The Wild, Wild, Wild, Wild, Wild World of Problem Solving (A Review of Sorts)

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How would you characterize "mathematical problem solving?" In all likelihood you will answer this question differently if you are a mathematician, a cognitive psychologist, or a mathematics educator: the phrase "problem solving" can take on radically altered meanings in these three disciplines This article explores some of the differences and their implications, and then looks for common ground among them. It is a review of sorts, in which an exemplary book or two from each field is examined. The substance of the books is discussed, of course. However, I shall also go beyond the texts in places, to discuss the "spirit" of the disciplines that gave rise to them. Let us begin with a problem book in mathematics In reacting to it, I put on my mathematician's hat.

The first of Hugo Steinhaus' *One hundred problems in elementary mathematics* asks the reader to consider a list of digits, $\{ N_i \}$. It starts with $N_1 = 2$ and $N_2 = 3$ Then for each $i = 1, 2, 3 \ldots$, the list is expanded as follows. We take the product N_iN_{i+1} If that product is a single digit, that digit is added to the list. If it is a two-digit number, then both digits are added to the list, with the "tens" digit first

We have $N_1 = 2$ and $N_2 = 3$. Then
$N_1N_2 = 2 \times 3 = 6$, so $N_3 = 6$
$N_2N_3 = 3 \times 6 = 18$, so $N_4 = 1$ and $N_5 = 8$, and so on.

Since each multiplication adds either one or two terms to the list, the sequence $\{ N_i \}$ is infinite The first few terms are 2, 3, 6, 1, 8, 6, 8, 4, 8, 4, 8, 3, 2, ... The problem: Prove that the digits 5, 7 and 9 never appear on the list.

This problem was new to me, and I found it intriguing. I generated a few terms in the sequence, to see if I could get a "feel" for its behavior and predict the evolving pattern of terms. I couldn't. I did, however, realize that every product of the form N_iN_{i+1} had to be even, thus that any odd digit that appeared in the list had to be the "tens digit" resulting from the product of two one-digit numbers. Since the largest such number is 81, no 9 could ever appear in the sequence. Once 9's are out, the largest available product is 64; that takes care of the 7's The case of the missing 5's is left to the reader.[1]

Even though this problem had a somewhat contrived feeling about it, I liked it. Working on it led me in a few unexpected directions (It wasn't quite as easy as the last paragraph would make it appear), and the intellectual "reward" for solving it felt about right for the amount of effort I put into it. My reactions to the other problems varied I was bored by some, either because they dealt with areas I don't find interesting or because the problems or their solutions were unaesthetic.[2] Others intrigued me. The ones I wanted to work on, but didn't know the answers to, became real *Problems* for me In these I invested intellectual (and emotional) energy.

There was, for example, problem 24: show that for any whole number n, there is a circle in the coordinate plane whose interior contains precisely n lattice points (points whose coordinates are both integers) The result is simple and elegant, the proof surprisingly easy I was pleased that my proof turned out to be almost identical with Steinhaus'. I was even more pleased when my solution to another problem was briefer than his. But there are perils as well as pleasures to engaging in this kind of problem solving. In problem 17 we are given $3n$ points in the plane, no three of which lie on the same straight line. "Can we form from these points – taking them as vertices – n triangles which do not overlap and do not embrace one another?" Well, I could, in a sort of hand-waving, contorted, inductive way. Steinhaus' solution was so straightforward – and illuminating – that I was truly embarrassed by the clumsiness of my "solution". And, of course, there were a variety of problems that I worked on for quite a while without success When you tackle real problems, even "elementary" ones, there are no guarantees.

While making such generalizations is dangerous, I think I can say that what I just described is typical of problem solving, as seen from the mathematician's point of view.[3] In "The Heart of Mathematics", for example, P.R. Halmos extols the "problem approach" to mathematics, arguing that "it is the duty of all teachers, and of teachers of mathematics in particular, to expose their students to problems much more than to facts." Halmos identifies six exemplary problem books, of which *One hundred problems in elementary mathematics* is one This book, very much in the mathematical problem solving mainstream, is uncompromisingly mathematical. Take problem 1 for example. The *interest* in the problem is that the result is slightly unexpected When one has an apparently unpredictable way to generate an infinite sequence of digits, one expects all of the digits to show up in that sequence – and with some regularity. That they do not is what makes

