Experiencing Mathematics
What do we do, when we do mathematics?

Reuben Hersh

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Reuben Hersh
To my students, my mentors, my collaborators and my colleagues.
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Preface

The 30 articles I have selected are arranged in three groups. The group named “Mostly for the right hand” contains reporting and analysis of mathematical life and work. The group named “Mostly for the left hand” is more personal. Some of its articles are satirical or fantastic. There’s also a small group of book reviews.

The general introduction has three parts: a manifesto, a self-introduction, and a chronology. The “manifesto” explains mathematical practice, with references to articles where these views are developed. The “self-introduction” tells, where did I come from? What did I do? The “chronology” tells how my philosophical ideas evolved and developed, again referring to articles below.

Then comes an amusing recent contribution to elementary mathematics, an annotated bibliography of my research, a couple of poems, a curriculum vitae and an annotated list of my mathematics research articles, and finally a virtually complete list of all my published articles.

To begin with, after the acknowledgments, here are two short pieces, meant to set the tone and flavor of what follows. They were both written in 1979, over 30 years ago.
Credits

The AMS gratefully acknowledges the kindness of these individuals, institutions, and publishers in granting the following permissions.

Abaris Books
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American Mathematical Society

Cambridge Scholars Publishing

Cambridge University Mathematical Society

College Publications, London

Department of Computer Science, University of Utah
Figure that appears in “True Facts About Imaginary Objects”, The Mathematical Experience, Philip J. Davis and Reuben Hersh, Birkhäuser, 1981, p. 408.


Springer Science+Business Media


Acknowledgments

The support of Gian-Carlo Rota was crucial to me. Philip Kitcher, Hao Wang, and Carlo Cellucci are philosophers who encouraged me. Thanks to philosophers at the Vrije Universiteit in Brussels and at the Philosophy Department of the University of Rome, for invitations to speak. The friendship and collaboration of Martin Davis has been a great benefit. I had friendly interactions with philosophers at the University of New Mexico. I was influenced by Imre Lakatos, Leslie White, George Polya, Peter Lax, John Dewey and Vera John-Steiner, and by the support and/or opposition of Joe Auslander, Jody Azzouni, Bill Beyer, John Bova, John Burgess, Chandler Davis, Philip J. Davis, Harold Edwards, Helena Eilstein, Jim Ellison, Dick Epstein, Paul Ernest, Sol Feferman, Bonnie Gold, Sam Goldberg, Russell Goodman, Klaus Heinemann, Adrian Johnston, George Lakoff, Paul Livingston, Ina Matte, Carlos Motta, Ed Nelson, Bob Osserman, Ulf Persson, Bharath Sriraman, Tom Tymoczko, and Kristin Umland.
Overture

Up till about five years ago, I was a normal mathematician. I didn’t do risky and unorthodox things, like writing a book such as this. I had my “field”—partial differential equations—and I stayed in it, or at most wandered across its borders into an adjacent field. My serious thinking, my real intellectual life, used categories and evaluative modes that I had absorbed years before, in my training as a graduate student. Because I did not stray far from these modes and categories, I was only dimly conscious of them. They were part of the way I saw the world, not part of the world I was looking at.

My advancement was dependent on my research and publication in my field. That is to say, there were important rewards for mastering the outlook and ways of thought shared by those whose training was similar to mine, the other workers in the field. Their judgment would decide the value of what I did. No one else would be qualified to do so; and it is very doubtful that anyone else would have been interested in doing so. To liberate myself from this outlook—that is, to recognize it, to become aware that it was only one of many possible ways of looking at the world, to be able to put it on or off by choice, to compare it and evaluate it with other ways of looking at the world—none of this was required by my job or my career. On the contrary, such unorthodox and dubious adventures would have seemed at best a foolish waste of precious time—at worst, a disreputable dabbling with shady and suspect ventures such as psychology, sociology, or philosophy.

The fact is, though, that I have come to a point where my wonderment and fascination with the meaning and purpose, if any, of this strange activity we call mathematics is equal to, sometimes even stronger than, my fascination with actually doing mathematics. I find mathematics an infinitely complex and mysterious world; exploring it is an addiction from which I hope never to be cured. In this, I am a mathematician like all others. But in addition, I have developed a second half, an Other, who watches this mathematician with amazement, and is even more fascinated that such a strange creature and such a strange activity have come into the world, and persisted for thousands of years.

I trace its beginnings to the day when I came at last to teach a course called Foundations of Mathematics. This is a course intended primarily for mathematics majors, at the upper division (junior or senior) level. My purpose in teaching this course, as in the others I had taught over the years, was to learn the material myself. At that time I knew that there was a history of controversy about the foundations. I knew that there had been three major “schools”; the logicists associated with Bertrand Russell, the formalists led by David Hilbert, and the constructivist school of L. E. J. Brouwer. I had a general idea of the teaching of each of these three schools. But I had no idea which one I agreed with, if any, and I had only a vague
idea of what had become of the three schools in the half century since their founders were active.

I hoped that by teaching the course I would have the opportunity to read and study about the foundations of mathematics, and ultimately to clarify my own views of those parts which were controversial. I did not expect to become a researcher in the foundations of mathematics, any more than I became a number theorist after teaching number theory.

Since my interest in the foundations was philosophical rather than technical, I tried to plan the course so that it could be attended by interested students with no special requirements or prerequisites; in particular, I hoped to attract philosophy students, and mathematics education students. As it happened there were a few such students; there were also students from electrical engineering, from computer science, and other fields. Still, the mathematics students were the majority. I found a couple of good-looking textbooks, and plunged in.

In standing before a mixed class of mathematics, education, and philosophy students, to lecture on the foundations of mathematics, I found myself in a new and strange situation. I had been teaching mathematics for some 15 years, at all levels and in many different topics, but in all my other courses the job was not to talk about mathematics, it was to do it. Here my purpose was not to do it, but to talk about it. It was different and frightening.

