Historically, textbook decisions have been local – each of the roughly 15,000 school districts in the U.S. could choose is own textbooks. Until the past decade, few states had statewide assessments, so there was little pressure to "teach to the test." There were homogenizing factors, of course. There were a small number of textbook publishers, so textbook choice, though theoretically unconstrained, was limited in practice; and, most school districts aimed at preparing their college-intending students for the (essentially universal) college calculus course, so the goal state was clearly established. By tradition, grades K-8 focused on arithmetic and then pre-algebra; algebra I was taken in 9th grade, plane geometry in 10th, algebra II and possibly trigonometry in 11th, and pre-calculus in 12th. Some students accelerated through calculus in high school; many students dropped out of the pipeline altogether. (The generally accepted figure in the 1980s was that each year, some 50% of the students at each grade level in secondary school failed to take the next year's mathematics course.) There was huge variation in the courses students took, but "traditional" instruction focused mostly on conceptual understanding and mastery of skills and procedures. There was a negligible amount of "problem solving," by any real definition.

In 1989 the National Council of Teachers of Mathematics produced the *Curriculum and Evaluation Standards for School Mathematics.* The volume was intended for teachers, and had few references; but the authors knew the problem solving research, and it showed. For the first time in a major policy document, there was a significant emphasis on the *processes* of doing mathematics: The first four standards at every grade level focused on problem solving, reasoning, communication using mathematics, and connections within and outside of mathematics. The U.S. National Science Foundation, recognizing that commercial publishers would not build such textbook series on their own, issued a request for proposals for the creation of “Standards-based” texts. In each of these texts, the authors elaborated their own vision of what it meant to learn according to the *Standards.* This variety was a good thing: different visions of a richer mathematics, focused on problem solving and reasoning, began to emerge. It is hard to get precise figures, but some estimates are that 20-25% of the K-12 textbooks in use today are Standards-based. Given the vagaries of the “loosely coupled” educational system in the U.S., that’s a non-trivial impact for research ideas! (Of course it took 25 years, and the ideas don’t necessarily reflect, and may sometimes be contradictory to, the views of the original researchers. But that’s the way the system works.)

So, there has been curricular progress in K-12 mathematics in the U.S., if not as much as one would like. Recent political events mean that the progress will either be accelerated or blocked, in the near future. As part of an attempt to improve mathematics instruction called the “Rate to the Top” initiative (see http://www2.ed.gov/programs/racetothetop/index.html), the federal government offered fiscal incentives to *collections* of states that produced high quality standards and plans for assuring that students reached them. Given the short time frame to apply for funding, the U.S. National (State) Governors Association and the Council of Chief State School Officers supported an effort to construct a set of standards for mathematics, known as the Common Core State Standards (see http://www.corestandards.org/, from which the Common Core State Standards for Mathematics (CCSSM) can be downloaded.)

As of this writing, 44 of the 50 states have committed to the Common Core initiative, meaning that they will replace their current state standards with CCSSM. By federal statute, they will need to use assessments (tests) that are deemed consistent with the CCSSM in order to measure student progress toward the goals of CCSSM. Two national consortia have been funded to produce assessments consistent with the CCSSM: PARCC (the Partnership for Assessment of Readiness for College and Careers; see http://www.achieve.org/PARCC) and the SMARTER Balanced Assessment consortium (see http://www.k12.wa.us/smarter/). By the time this article appears, both consortia will have published their “specs” for assessments consistent with CCSSM. Simply put, those assessments (tests) will shape the mathematics experiences of the students in the states that have committed to the Common Core State Standards initiative. As we know, testing – especially high-stakes testing – determines the foci of classroom instruction. The CCSSM place significant emphasis on what they call mathematical *practices,* claiming that people who are mathematically proficient:

- Make sense of problems and persevere in solving them.
- Reason abstractly and quantitatively.
- Construct viable arguments and critique the reasoning of others.
- Model with mathematics.
- Use appropriate tools strategically.
- Attend to precision.
- Look for and make use of structure.
- Look for and express regularity in repeated reasoning.

If the tests produced by the consortia provide students with opportunities to demonstrate such mathematical habits of mind, the tests will serve as a lever for moving the K-12 system in productive directions. But, if they consist largely of short answer questions aimed at determining students' mastery of facts and procedures, they will serve impede the kind of progress we have been making over the past 25 years.

In sum, progress in K-12 has been slow but steady; it may get a boost or a setback in the immediate future, depending on the high-stakes tests that the two assessment consortia adopt. But there has been significant progress. I wish I could say the same about collegiate mathematics over the same time period. For a while calculus reform flourished, but it seems to have stabilized and become "same old, same old." There have been glimmers of excitement surrounding innovations in linear algebra and differential equations (stimulated in some part by technology), but not so much that the general zeitgeist of collegiate mathematics instruction is noticeably different from what it was when I was a math major. And that was a long time ago!