For the Learning of Mathematics 3, 3 (March 1983)
FLM Publishing Association, Montreal Quebec, Canada

the problem worth looking at. The *value* to working the problem lies in the solution process. By making systematic observations of a "messy" phenomenon, one gains insights into its nature. Properly polished and refined, those insights are the tools that provide the mathematical argumentation to solve the problem. Here the solution path was short and simple. Yet, working a problem this way is typical of the way "real mathematics" gets done. The *reward* for solving problem 1 is an increase in one's knowledge, a sense of pride for having had the insight, and a sense of mastery and achievement for having solved the problem. All of this is reflected in the format of the book, which is as simple as possible. Problems are posed and solutions given – period. There is a striking and not at all coincidental similarity to the format of crossword puzzle books. In both cases it is assumed that the material stands well on its own, and that the readers come prepared to deal with the material on its own terms.

It is worth stressing two final points, alluded to earlier, about the relationship between the reader of a problem book and the problems in it. First, a problem is only a *Problem* (as mathematicians use the term) if you don't know how to go about solving it. A problem that holds no "surprises" in store, and that can be solved comfortably by routine or familiar procedures (no matter how difficult!) is an *exercise*. This latter description applies to most of the "word problems" that students encounter in elementary school, to "mixture problems", "rate problems", or other standard parts of the secondary curriculum. Dealing with them is certainly an important part of learning mathematics, but (unless the context is unusual) working such exercises is not generally considered "doing problem solving".

Second, a problem is not a Problem until one wants to solve it. (The presumption in most problem books is that the reader does. Why else would he or she be looking at the problem book?) Once one wants to solve a Problem, there is an emotional and intellectual commitment to the solution, and the risks and rewards concomitant with that commitment.

We now move on to cognitive psychology, where Lauren Resnick and Wendy Ford's *The psychology of mathematics for instruction* (*PMI*) represents another world view entirely. The authors define their task as follows: "How is it that people think about mathematics?. . How does understanding of mathematical concepts develop?" The psychology of mathematics studies "how human performance of mathematically significant skills becomes fluent, and how those skills are integrated in the context of mathematical problem solving." This is a solid and responsible introductory text, a welcome alternative to the pap one usually finds in the standard, silly ed psych books. It offers a good overview of the relevant psychological literature. I'll talk about the good stuff first. My caveats come later.

PMI is divided into two parts. The first, "Mathematics as computation", is concerned with lower-order skills. Unfortunately, psychology – as represented here – puts its worst foot forward in the first substantive chapter, an exposition of "the psychology of drill and practice". To the jaundiced eye, more than half a century's intense study of that issue seems to reveal little more information about it than would be dictated by commonsense [4]. Things improve in the next chapter, however, where we read about learning hierarchies, task analyses, and the like. There are useful implications in this work for the curriculum, and also for working with individual students: careful analyses of topic structure can suggest the ordering of subject matter coverage, and point to places where students are likely to experience difficulties. But because this (behaviorist) perspective ignored the student's *mind*, it was ultimately sterile. The real paydirt comes in chapter 4, "analyses of performance on computational tasks". There we see that simple tasks are not as simple as they might seem, and that there are great benefits to unraveling their underlying complexity.

My favorite example is Brown and Burton's [1978] "buggy" paper, and the more recent extensions of it. That research, whose precursors reach back to the work of educational psychologists in the 1930's [e.g. Brownell, 1935] indicates that student's errors in elementary arithmetic are not, as one might naively expect, random mistakes that occur simply because the students have not yet learned the "right" procedures. In some 40% of the cases they examined, Brown and Burton were able to predict, with consistency, the *incorrect* answers that students would obtain for arithmetic tasks. Far from being random, these mistakes were the results of the student's consistent application of incorrect procedures – of "bugs" in their algorithms, to use the computer scientists' phrase. There are tremendous implications to this kind of finding. Pedagogically, it points to the serious flaws in the "show the students the correct procedure and let them work on it until they get it right" model of instruction. Often, students will master the "wrong" procedures. Then, in order to learn the right ones, they will have to "unlearn" the incorrect ones (i.e., be "debugged"). At a broader level, this kind of research rejects the "empty urn" model of students, where the students are considered as passive receptacles waiting for knowledge to be poured into them. Rather, it shows them to be active agents in the construction of their own knowledge, with the "bugs" as the byproduct of the students' attempts to perceive regularities in the world around them. Explorations into the nature of this kind of knowledge, and the mechanisms that support it, lead into deep questions of psychology and epistemology.