As the semester progressed, it became clear to me that this time it was going to be a different story. The course was a success in one sense, for there was a lot of interesting material, lots of chances for stimulating discussions and independent study, lots of things for me to learn that I had never looked at before. But in another sense, I saw that my project was hopeless.

In an ordinary mathematics class, the program is fairly clear cut. We have problems to solve, or a method of calculation to explain, or a theorem to prove. The main work to be done will be in writing, usually on the blackboard. If the problems are solved, the theorems proved, or the calculations completed, then teacher and class know that they have completed the daily task. Of course, even in this ordinary mathematical setting, there is always the possibility or likelihood of something unexpected happening. An unforeseen difficulty, an unexpected question from a student, can cause the progress of the class to deviate from what the instructor had intended. Still, one knew where one was supposed to be going; one also knew that the main thing was what you wrote down. As to spoken words, either from the class or from the teacher, they were important insofar as they helped to communicate the import of what was written.

In opening my course on the foundations of mathematics, I formulated the questions which I believed were central, and which I hoped we could answer or at least clarify by the end of the semester.

What is a number? What is a set? What is a proof? What do we know in mathematics, and how do we know it? What is “mathematical rigor”? What is “mathematical intuition”?

As I formulated these questions, I realized that I didn’t know the answers. Of course, this was not surprising, for such vague questions, “philosophical” questions, should not be expected to have clearcut answers of the kind we look for in mathematics. There will always be differences of opinion about questions such as these.
But what bothered me was that I didn’t know what my own opinion was. What was worse, I didn’t have a basis, a criterion on which to evaluate different opinions, to advocate or attack one viewpoint or another.

I started to talk to other mathematicians about proof, knowledge, and reality in mathematics and I found that my situation of confused uncertainty was typical. But I also found a remarkable thirst for conversation and discussion about our private experiences and inner beliefs.

This book is part of the outcome of these years of pondering, listening, and arguing.
The Ideal Mathematician
(with Philip J. Davis)

We will construct a portrait of the “ideal mathematician.” By this we do not mean the perfect mathematician, the mathematician without defect or limitation. Rather, we mean to describe the most mathematician-like mathematician, as one might describe the ideal thoroughbred greyhound, or the ideal thirteenth-century monk. We will try to construct an impossibly pure specimen, in order to exhibit the paradoxical and problematical aspects of the mathematician’s role. In particular, we want to display clearly the discrepancy between the actual work and activity of the mathematician and his own perception of his work and activity.

The ideal mathematician’s work is intelligible only to a small group of specialists, numbering a few dozen or at most a few hundred. This group has existed only for a few decades, and there is every possibility that it may become extinct in another few decades. However, the mathematician regards his work as part of the very structure of the world, containing truths which are valid forever, from the beginning of time, even in the most remote corner of the universe.

He rests his faith on rigorous proof; he believes that the difference between a correct proof and an incorrect one is an unmistakable and decisive difference. He can think of no condemnation more damning than to say of a student, “He doesn’t even know what a proof is.” Yet he is able to give no coherent explanation of what is meant by rigor, or what is required to make a proof rigorous. In his own work, the line between complete and incomplete proof is always somewhat fuzzy, and often controversial.

To talk about the ideal mathematician at all, we must have a name for his “field,” his subject. Let’s call it, for instance, “non-Riemannian hypersquares.”

He is labelled by his field, by how much he publishes, and especially by whose work he uses, and by whose taste he follows in his choice of problems.

He studies objects whose existence is unsuspected by all except a handful of his fellows. Indeed, if one who is not an initiate asks him what he studies, he is incapable of showing or telling what it is. It is necessary to go through an arduous apprenticeship of several years to understand the theory to which he is devoted. Only then would one’s mind be prepared to receive his explanation of what he is studying. Short of that, one could be given a “definition,” which would be so recondite as to defeat all attempts at comprehension.

The objects which our mathematician studies were unknown before the twentieth century; most likely, they were unknown even thirty years ago. Today they are the chief interest in life for a few dozen (at most, a few hundred) of his comrades. He and his comrades do not doubt, however, that non-Riemannian hypersquares have a real existence as definite and objective as that of the Rock of Gibraltar or
Halley’s Comet. In fact, the proof of the existence of non-Riemannian hypersquares is one of their main achievements, whereas the existence of the Rock of Gibraltar is very probable, but not rigorously proved.

It has never occurred to him to question what the word “exist” means here. One could try to discover its meaning by watching him at work and observing what the word “exist” signifies operationally.

In any case, for him the non-Riemannian hypersquare exists, and he pursues it with passionate devotion. He spends all his days in contemplating it. His life is successful to the extent that he can discover new facts about it.

He finds it difficult to establish meaningful conversation with that large portion of humanity that has never heard of a non-Riemannian hypersquare. This creates grave difficulties for him; there are two colleagues in his department who know something about non-Riemannian hypersquares, but one of them is on sabbatical, and the other is much more interested in non-Eulerian semirings. He goes to conferences, and on summer visits to colleagues, to meet people who talk his language, who can appreciate his work and whose recognition, approval, and admiration are the only meaningful rewards he can ever hope for.

At the conferences, the principal topic is usually “the decision problem” (or perhaps “the construction problem” or “the classification problem”) for non-Riemannian hypersquares. This problem was first stated by Professor Nameless, the founder of the theory of non-Riemannian hypersquares. It is important because Professor Nameless stated it and gave a partial solution which, unfortunately, no one but Professor Nameless was ever able to understand. Since Professor Nameless’ day, all the best non-Riemannian hypersquarers have worked on the problem, obtaining many partial results. Thus the problem has acquired great prestige.