Rethinking "problem solving"

I got my "start" in problem solving and I still think that, in some ways, it deserves to be called "the heart of mathematics" (Halmos, 1980). More broadly, there is a view of mathematics as the "science of patterns" (Steen, 1988). What I like about this framing is that one thinks of science as consisting of systematic explorations – and mathematics as we practice it certainly has that character. This broad a framing includes problem posing as well as problem solving, and a certain form of empiricism, which was made explicit in Pólya's (1954) title, "patterns of plausible inference." In doing mathematics we explore; we seek systematicity; we make conjectures; and, we use problem solving techniques in the service of making and verifying those conjectures. Yet more broadly, we engage in "thinking mathematically" – a title used by John Mason, Leone Burton, and Kaye Stacey (1982, 2010) and which I wish I'd thought of. Here I want to push things one step further.

At heart, doing mathematics – whether pure or applied – is about sense-making. We observe an object, or a relationship, or a phenomenon, and we ask: What properties must it have? How do we know? Do all objects that look like this have the same property? Just what does it mean to "look like this"? Are there different ways to understand this? With that mindset, simple objects or observations become the starting points for explorations, some of which become unexpectedly rich and interesting. Third graders observe that every time they add two odd numbers the sum is even. Must it always be so? How would one know? We observe that some numbers can be factored, others can't. How many of the unfactorable kind are there? How can I measure the height of a tree without climbing it? How many different crayons do I need to color a map, so that every pair of countries that share a border have different colors?

What I strive to do in my problem solving courses is to introduce my students to the idea that mathematics is about the systematic exploration and investigation of mathematical objects. Elsewhere (see, e.g., Schoenfeld, 1989; see also Arcavi, Kessel, Meira & Smith, 1998; English & Sriraman, 2010) I have described our first-week discussions of the magic square. We start with the 3 x 3, which my students solve easily. But that is just a start. Did we have to get an answer by trial and error, or are there reasons that even numbers go in the corners, and that 5 goes in the center? Is the solution unique (modulo symmetry), or are there distinct solutions? Having finished with the original 3 x 3, we ask: what if I had nine other integers? Say 2 through 10, or the odd numbers from 1 through 17, or any arithmetic sequence? Can we find a magic square for which the sum of each row, column, and diagonal is 87? How about 88? We observe that if we multiply all the cells in a magic square by a constant, we get a magic square; if we add a constant to each cell, we get a magic square. Thus, we can generate infinitely many 3 x 3 magic squares. But can we generate all the 3 x 3 magic squares this way?

The reason for this discussion is that I want to introduce my students to what it means to *do* mathematics. I want them to understand that mathematics isn't just about mastering facts and procedures, but that it's also about asking questions (problem posing, if you will) and then pursuing the answers in reasoned ways. The problem solving strategies are tools for sorting things out, seeing what makes the mathematical objects and relationships "tick." So yes, we are solving problems, but as part of a larger sense-making enterprise. That, in part, is why attending to my students' beliefs is so important a part of the course. Having been "trained" by their prior experience to understand (believe) that doing mathematics means "mastering" content selected and organized by others; that all problems can be solved in short order, usually by the techniques the teacher has presented within the past week; that proofs have nothing to do with discovery; and so on[2], my students needed to be "untrained" or "retrained" by their experiences in my course. Thus I give them extended opportunities to make observations and conjectures, and provide them with the tools that enable them to experience the doing of mathematics as a sense-making activity.

My question is, can't we approach all mathematics teaching this way? I believe that all of K-12 mathematics, and a good deal of collegiate mathematics, can be seen as a set of sensible answers to a set of sensible questions. Given the pace at which K-12

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[2] See Schoenfeld (1992) for a list of counterproductive beliefs that students typically develop.

mathematics proceeds, I am sure that this could be done without any formal loss of content. What would be gained is that students would experience mathematics as an exciting sense-making domain, which is the way we see it as mathematicians. If K-12 students truly experienced mathematics that way, I'm willing to be that similarly oriented collegiate instruction could build on well-established habits of mind, and proceed much more effectively than it currently does. (Despite the fact that the students in my problem solving courses through the years could be labeled "the best and the brightest" – it's a non-trivial achievement to get into Berkeley – my feeling has always been that my problem solving courses have been remedial in a significant way. The vast majority of the students who entered those courses were unaware of basic mathematical problem solving strategies, and, as a function of their experience, did not view mathematics as a domain that they could make sense of.)