Part II of *PMI*, "Mathematics as conceptual understanding and problem solving", should take us closer to home. Indeed, one finds some absolute essentials. There is, for example, Piaget. I must be careful here, for in mentioning his name I do not wish to invoke those versions of a stage theory in which students ascend through various "stages" of development much as one climbs a staircase, magically taking the last step from "concrete" to "formal" some time around their thirteenth birthdays. Piaget himself eschewed that simplistic version of his early work decades before armies of researchers on this continent enthusiastically set about "validating" it, developing Piagetian curricula, etc. That research, some of which is reviewed in *PMI* and some of which is still being done, is of little value. Rather, I wish to invoke the theoretical notion underlying Piaget's work, that of a genetic epistemology [summarized in Piaget, 1971] To understand learning, one must explore the genesis of knowledge. That is, one must explore the way that the individuals build intellectual structures (explanatory frameworks) to interpret and interact with their environments, and how these structures grow and change.[5] The serious consideration of such issues is essential for good teaching In my biased opinion, worrying about students' problem solving performance without taking such issues into account may be somewhat like worrying about performing surgery, without considering basic physiology

Our discussion to this point has been mostly about psychology Since we have reached "conceptual understanding and problem solving", I would like to turn to the psychologists' treatment of *mathematics*, as reflected in this book. There are three more chapters in part II of *PMI*. Let us begin with "structure and insight in problem solving", which discusses the Gestaltists. Since there are notable mathematicians among the Gestaltists, one would expect to find this chapter virtually steeped in the mathematical spirit. The mathematician's perspective, as reflected in the discussion of Steinhaus' book, should shine through here. Oddly, it barely glimmers. The treatment of Wertheimer, and of psychological experiments supporting his ideas, is thorough. Likewise for the treatment of Duncker (although from the mathematician's point of view Duncker's treatment of mathematics is a bit odd: it seems somehow disjointed, out of context More about this later) And then, the treatment of Pólya is strangely dry and lifeless Yes, Pólya is a Gestaltist. But he is also an apostle of inquiry and discovery in mathematics, as well as a mathematician of note. Somehow, all of that – the essence of what he tries to communicate as a teacher – seems to get lost. Moreover, there are strange omissions in this chapter. A good case can be made that mathematical Gestaltism, if not Gestaltism itself, begins with Henri Poincaré (1854-1912), pre-eminent scientist and mathematician of his time. His collection of essays *The foundations of science* [1913] gave an opinionated survey of the state of the art, and an absolutely classic exposition of the mathematician's view of mathematics and mathematical instruction *The foundations of science* includes Poincaré's extensive discussion of his discovery of the structure of Fuchsian functions, condensed here to the following two sentences. After struggling unsuccessfully with the problem for a long time, Poincaré decided to take a "breather." He put the problem out of his (conscious) mind, and went on a geological excursion – but as he boarded the bus he had a sudden inspiration as to the underlying nature of Fuchsian functions, which he later verified at his leisure The architecture of this discovery was later codified as the famous four-step Gestalt model (preparation, incubation, illumination, verification) by Graham Wallas in his [1926] *The art of thought*. The influence of this story on mathematicians was so great that, many decades later, another eminent mathematician, Jacques Hadamard, devoted an entire monograph (*Essay on the psychology of invention in the mathematical field*) to its exegesis To many mathematicians, Gestaltism revolves around the Poincaré-Hadamard axis (although I confess that I find Hadamard's essay singularly empty) Yet neither of these two greats is even mentioned in *The psychology of mathematics for instruction* And the one mathematician of note who is, Pólya, appears only as a shadow of himself What's happening here?

What's happening is that the "mathematical" and "psychological" perspectives have run into each other, head on. The chapter "Information processing analyses of understanding", for example, deals with semantic nets, the nature of conceptual representation, access to long-term memory, etc. Indeed, these are elements of mathematical performance (though usually of the ordinary "mastery" kind). But there is little mathematics here, as mathematicians see it – and would like to see conveyed in the classroom.[6] This would not be important were *PMI* a research volume for psychologists, where the domain of inquiry happened to be mathematics. But this is a book about *Mathematics For Instruction*, clearly intended to have curricular impact. One senses, in reading it, that "this is what should be taken into the classroom (and should suffice)". There is, then, the obligation to explore the way mathematics is treated in the book.