Our hero often dreams he has solved it. He has twice convinced himself during waking hours that he had solved it but, both times, a gap in his reasoning was discovered by other non-Riemannian devotees, and the problem remains open. In the meantime, he continues to discover new and interesting facts about the non-Riemannian hypersquares. To his fellow experts, he communicates these results in a casual shorthand. “If you apply a tangential mollifier to the left quasi-martingale, you can get an estimate better than quadratic, so the convergence in the Bergstein theorem turns out to be of the same order as the degree of approximation in the Steinberg theorem.”

This breezy style is not to be found in his published writings. There he piles up formalism on top of formalism. Three pages of definitions are followed by seven lemmas and, finally, a theorem whose hypotheses take half a page to state, while its proof reduces essentially to “Apply Lemmas 1–7 to definitions A–H.”

His writing follows an unbreakable convention: to conceal any sign that the author or the intended reader is a human being. It gives the impression that, from the stated definitions, the desired results follow infallibly by a purely mechanical procedure. In fact, no computing machine has ever been built that could accept his definitions as inputs. To read his proofs, one must be privy to a whole subculture of motivations, standard arguments and examples, habits of thought and agreed-upon modes of reasoning. The intended readers (all twelve of them) can decode the formal presentation, detect the new idea hidden in lemma 4, ignore the routine and uninteresting calculations of lemmas 1, 2, 3, 5, 6, 7, and see what the author is doing and why he does it. But for the noninitiate, this is a cipher that will never
yield its secret. If (heaven forbid) the fraternity of non-Riemannian hypersquarers should ever die out, our hero’s writings would become less translatable than those of the Maya.

The difficulties of communication emerged vividly when the ideal mathematician received a visit from a public information officer of the University.

**P.I.O.:** I appreciate your taking time to talk to me. Mathematics was always my worst subject.

**I.M.:** That’s O.K. You’ve got your job to do.

**P.I.O.:** I was given the assignment of writing a press release about the renewal of your grant. The usual thing would be a one-sentence item, “Professor X received a grant of Y dollars to continue his research on the decision problem for non-Riemannian hypersquares.” But I thought it would be a good challenge for me to try and give people a better idea about what your work really involves. First of all, what is a hypersquare?

**I.M.:** I hate to say this, but the truth is if I told you what it is, you would think I was trying to put you down and make you feel stupid. The definition is really somewhat technical, and it just wouldn’t mean anything at all to most people.

**P.I.O.:** Would it be something engineers or physicists would know about?

**I.M.:** No. Well, maybe a few theoretical physicists. Very few.

**P.I.O.:** Even if you can’t give me the real definition, can’t you give me some idea of the general nature and purpose of your work?

**I.M.:** All right, I’ll try. Consider a smooth function $f$ on a measure space $\Omega$ taking its value in a sheaf of germs equipped with a convergence structure of saturated type. In the simplest case...

**P.I.O.:** Perhaps I’m asking the wrong questions. Can you tell me something about the applications of your research?

**I.M.:** Applications?

**P.I.O.:** Yes, applications.

**I.M.:** I’ve been told that some attempts have been made to use non-Riemannian hypersquares as models for elementary particles in nuclear physics. I don’t know if any progress was made.

**P.I.O.:** Have there been any major breakthroughs recently in your area? Any exciting new results that people are talking about?

**I.M.:** Sure, there’s the Steinberg-Bergstein paper. That’s the biggest advance in at least five years.

**P.I.O.:** What did they do?

**I.M.:** I can’t tell you.

**P.I.O.:** I see. Do you feel there is adequate support in research for your field?

**I.M.:** Adequate? It’s hardly lip service. Some of the best young people in the field are being denied research support. I have no doubt that with extra support we could be making much more rapid progress on the decision problem.

**P.I.O.:** Do you see any way that the work in your area could lead to anything that would be understandable to the ordinary citizen of this country?

**I.M.:** No.

**P.I.O.:** How about engineers or scientists?

**I.M.:** I doubt it very much.

**P.I.O.:** Among pure mathematicians, would the majority be interested in or acquainted with your work?
I.M.: No, it would be a small minority.
P.I.O.: Is there anything at all that you would like to say about your work?
I.M.: Just the usual one sentence will be fine.
P.I.O.: Don’t you want the public to sympathize with your work and support it?
I.M.: Sure, but not if it means debasing myself.
P.I.O.: Debasing yourself?
I.M.: Getting involved in public relations gimmicks, that sort of thing.
P.I.O.: I see. Well, thanks again for your time.
I.M.: That’s O.K. You’ve got a job to do.

Well, a public relations officer. What can one expect? Let’s see how our ideal mathematician made out with a student who came to him with a strange question.

Student: Sir, what is a mathematical proof?
I.M.: You don’t know that? What year are you in?
Student: Third-year graduate.
I.M.: Incredible! A proof is what you’ve been watching me do at the board three times a week for three years! That’s what a proof is.
Student: Sorry, sir, I should have explained. I’m in philosophy, not math. I’ve never taken your course.
I.M.: Oh! Well, in that case—you have taken some math, haven’t you? You know the proof of the fundamental theorem of calculus—or the fundamental theorem of algebra?
Student: I’ve seen arguments in geometry and algebra and calculus that were called proofs. What I’m asking you for isn’t examples of proof; it’s a definition of proof. Otherwise, how can I tell what examples are correct?
I.M.: Well, this whole thing was cleared up by the logician Tarski, I guess, and some others, maybe Russell or Peano. Anyhow, what you do is, you write down the axioms of your theory in a formal language with a given list of symbols or alphabet. Then you write down the hypothesis of your theorem in the same symbolism. Then you show that you can transform the hypothesis step by step, using the rules of logic, till you get the conclusion. That’s a proof.
Student: Really? That’s amazing! I’ve taken elementary and advanced calculus, basic algebra, and topology, and I’ve never seen that done.
I.M.: Oh, of course no one ever really does it. It would take forever! You just show that you could do it, that’s sufficient.
Student: But even that doesn’t sound like what was done in my courses and textbooks. So mathematicians don’t really do proofs, after all.
I.M.: Of course we do! If a theorem isn’t proved, it’s nothing.
Student: Then what is a proof? If it’s this thing with a formal language and transforming formulas, nobody ever proves anything. Do you have to know all about formal languages and formal logic before you can do a mathematical proof?
I.M.: Of course not! The less you know, the better. That stuff is all abstract nonsense anyway.
Student: Then really what is a proof?
I.M.: Well, it’s an argument that convinces someone who knows the subject.
Student: Someone who knows the subject? Then the definition of proof is subjective; it depends on particular persons. Before I can decide if something is a proof,
I have to decide who the experts are. What does that have to do with proving things?