Rethinking Research on Classroom Environments

A comment made by one of the advisory board members of one of my projects ("Classroom Practices that Lead to Student Proficiency with Word Problems in Algebra", NSF grant DRL-0909815), struck me as particularly interesting. Megan Franke (2011) noted that, of the various classroom variables she had looked at, the one that seemed to have the strongest impact on student learning was the amount of time students spent explaining their ideas. This resonates, not only with the discussion of sense-making above, but with an emerging body of research focusing on the character of classroom environments that support the kinds of rich student engagement and thinking that one would like. Engle, for example (Engle, in press; Engle and Conant, 2002) has developed a "productive disciplinary engagement framework." Reviewing the best-known examples of rich learning environments in mathematics and science, Engle concludes that the most powerful learning environments all include aspects of:

- Problematizing – students participate in the act of framing meaningful questions, which the class explores.
- Agency and authority – students are empowered to seek information, distill it, craft arguments, and explain them.
- Disciplinary accountability – students learn what it is to make claims and arguments that are consistent with disciplinary norms.
- Resources – when tools or information is needed, the students have access to them.

For an elaboration of these ideas with examples drawn from my problem solving courses, see Schoenfeld (2012). Gresalfi, Martin, Hand, & Greeno (2009) offer a framework (see Fig. 1) that indicates ways on which classroom participation structures can lead to differential outcomes in terms of student agency, argumentation, and accountability.

<table>
<tr><td rowspan="6">Negotiation of</td><td rowspan="3">Participation structure</td><td>How an idea enters the common ground
Other people need to be able to understand</td><td rowspan="6">Leads to definitions of</td><td rowspan="2">What students are accountable for
Making sure that their symbols make sense</td></tr>
<tr><td>Who is expected to author or critique ideas
Other students</td></tr>
<tr><td>Norms of argumentation
students need to talk to and convince each other</td><td rowspan="3">Who students are accountable to
Other students and the teacher</td></tr>
<tr><td rowspan="3">Task as realized</td><td>Requirements for sense-making
doing mathematics—coordinating different records of the trip</td></tr>
<tr><td>Openness of the task
moderate—inventing symbols that others' could understand</td></tr>
<tr><td>Requirements for successful completion
Students need to be able to understand others' representations</td><td>What kinds of agency students can exercise.
Conceptual</td></tr>
</table>

Fig. 1. A model of how competence gets constructed in the classroom.
From Gresalfi, Martin, Hand, & Greeno (2009), p. 54, with permission.

This kind of framework can be useful, as we seek to understand both how to craft classrooms more focused on sense making and to document their effects.

Rethinking Technology (a.k.a. entering the 21st century)

When I took my first statistics class, all of the examples were "cooked." This was before the days of widespread access to calculators and computers, so everything I did had to be hand-computable. As a result, the variation of every distribution I worked with was a perfect square! The presence of computational technology should have radicalized the ways in which our students can engage with statistics, in that no data analysis is now an obstacle. Students should be able to ask their own questions and gather their own data. Yet, few students have this experience. In Schoenfeld (2012) I give the example of how, sitting at my desk in early June and watching it rain, I wondered whether this was atypical. The San Francisco Bay Area is supposed to have a "dry" season, and it seemed that we had gotten more rain than we should have. Using Google I was quickly able to find data regarding annual monthly rainfall and recent rainfall, at which point I could do some simple statistical analyses to verify that this year's June rainfall was anomalously high. (In fact, it went on to set a record.) From my perspective, I was clearly doing mathematics. My question is, where do today's students learn to gather such information and to operate on it? Asking questions, seeking data, building models, and drawing inferences should be everyday experiences for our students.

Similarly, the presence of computational tools – whether symbolic calculators, graphers, or Wolfram Alpha (see http://www.wolframalpha.com/) – has the potential to radically reshape the knowledge to which students have access in mathematics classrooms, and the ways they can operate on it. Pure mathematics can become an empirical art for students in ways that it was not, even for mathematicians, until recently. Where are students learning to harness these skills – not for the sake of learning to be fluent with technology, but as means to mathematical ends? There have been some inroads along these lines, for example with dynamic geometry software, but for the most part these positive examples are the exceptions that probe the rule.

Concluding Comments

I thank to the editors for the opportunity to think about the current state of mathematical problem solving, both in theory and in instruction. I became a mathematics educator many years ago because of my love for mathematics and my wish to share it with students, who were typically deprived of the pleasures that I consistently experienced as a mathematician. Problem solving provided a way into the joys of doing mathematics and the pleasures of discovery. I firmly believe that problem solving – or a broader conception of mathematics as sense making – still can do so, and I hope to see us make progress along those lines.

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