A clear delineation of the difference in the mathematical and cognitive-psychological perspectives can be seen in chapter 5, "Teaching the structures of mathematics". What are these structures, as seen in *PMI*? There is an extensive discussion of multiple representations for base 10 numeration, through concrete arrays, expanded notation, and standard notation. There are Montessori materials, Dienes blocks, and other ways of getting these "structures" across. There are discussions of iconic, enactive, and symbolic representations These are the turf of the psychologist of Mathematics-For-Instruction, the structures and processes of *cognition*. They are a far cry from the structures and processes of *mathematics* If you ask a mathematician to discuss "the structure of the whole numbers and elementary arithmetic operations", the response may vary depending on how your question is interpreted. It will most likely be in terms of groups, rings, integral domains, and other such structures: the integers form an Abelian group under addition with 0 as additive identity, etc You may be shown how to "build" the natural numbers using the Peano axioms or treated to a proof, using a nested induction, that for any two given integers a and b, $a + b = b + a$ (Poincaré gave this argument special attention). But unless you steer the conversation in the direction of topics to which *PMI* devotes its attention, you are unlikely to hear them discussed. The ability to manipulate numbers will be taken for granted in "discussions of mathematical structures" – a term that some mathematicians might take as a definition of the study of mathematics itself. From this perspective, there is virtually no mathematics in *PMI*, and there is precious little inkling in the book of what mathematics might be about I suspect that the mathematician would consider the book a *reductio ad absurdum*: its discussion of the "structures of mathematics" can be compared with a discussion of the "structure" of a building that focuses solely on the shape of the bricks from which it is built. The psychologist's counter-argument is that a castle built with bricks of sand. Well, they are both right (and wrong). But the cast of characters should be enlarged before we try to decide who should play what role in problem solving.

While I am by nature partial to theory, it strikes even me that an element of reality is missing from the discussion thus far – classroom reality, that is. Perhaps mathematics education could provide that. Perhaps, in the best of all possible worlds, mathematics education could serve as the mediator between cognitive psychology and mathematics, taking the best of both and applying them, with wisdom, in the classroom. Perhaps, but that has not happened.

Two volumes from NCTM provide the best view of the North American math ed perspective on research and development in problem solving. The research is amply covered in *Research in mathematics education* (*RIME*), the first volume of NCTM's professional reference series. As one would hope, the focus of much of the research is on the classroom. Unfortunately, it is not terribly revealing. There are I think, three major reasons for this.[7]

The first cause of difficulty was noted by Frank Lester in his *RIME* chapter: "Past problem solving research in mathematics has suffered from the absence of neglect of theory." [p. 315] No clear sense of direction emerged in the discipline, and no coherent lines of inquiry in the research. Studies did not focus on common questions, did not build on one another, and did not help to sharpen issues for further examination. An aggregate of ad hoc empiricism, the whole of the discipline was less than the sum of its parts. [See the whole of Begle, 1978, especially his "general comments" on page 155-156.]

The second cause of difficulty is that mathematics education has not had its methodological house in order (and in this it was certainly not alone). Falling prey, perhaps, to the general desire on the part of social sciences to be "scientific", mathematics education in the 1960's and 1970's was an often statistical affair. Much of the literature was conducted under what might be called the "agricultural model" of research. The underlying assumption was that groups of humans (perhaps like fields of corn) could be "treated" in different ways, and that the "effects" of the treatments could later be determined by statistical analyses. When human beings proved more complex than their vegetable counterparts and the empirical results proved to be contradictory and confusing, the research base of the entire discipline was undermined. Though it seems so long ago, it was just in 1975 that Jeremy Kilpatrick argued that the discipline needed to undertake some case studies, in order to discover what some of the "right" issues to examine in research might be. As the discipline did move toward qualitative studies, it encountered a host of methodological questions. Now here I think that we are making great strides, both empirically and theoretically. But it has taken longer than it should have, partly because of mathematics education's isolation from other communities addressing similar questions.