*I.M.*: No, no. There’s nothing subjective about it! Everybody knows what a proof is. Just read some books, take courses from a competent mathematician, and you’ll catch on.

**Student:** Are you sure?

*I.M.*: Well, it is possible that you won’t, if you don’t have any aptitude for it. That can happen, too.

**Student:** Then you decide what a proof is, and if I don’t learn to decide in the same way, you decide I don’t have any aptitude.

*I.M.*: If not me, then who?

Then the ideal mathematician met a positivist philosopher.

**P.P.**: This Platonism of yours is rather incredible. The silliest undergraduate knows enough not to multiply entities, and here you’ve got not just a handful, you’ve got them in uncountable infinities! And nobody knows about them but you and your pals! Who do you think you’re kidding?

*I.M.*: I’m not interested in philosophy, I’m a mathematician.

**P.P.**: You’re as bad as that character in Molière who didn’t know he was talking prose! You’ve been committing philosophical nonsense with your “rigorous proofs of existence.” Don’t you know that what exists has to be observed, or at least observable?

*I.M.*: Look, I don’t have time to get into philosophical controversies. Frankly, I doubt that you people know what you’re talking about; otherwise you could state it in a precise form so that I could understand it and check your argument. As far as my being a Platonist, that’s just a handy figure of speech. I never thought hypersquares existed. When I say they do, all I mean is that the axioms for a hypersquare possess a model. In other words, no formal contradiction can be deduced from them, and so, in the normal mathematical fashion, we are free to postulate their existence. The whole thing doesn’t really mean anything, it’s just a game, like chess, that we play with axioms and rules of inference.

**P.P.**: Well, I didn’t mean to be too hard on you. I’m sure it helps you in your research to imagine you’re talking about something real.

*I.M.*: I’m not a philosopher, philosophy bores me. You argue, argue and never get anywhere. My job is to prove theorems, not to worry about what they mean.

The ideal mathematician feels prepared, if the occasion should arise, to meet an extragalactic intelligence. His first effort to communicate would be to write down (or otherwise transmit) the first few hundred digits in the binary expansion of $\pi$.

He regards it as obvious that any intelligence capable of intergalactic communication would be mathematical and that it makes sense to talk about mathematical intelligence apart from the thoughts and actions of human beings. Moreover, he regards it as obvious that binary representation and the real number $\pi$ are both part of the intrinsic order of the universe.

He will admit that neither of them is a natural object, but he will insist that they are discovered, not invented. Their discovery, in something like the form in which we know them, is inevitable if one rises far enough above the primordial slime to communicate with other galaxies (or even with other solar systems).

The following dialogue once took place between the ideal mathematician and a skeptical classicist.
**S.C.** You believe in your numbers and curves just as Christian missionaries believed in their crucifixes. If a missionary had gone to the moon in 1500, he would have been waving his crucifix to show the moon-men that he was a Christian, and expecting them to have their own symbol to wave back. You’re even more arrogant about your expansion of pi.

**I.M.** Arrogant? It’s been checked and rechecked, to 100,000 places!

**S.C.** I’ve seen how little you have to say even to an American mathematician who doesn’t know your game with hypersquares. You don’t get to first base trying to communicate with a theoretical physicist; you can’t read his papers any more than he can read yours. The research papers in your own field written before 1910 are as dead to you as Tutankhamen’s will. What reason in the world is there to think that you could communicate with an extragalactic intelligence?

**I.M.** If not me, then who else?

**S.C.** Anybody else! Wouldn’t life and death, love and hate, joy and despair be messages more likely to be universal than a dry pedantic formula that nobody but you and a few hundred of your type will know from a hen-scratch in a farmyard?

**I.M.** The reason that my formulas are appropriate for intergalactic communication is the same reason they are not very suitable for terrestrial communication. Their content is not earthbound. It is free of the specifically human.

**S.C.** I don’t suppose the missionary would have said quite that about his crucifix, but probably something rather close, and certainly no less absurd and pretentious.

The foregoing sketches are not meant to be malicious; indeed, they would apply to the present authors. But it is a too obvious and therefore easily forgotten fact that mathematical work, which, no doubt as a result of long familiarity, the mathematician takes for granted, is a mysterious, almost inexplicable phenomenon from the point of view of the outsider. In this case, the outsider could be a layman, a fellow academic, or even a scientist who uses mathematics in his own work.

The mathematician usually assumes that his own view of himself is the only one that need be considered. Would we allow the same claim to any other esoteric fraternity? Or would a dispassionate description of its activities by an observant, informed outsider be more reliable than that of a participant who may be incapable of noticing, not to say questioning, the beliefs of his coterie?

Mathematicians know that they are studying an objective reality. To an outsider, they seem to be engaged in an esoteric communion with themselves and a small clique of friends. How could we as mathematicians prove to a skeptical outsider that our theorems have meaning in the world outside our own fraternity?

If such a person accepts our discipline, and goes through two or three years of graduate study in mathematics, he absorbs our way of thinking, and is no longer the critical outsider he once was. In the same way, a critic of Scientology who underwent several years of “study” under “recognized authorities” in Scientology might well emerge a believer instead of a critic.