The third major difficulty has to do with the way that "problem solving" is conceived in mathematics education. At the mathematical end of the spectrum, there is some question as to the degree of *P*roblem solving that appears in "Problem solving *à la* math ed." The discipline is steeped in Pólya and that, in my opinion, is the place to start. But here too Pólya suffers in translation (although not as badly as at the hands of psychologists). Pólya's intent, as I understand it, was to uncover useful strategies for inquiry into (somewhat) novel situations: His discussion of the "mental operations typically useful for the solution of problems" is for real "*P*roblems". Yes, "looking for patterns" is important and useful. But if we reduce this to the strategy "plug in the values $n = 1, 2, 3, 4$ whenever you see an n", train students on problems where the technique always works, and then test them on more of the same problems, are we dealing with *P*roblem solving? I think not, although I would argue that the subject matter we are dealing with (and it is subject matter, although rearranged in non-routine fashion) is valuable. At the cognitive end of the spectrum we find equally important issues. By now there is ample empirical and theoretical evidence to indicate that, no matter how essential heuristics may be as a component of problem solving performance, they do not tell the whole story: A teaching theory based solely on heuristics is doomed to failure.[8] In order for the discipline to make progress on problem solving, it must come to grips with this issue.

Well, so much for research. What about development? Consider as exemplar the NCTM's 1980 Yearbook, *Problem solving in school mathematics*. I am of two minds about the book. First, the positive. It is an excellent resource for teachers. In it you will find useful ideas about posing problems, supplementing textbook problems, livening up classroom discussions, using problem solving strategies, grading problem solving performance, and more. I have almost invariably recommended the Yearbook to teachers who ask me where they can "get started" in problem solving. And now the negative. I find *Problem solving in school mathematics* a deeply troubling book, because of the way it treats its subject. The word that comes to mind is *facile*, a word that I think characterizes this decade's approach to problem solving as the "theme of the 1980's". One gets the impression that it should be relatively easy for the teacher who sows some heuristic oats in the classroom to reap a harvest of budding young problem solvers. That just isn't true. If I have learned anything in a decade of research and teaching in problem solving, it is just how difficult it is to have any demonstrable and lasting effect on my students' problem solving performance. The intellectual issues involved in understanding problem solving skills are singularly complex and subtle, the issues involved in communicating that knowledge even more so. Now this is not to say that one should not try, or that the power of positive thinking will not work wonders. I believe in the value of problem solving, and think that we are making progress towards an understanding of it. But there are dangers to inducing great expectations. We have seen "drill and practice" replaced by the "new math". It was replaced in turn by the "back to basics" movement, which (happily) succumbed in its turn to "problem solving". While we are making progress in understanding problem solving, will the progress come soon enough? If not, we may be doomed to a decade of "back to back to basics" because of societal dissatisfaction with broken, facile promises.

Let us pause here to take stock. The discussion thus far is represented in Figure 1. As you might suspect, I shall argue that the resolution of the Problem of problem solving must take place in the intersection of the three circular regions – or at least that it must take liberally from all of them, and a number of others as well.

If the Venn diagram in figure 1 reminds you of the "new math", good. I would like to exhume that particular dead horse for the purpose of flogging it further, as a case in point. Of course we have hindsight and an additional twenty years of research at our disposal, but I think a clear case can be made that the "new math" was absolutely and incontrovertibly doomed to be a disaster. The idea behind it (and in general, the curricular reforms of the 1960's) was to infuse "real" mathematical content into the curriculum, early on. Of course the formal structures and procedures wouldn't be there. But simplified versions of them would, and the learner would "spiral" through increasingly complex embodiments of an idea until its richness became comfortably familiar. Unfortunately, this approach (though based in part on contemporaneous psychological theory and promoted by leading mathematics educators) stubs its toe both on psychology and the classroom. Simplified versions of abstractions are still abstractions, simplified theoretical structures still theoretical structures. Naive attempts to teach these to young students run headlong into confrontation with developmental psychology. But even if the psychology were "right", and mathematics and psychology had appropriately joined forces in the new curricula, they would have floundered on pedagogical grounds. The transmission of knowledge is a social process that depends on the cooperation and goodwill of all concerned. Teachers who are insecure about their mathematics, who feel unprepared to deal with new bodies of knowledge, and who are unhappy at having new curricula crammed down their throats, are hardly the most effective communicators of