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*cf. the description of Coronado’s expedition to Cibola, in 1540: “...there were about eighty horsemen in the vanguard besides twenty-five or thirty foot and a large number of Indian allies. In the party went all the priests, since none of them wished to remain behind with the army. It was their part to deal with the friendly Indians whom they might encounter, and they especially were bearers of the Cross, a symbol which...had already come to exert an influence over the natives on the way” (H. E. Bolton, Coronado, University of New Mexico Press, 1949).
If the student is unable to absorb our way of thinking, we flunk him out, of course. If he gets through our obstacle course and then decides that our arguments are unclear or incorrect, we dismiss him as a crank, crackpot, or misfit.

Of course, none of this proves that we are not correct in our self-perception that we have a reliable method for discovering objective truths. But we must pause to realize that, outside our coterie, much of what we do is incomprehensible. There is no way we could convince a self-confident skeptic that the things we are talking about make sense, let alone “exist.”
Manifesto

When I realized that I didn’t have a clear picture of what I was actually doing as a mathematician, I looked for help on the library shelves. I was surprised and disappointed at how little help I found. What was helpful usually had been written by a fellow mathematician, not by a philosopher.

I have tried, little by little, to see as clearly as possible what I am actually doing—what is my own “mathematical experience,” so to speak. Here in this manifesto I state some conclusions I have come to, and point to articles below where these conclusions are elaborated and defended.

...Mathematics isn’t a fiction, it’s a reality.

...Mathematics is not primarily syntactic deductions from meaningless sentences made out of undefined terms. It is meaningful, understandable, communicable.

...Mathematics is not “out there,” in an abstract realm apart from human consciousness or material reality. It is “down here,” in our individual minds and in our shared consciousness.

“Certain kinds of ideas (concepts, notions, conceptions, and so forth) have science-like quality. They have the rigidity, the reproducibility, of physical science. They yield reproducible results, independent of particular investigators. Such kinds of ideas are important enough to have a name. Study of the lawful, predictable parts of the physical world has a name, ‘physics.’ Study of the lawful, predictable parts of the social-conceptual world also has a name, ‘mathematics.’ A world of ideas exists, created by human beings, existing in their shared consciousness. These ideas have objective properties, in the same sense that material objects have objective properties. The construction of proof and counter-example is one method of verifying the properties of these ideas. This branch of knowledge is called mathematics.”

(What is mathematics, really?, Page 19)

...A mathematical entity is a concept, a shared thought. Once you have acquired it, you have it available, for inspection or manipulation. If you understand it correctly (as a student, or as a professional) your “mental model” of that entity, your personal representative of it, matches those of others who understand it correctly. (As is verified by giving the same answers to test questions.) The concept, the cultural entity, is nothing other than the collection of the mutually congruent personal representatives, the “mental models”, possessed by those participating in the mathematical culture.

The distinctive characteristic of mathematical concepts, compared to other shared concepts, is their near-complete unanimity. In this respect they are like the experimental facts of an empirical science. All those who have correctly mastered or internalized a mathematical concept (according to the standard judgments carried out by the mathematical community) will agree on the properties of that
concept, after due and necessary communication and conversation. This unanimous agreement and acceptance is generally attained by the procedure called “proof”—reasoning on the properties of mathematical entities, based on direct observation of one’s own models of such entities. I call it “mathematicians’ proof,” to recognize its distinctness from the “formal proofs” taught in logic. This distinction is elaborated below in *To Establish New Mathematics, We Use Our Mental Models and Build on Established Mathematics*.

A new branch of learning is accepted and absorbed into established mathematics (as happened to probability theory, years ago, and later to proof theory and model theory in logic, and more recently to information theory and cryptography and network theory), if that new branch has conclusive, compelling arguments accepted by everyone who is qualified, who understands what it is all about, who possesses the concept (See *Definition of Mathematics*).

The individual mental aspect means that once you’ve mastered a mathematical concept, it’s yours—to use, to inspect, to consider, to turn upside down and inside out. That’s how mathematical research is done. That’s why mathematicians’ proof is convincing and compelling. This point is elaborated below in *How Mathematicians Convince Each Other or “The Kingdom of Math is Within You”*.

This notion of the individual or personal mental model of a mathematical concept enables us to clarify an important issue in mathematics education. What do we mean by saying a student “understands” a concept? There is a contrast between “really understanding” and “just going through the motions.” Going through the motions—carrying out an algorithm correctly—may be acceptable for a passing grade. Really understanding deserves an A. But what do we mean by “really understanding”? What we really mean, I would argue, is possessing an adequate mental model of the concept. Possessing an adequate mental model is displayed by answering questions the student hasn’t seen before, or by being able to “play around” with a concept, to connect it with other concepts or to modify it sensibly. This is sometimes called “learning to think like a mathematician.” (This educational aspect of the notion of personal or individual mental model of a mathematical concept is not elaborated in any of the papers collected here, or already published elsewhere. It may become the subject of a future paper.)

This individual mental aspect has two essential sub-aspects. One aspect is consciousness. When I’m thinking about a geometry problem, I’m conscious of my thoughts. But an insight, a previously unseen connection or possibility, can arise in some hidden, obscure way, from my subconscious, my “intuition”. Further “down below” are the basis of my consciousness, namely, my electro-chemical brain and nerve processes. When I am thinking about mathematical questions, I have no concern with that deep level. But when I am philosophizing about mathematical practice, I keep it in mind, I recognize it. This helps me to recognize and accept that *mathematical thoughts and ideas actually exist; they are real entities*. They are embodied, they are made possible by the thinker’s flesh and blood, by his/her functioning brain and nervous system. We experience our own thoughts in the most direct and immediate way conceivable. At the same time, they are grounded in biological and physical reality. (See *How Mathematicians Convince Each Other or “The Kingdom of Math is Within You”*.)