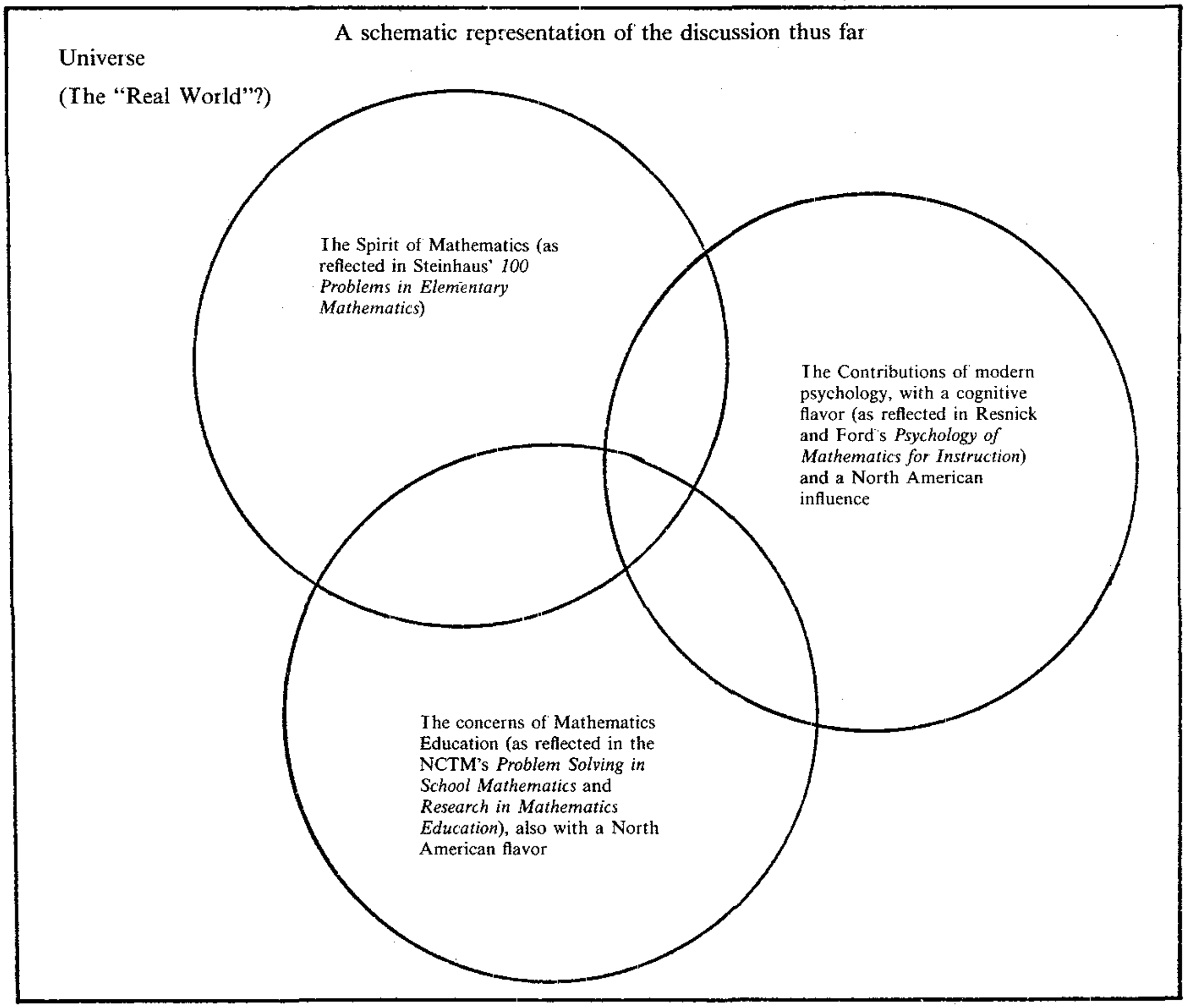

Figure 1

that knowledge It is dangerous to ignore classroom reality in a classroom discipline. Knowledge of mathematics alone, or even of mathematics and only one of psychology and mathematics education, is not sufficient.

Similar statements can be made about other regions in Figure 1 that lie outside the central intersection I would no more trust anyone who has not done mathematics to discuss the "structures of mathematics" than I would trust a teetotaler to discourse on the gustatory merits of fine wine. The view of mathematics represented in *PMI* is a travesty, and to have that represent "mathematics for instruction" would be devastating. Yet to be ignorant of the cognitive essentials in the book is equally devasting – as it has been for mathematics education, in the areas of legitimate overlap [9] Finally, there is more to mathematics than the structures of mathematics. There are the aesthetics of mathematics, and the emotions of mathematics As I tried to indicate in the discussion of *100 problems*, doing mathematics is an intensely personal and emotional enterprise. Instruction that fails to take this into account is necessarily dry and lifeless. It does justice neither to the students nor to mathematics.

Of course, this brief discussion does not do justice to any of the disciplines in Figure 1 In particular, I read between the lines of *100 problems* to present a mathematician's view of problem solving. For a deep and informative view of *The mathematical experience* as a whole, see Davis and Hersh [1981] It is a marvelous book that offers a charming and substantive discussion of what mathematics is all about Unfortunately I don't know of comparable books about psychology or mathematics education, but *PMI* and the two NCTM volumes stand well on their own as examplars of their fields, and will withstand any injustices I have done them. Another, far more important, qualification to this discussion is that it covers much too little: Figure 1 represents only a small part of the problem solving picture. Many more issues and perspectives should be included in it, among them (in no particular order)

— what might be called a "Western European" view of *P*sychology and *M*athematics *E*ducation, as reflected in the International Group for *PME*, or captured perhaps in the flavor of the journal *Educational Studies in Mathematics* In general this view has a more traditionally humanist flavor, a greater conscious debt to traditional psychology, and a greater familiarity with mathematics than its counterpart on this side of the Atlantic.
— the "Eastern European" perspective of same, as reflected in the 14-volume Soviet Studies [SMSG, 1969-1975], Vygotsky, Krutetskii, etc. The intertwining of social and cognitive perspectives may not produce good "science", but it sure has produced some interesting ideas
— a "radical constructivist" view of learning, as reflected in Papert's *Mindstorms*. You may or may not agree with what Papert has to say, but you should think seriously about it.
— anthropological and sociological studies that deal with the cultural context within which learning takes place [e.g. Rogoff and Lave, in press]; in general, the whole issue of the relationships among "real world" mathematics, "pure" mathematics, and "school" mathematics.
— "Foundations," both of disciplinary knowledge [e.g. Lakatos, 1977], and of *Personal knowledge* [Polanyi, 1958]. This latter category includes the emotional aspects of doing mathematics.

In sum: Those who worry about problem solving face a task of absolutely enormous proportions, calling for a synthesis of the best skills and knowledge from a collection of widely disparate disciplines. Is there hope? And if so, how to proceed?

I believe there is hope, and I am pleased to be able to point to a book that substantiates that belief: John Mason, Leone Burton, and Kaye Stacey's [1982] *Thinking mathematically* It goes without saying that you can't please all of the disciplines all of the time [see, e.g., Aesop, pp. 136-138], but this book takes artfully from a number of the disciplines mentioned above. Its approach is "hands on". The opening question (in a chapter entitled "everyone can start") asks:

> In a warehouse you obtain a 20% discount but you must pay a 15% sales tax. Which would you prefer to have calculated first, discount or tax?

Now this is not exactly high-powered mathematics, but I suspect it would throw a fair number of adults. The opening suggestion is that "the best way to start is by trying some specific cases", perhaps with an easy number like a price of $100 "Surprised by the result? Most people are, and it is that surprise which fuels mathematical thinking. Now, will the same thing happen for a price of say $120? Try it and see" Bit by bit, with a delicate discussion of "specializing" and "generalizing," the reader is led (but not by the nose!) to the general argument A bit later on, we find this problem.

> Take a square and draw a straight line across it. Draw several more lines in any arrangement so that the lines all cross the square, and the square is divided into several regions. The task is to colour the regions in such a way that adjacent regions are never coloured the same. (Regions having only one point in common are not considered adjacent.) How few different colours are needed to colour any such arrangement?

Now this problem has a bit more mathematical substance. (If mathematicians in the audience are still unsatisfied, how about determining the number of rectangles on an 8×8 chessboard, or perhaps on an $n \times m$ rectangle; how about Goldbach's conjecture?). But the discussion has even more. Again, it suggests possible paths of attack (only when you get stuck): "try colouring an arrangement. What do you KNOW? How is an arrangement constructed? What do you want to find? Be systematic!" A few pictures are drawn and colored. Then, based on the patterns they've seen, the authors make the obvious conjecture. And what do you know, It's ... WRONG! Closer examination reveals why it's wrong, and progressive refinements lead to a solution. How about that: making mistakes, and recovering from them, is part of doing mathematics! Later chapters deal with this in greater detail. We are told that "being stuck is an honourable state and an essential part of improving thinking" – so long as one learns from it. We learn about Getting Started (heuristics for inquiry), Getting Involved, Mulling, Keeping Going (the emotions of mathematics), Insight, Being Skeptical (what "proof" means), and Contemplating (reflection). And we learn that mathematics is open-ended: some of the problems lead to conjectures that may or may not be solvable, and we are invited to pursue them if we find them of interest.