A thought as experienced directly, subjectively, and a thought as an electro-chemical event in my nervous system, are not two different things. They are two
different ways of viewing the same thing, different aspects or versions of a single entity. (See Mathematical Intuition (Poincaré, Polya, Dewey); To Establish New Mathematics, We Use Our Mental Models and Build on Established Mathematics; and How Mathematicians Convince Each Other or The Kingdom of Math is Within You”.)

Mathematical knowledge being an activity of fallible flesh and blood, it is fallible and tentative. Most fallible and tentative when the knowledge is recent and complex, less so when it is ancient and simple.

Mathematicians’ proof is the principal way a piece of mathematics becomes part of established mathematics. Its proof is a warrant for asserting it in other proofs. (See Mathematical Intuition (Poincaré, Polya, Dewey).) This view connects the philosophy of mathematical practice with the pragmatism of John Dewey.
Self-introduction

My name is Reuben Hersh. I am 86 years old. I live in Santa Fe, New Mexico, in an old adobe house with my partner, Veronka (the psycholinguist Vera John-Steiner).

I was born in New York City in 1927. My father, Philip (originally Fishel) came from the Polish town of Pabieniece, a suburb of the industrial city of Lodz. My mother, Mildred (originally Malke), was from a forest village called Betsnivets, in the southwestern Ukraine. The nearest notable town is Letichev, the nearest actual city is Vinnitsa.

Malke and Fishel met in New York after the end of the First World War. Before she married, my mother supported herself and her ailing sister Miriam by operating a sewing machine in a curtain factory. She was a unionist, a socialist sympathizer. Fishel/Philip was a firm Zionist. He had been in Palestine for a couple of years before coming to the U.S. Arriving almost penniless, he found on Delancey Street, in lower Manhattan, a jobber in dry goods, Sharofsky and Oremland, who would trust him with a load of sheets, towels and pillow cases. He carried these on his back, to sell on credit to Italian tenement dwellers on the Lower East Side.

This kind of self-employment used to be called “customer peddler”. “First you try to sell to them, then you try to collect from them. A dollar down, a dollar when you catch me.” In a few years, my father became a businessman, employing a few peddlers and collectors. They went house to house, selling dry goods and other things, to people, mostly Black, who might not get credit from regular stores.

Until 1940, we lived on Nereid Avenue in the Bronx, way up at the very end of the White Plains Road line of the IRT subway. We had our own house, and rented to tenants upstairs. Everyone else in the neighborhood was Sicilian. Up the hill half a block away was a Catholic boys’ high school, Mount Saint Michael’s. In the basement of our house were my father’s office and storeroom. At the time I started high school, we moved to a neighboring suburb, Mount Vernon, where my father opened a store, while still keeping his customer peddler enterprise.

I was eager to escape from home, and left for college in 1943. It was during the Second World War. I longed to go fight Hitler, but before I was old enough to enlist, the war ended. As a would-be fighter against Fascism, I joined the Communist Party of the United States (CPUSA) in the fall of 1945, and remained a party member until 1957, when I read in the New York Times Nikita Khrushchev’s “secret” speech to the Presidium of the Communist Party of the Soviet Union (CPSU).

Before World War II, my mother mailed food and clothing to her parents back in the Ukraine. They had moved away from their remote village to Vinnitsa, where my grandfather managed a small factory of some kind. (He had actually received a profound training in Torah, from my great grandfather, the head of a yeshiva in Vinnitsa.)
My father Philip, one of eleven brothers and sisters by two different mothers, never mentioned his family in Poland, until one day in 1947, when he took me aside to read a list of 32 names, the names of his brothers, sisters, nephews and nieces, who had been murdered by the Nazis.

My mother didn’t talk about her family’s fate. Only years later did I learn that her mother and father, grandparents whom I never knew, had survived the German occupation by hiding in their apartment in Vinnitsa. Then they were murdered, by some young Ukrainian thug who wanted their apartment for his girl friend.

After college I did an 18-months enlistment in the U.S. Army. In 1948 I married my first wife, Phyllis Falchook. I worked for four years at the Scientific American magazine, rising from mail clerk to editorial assistant. For four years after that, I worked in machine shops in the New York metropolitan area, as drill press operator, engine lathe hand and second class machinist. Our son Daniel was born in 1956, our daughter Eva Stephanie three years later in 1959. In 1956 I suffered a serious accident at work, losing half of my right thumb. I decided to stop doing machine shop work, before I lost any more fingers.

It took me months to decide what to do next. I had a wife and child to feed, as well as myself. I made a major philosophical decision. Instead of devoting my life to changing the world, I could just try to earn a living doing something I enjoyed!

After a while, I remembered that before becoming an English Lit major in college, I had seriously considered majoring in math. But a deadly dull second semester of calculus convinced me not to take any more math courses.

Long before that, back in high school, I had amused myself by playing around with arithmetic. Fascinating things happen if in a multi-digit number (a number in the teens or hundreds or thousands) you add up all the digits. If necessary, repeat that operation, till you end up with a single digit between 1 and 9. (In my mind I called this number the “digital sum”.) Then, when you add or multiply two numbers, their sum or product has a “digital sum,” which is the same as the sum or product of the digital sums of the two numbers you started with! I even could PROVE that this is so. Later on, I learned that this is all a very old story, known for centuries as “casting out nines.” Anyhow, I had discovered it on my own, never even talking about it to anyone else.

So I always liked math, but never thought of it as a life work, because I had to go fight Fascism. But by the time I cut off half my right thumb, I realized that my efforts to fight Fascism had been deluded and ineffective.

While a machine shop wage slave, I had come across in the public library an interesting book, What Is Mathematics?, by two authors, Richard Courant and Herbert Robbins. I didn’t know who they were, but I couldn’t bear to give this book back to the library. I lied, claiming I had lost the book, and paid whatever fine the library required. To this day, I still have that library copy! I never dared to tell this little story to Richard Courant, when, years later, as a graduate student at NYU, I was assigned to be his copy editor on the English translation of Volume 2 of Methods of Mathematical Physics (universally referred to as “Courant-Hilbert”).

In 1957, the thought of going to graduate school in math was scary. But I consider it a rule of life: if I am scared to do something, then that is what I must go and do. I got some workmen’s compensation for my lost half-thumb, collected unemployment insurance as long as possible by pretending to look for a job, and got admitted as a math grad student at New York University. In 1962,
under the supervision of Peter Lax, I received a Ph.D., for finding and solving the most general well-posed half-space initial-boundary value problem, for a constant-coefficient first-order system of hyperbolic partial differential equations (PDE1, in the List of Articles below). I spent two years as an instructor at Stanford, where I wrote a *Scientific American* article, jointly with Paul Cohen, about his sensational proof that the Continuum Hypothesis is undecidable from the Zermelo-Frankel axioms of set theory. Then I found steady work at the University of New Mexico, in Albuquerque. I taught there from 1964 until 2011 (part time after 1995). I also taught briefly at Santa Fe Prep and at St. Johns College, and on leave, at Berkeley, Brown, and the *Politecnico* in Mexico City.

For a few years I worked on variations and extensions of my mixed initial-boundary value problem. Then a young probabilist at UNM named Richard J. Griego challenged me to work on the interactions of probability and partial differential equations. We wrote a joint expository paper for *Scientific American*, about Brownian motion and potential theory. Then, thanks to another colleague, Nat Friedman, we came across a virtually unpublished example by Mark Kac, where a *hyperbolic* equation, the wave equation with an additional diffusive term, is solved by a probabilistic model. This was a special case of something much more general—abstract evolution, with random change of the law of evolution. Peter Lax suggested a name for our new theory: “Random evolutions.”

Explicit formulas could be written down! The central limit theorem of probability could be used to prove singular perturbation theorems! (RE2)

It took ten years to work all this out, with several collaborators—Griego, Mark Pinsky, George Papanicolaou, Bob Cogburn (RE3-6). Our new theory was interesting and useful. Most satisfying was this new rigorous, physically meaningful result: “In the limit of small mean free path, the expected positions of particles changing speed and direction by random collisions converge to solutions of a diffusion equation.” My expository article in the *Mathematical Intelligencer* references over 100 papers. (RE8) The Ukrainian Academy of Sciences invited me to a meeting on random evolutions at its resort on the Black Sea. Its organizer, Anatoliy Swishchuk, took me to visit my mother’s birthplace, Betsnivets.

This work led naturally into a whole other area of fruitful research, in which I transform the solution of a given initial-value problem, by an explicit formula, to the solution of some other, simpler or more complex problem. For instance, second-order equation to first-order or vice versa; singular equation to regular; equation with a parameter to one without the parameter; Goursat problem to Cauchy problem. I call this “the method of transmutations.” (PDE12)

Here is a striking fact about group theory and constant-coefficient evolutionary pde’s:

Let $P(d/dt, d/dx_j) = 0$ be a linear system of constant co-efficient partial differential equations, possibly hyperbolic or parabolic, for which the “concrete” initial-value problem (“the Cauchy problem”) is well-posed and has the “fundamental solution” $G(t, x_j)$. Let $A_j$ be mutually commuting group generators on some Banach space $B$. Then the initial-value problem on $B$, for the general abstract system of operator-differential equations $P(d/dt, A_j) = 0$, is also well-posed, and is solved explicitly by the integral of $\{\exp(s_j A_j)G(t, s_j)\}$. (LO1)

As an application, I published a new explicit solution of a general $n$th order linear parabolic system with variable coefficients, in terms of the solution of the
first-order equation generated by the \( n \)th root of the leading terms of the system. (PDE8, 10)

My UNM colleague Stan Steinberg and I generalized this result to non-commuting linear operators that are the generators of a Lie group. (PDE14) As an application, we solve hyperbolic equations in \( n \) dimensions with quadratic polynomial coefficients.

After the age of 50, my mind turned away from creating and discovering mathematics, and became fascinated by a different kind of question. What does it mean, to be a mathematician or to do mathematics? What does it mean, “to create or discover mathematics”? What do we do, when we do mathematics?
Chronology

Why are these stories about initial-value problems, stochastic processes, and linear operators, of any philosophical or methodological interest? Because they show how mathematics grows. Solving a problem, you get a new formula, or a new algebraic condition. The new formula suggests new questions, or new versions of old questions. Among these new questions, you look for one that’s neither too simple (uninteresting) nor too hard (hopeless). Answering this new question will yield another new formula or new condition, and so on and on and on.

Can one say more, in a philosophical sense? Yes, one can! More was said by Imre Lakatos, in his beautiful subversive challenge, *Proof and Refutations*. Much more was said by George Polya, in his series of fascinating books on mathematical heuristics. In philosophically deep ways, these authors dealt with the actual practice of living mathematics. More is being said even today, by mathematicians interested in questions of heuristics and methodology. Unfortunately, this kind of philosophizing is mostly ignored, by the dominant analytic or linguistically oriented “philosophy of mathematics.”

The “Overture” above, published 30 years ago in *The Mathematical Experience*, explains how an ordinary mathematician working on partial differential equations got entangled in philosophy. Since the 1970s, I have been seeking to understand the meaning, the significance, the essential nature of the activity that I and my colleagues, mentors and students carry on, and that we call “mathematics.” This searching is a kind of “philosophy of mathematical practice”.