Thinking mathematically demonstrates that one can do honest (also useful) *M*athematics as the school level. It deals directly with questions of personal knowledge. It is consistent with psychological theory, both at the "nuts and bolts" level (supporting the development of multiple representations for concepts, although perhaps not as directly as afficionados of *PMI* would like) and, when these can be taken somewhat for granted, at the "executive" or "control" level (in discussions of the "monitor"). It does all of this in a way that threatens neither student nor teacher, perhaps even encouraging them to work together. Of course I could find grounds for complaint, but I do not wish to cavil: this is a nice book.

Thus we have an existence proof for hope. What next? My feeling is that there is great potential in the connections, as yet unmade (well, barely made) among the constituencies mentioned during and after the discussion of Figure 1. In that discussion I focused on the dangers that arise from lack of communication among the relevant disciplines. The flip side of the coin deals with the advantages of meaningful contact among them. Some of my most fruitful moments as a researcher have been prompted by colleagues from other disciplines and other paradigms, who said "Wait a minute. Have you looked at it this way?"

One pipedream that I have is the following. Suppose that we could gather together a collection of the world's best mathematicians, mathematics educators, classroom teachers, cognitive psychologists, researchers in artificial intelligence, cultural anthropologists, epistemologists, etc. – a few of each, so that numbers didn't get out of hand. Suppose those people could (unobtrusively) observe some students in school and at home for a couple of days, and then be free to ask whatever questions (gather whatever protocols, perform whatever experiments) they liked for a few days. Then we lock all of those people in a room, with the assigned task of making sense of what they've seen. Candidates for "sensible statements" are those that are agreed upon by representatives of at least three disparate disciplines. Not only would we learn a great deal, but I suspect the participants would learn as much about their own disciplines.

Well, that was a pipedream. More realistically, we might convene a meeting where representatives of various perspectives spend a week trying to "explain" say, one twenty-minute long videotape of a problem solving session. (I hope to try this at ICME V.) I think the benefits could be enormous. Short of that, how about reserving 20% of our colloquia for speakers outside our disciplines (and taking them seriously)? There is much to be gained, and much fun to be had in the process.

Notes

[1] If you are not a mathematician, you may have had trouble following this argument. That's typical: the reader is expected to have to work at it. quite a bit, in fact

[2] The term is used advisedly. See Halmos' [1980] comments on the aesthetics of mathematical problems

[3] I should be more precise here. Of course there is no single such thing as "the mathematician's perspective," or the cognitive psychologist's or the mathematics educator's. All of these are remarkably variegated species. Yet there are views typically (or at least very frequently) expressed, that are at least familiar to people in those fields. That is the sense in which I use "the *x* perspective." My characterizations are, however, limited to this continent. Mathematics in Europe may be similar to mathematics here, but cognitive psychology and "math ed" are quite different

[4] I don't want to get sidetracked here into a discussion of "commonsense;" it is, clearly, a function of the social and psychological context of the times. Whatever naive commonsense may be, however, it alone would not be likely to produce the fads, fashions, and spectacular curricular flip-flops that we have witnessed in mathematics education over the past few decades – often influenced by contemporaneous psychological theory.

[5] For a somewhat radical view of Piaget's genetic epistemology (with which I have some sympathy) see Papert's *Mindstorms*, especially chapter 7

[6] It should not be inferred that such mathematics from the mathematician's perspective does not occur at the school level, or that it cannot be (has not been) modeled. One notable example is Alan Bundy's [1975] discussion of heuristic processes for solving elementary algebraic equations. I was surprised not to find that reference in *PMI*. There are also examples of interesting mathematics in Artificial Intelligence studies beyond the level of *PMI*, for example Doug Lenat's [1979] AM and [1981] EURISKO programs.

[7] I regret that there is not the space here for mitigatory comments, for what I say may come across as unremittingly negative. My perspective is not. A look at the evolution of the *Journal for Research in Mathematics Education* since its inception reflects the growth of the discipline, and provides reasonable grounds for optimism

[8] Since I have argued this point extensively elsewhere, I will not repeat the argument here. My two "in press" entries in the bibliography provide some theoretical mechanisms for dealing with "other than heuristic" components of problem solving

[9] See, for example, the brief comments about this in [Schoenfeld 1982]

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