Naturally, I did not expect to figure out the answers all by myself. One prominent, still respected collection is the anthology by Paul Benacerraf and Hilary Putnam. But “philosophy of mathematics in journal articles” had practically no connection with “philosophy of mathematics from the viewpoint of the mathematician.” In books and articles bearing the label “philosophy of mathematics”, I found ongoing arguments about Foundations, inherited from ancestral giants: “Quine” “Carnap” “Wittgenstein” “Frege.” Arguments disconnected from what mathematicians do and think about.

Since I wasn’t interested in these arguments between “philosophers of mathematics,” it followed that whatever I might be doing, it wasn’t “philosophy of mathematics”. But “the times they are a-changing”. See article 15 below for information about important recent work of Michael Crowe, Donald Gillies, Emily Grosholz, Herber Breger, Philip Kitcher, William Asprey, Javier Echeverria, and others. A recent book by academic philosophers of mathematics bears the title *Philosophy of Mathematical Practice*. In 2009 an Association for the Philosophy of Mathematical Practice (APMP) was “launched,” mostly in Germany and the Netherlands. “The APMP aims to foster the philosophy of mathematical practice, that is, a broad
outward-looking approach to understanding mathematics that engages with mathematics in practice—including issues in history of mathematics, the applications of mathematics, cognitive science, etc.” In October, 2013, it will hold a meeting right here in the USA, in Urbana, Illinois! Newly posted articles by Ursula Martin and Alison Pease, at the University of London, contain discourse analysis of on-line conversations by mathematicians, discussing on-going work solving mathematical problems.

In 1979 I wrote my first philosophical article, “Some proposals for reviving the philosophy of mathematics” (PP21) It could not be included here, because Elsevier, the publisher who owns the copyright, is being boycotted by mathematicians. I submitted that article to the American Mathematical Monthly, but a Harvard Philosopher explained that this piece was to philosophy as doggerel is to poetry. Nevertheless, it was immediately accepted by Gian-Carlo Rota for his Advances in Mathematics, and reprinted in Thomas Tymoczko’s anthology.

This article describes the philosophical plight of the working mathematician: “The typical “working mathematician” is a Platonist on weekdays and a formalist on Sundays. That is, when he is doing mathematics, he is convinced that he is dealing with an objective reality whose properties he is attempting to determine. But then, when challenged to give a philosophical account of this reality, he finds it easiest to pretend that he does not believe in it after all.” (Be aware that the term “realist” is often used as an equivalent term for “platonist”.)

I quoted Jean Dieudonné: “On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say, “Mathematics is just a combination of meaningless symbols,” and then we bring out Chapters 1 and 2 on set theory. Finally we are left in peace to go back to our mathematics and do it as we have always done, with the feeling each mathematician has that he is working with something real. This sensation is probably an illusion, but is very convenient. That is Bourbaki’s attitude toward foundations.”

And also Paul Cohen: “To the average mathematician who merely wants to know his work is securely based, the most appealing choice is to avoid difficulties by means of Hilbert’s program. Here one regards mathematics as a formal game and one is only concerned with the question of consistency. The Realist position is probably the one which most mathematicians would prefer to take. It is not until he becomes aware of some of the difficulties in set theory that he would even begin to question it. If these difficulties particularly upset him, he will rush to the shelter of Formalism, while his normal position will be somewhere between the two, trying to enjoy the best of two worlds.”

It disturbed me that mathematicians’ view of the nature of their work was commonly opportunistic, even unprincipled. And I offered a historical explanation of this state of affairs. Until well into the nineteenth century, geometry was regarded by everybody, including mathematicians, as the firmest, most reliable branch of knowledge. (Mathematical analysis derived its meaning and legitimacy from its link with geometry.) It was accepted, that the properties of space, of the Continuum, are exact, eternal, and knowable with certainty by the human mind. In the nineteenth century several disasters took place. Non-Euclidean geometries showed that there is more than one thinkable geometry. Analysis overtook geometric intuition, producing space-filling curves and continuous nowhere-differentiable curves.
These stunning surprises left geometric intuition, on which mathematics had been thought to rest, no longer so rigid, a bit wobbly!

The situation was intolerable, I explained, because all the way back to Plato, geometry had served as the great example of certainty in human knowledge. Spinoza and Descartes followed the "more geometrico" in establishing the existence of God. Newton followed it in establishing his laws of motion and gravitation. The loss of certainty in geometry threatened the loss of all certainty in human knowledge.

Nineteenth century mathematicians rose to this challenge. Following Richard Dedekind and Karl Weierstrass, they turned from geometry to arithmetic, as the foundation for mathematics—a great achievement, still taught to math grad students in the first week of grad school. Yes, you can base the Continuum on arithmetic, on the natural numbers. But there is a hangup—a worm in the apple. In order to construct the Continuum from arithmetic, you must use infinite sets.

Set theory at first seemed to be almost the same thing as logic, so it seemed that set theory was a solid foundation for mathematics. But, as Gottlob Frege put it in a famous postscript, "Just as the building was completed, the foundation collapsed." He had received a postcard from Bertrand Russell, communicating the Russell paradox. This was the beginning of the "crisis in foundations" in the first quarter of the 20th century.

The "logicists" Frege and Russell reformulated set theory to avoid the paradoxes. But their patched-up set theory, including an axiom of infinity, was too complicated to identify with "logic" in the philosophical sense of "the rules for correct reasoning." It became untenable, to claim that mathematics is just one vast tautology, nothing more than logic.

Russell wrote, "I wanted certainty in the kind of way in which people want religious faith. I thought that certainty is more likely to be found in mathematics than elsewhere. But I discovered that many mathematical demonstrations, which my teachers expected me to accept, were full of fallacies, and that, if certainty were indeed discoverable in mathematics, it would be in a new field of mathematics, with more solid foundations than those that had hitherto been thought secure. But as the work proceeded, I was continually reminded of the fable about the elephant and the tortoise. Having constructed an elephant upon which the mathematical world could rest, I found the elephant tottering, and proceeded to construct a tortoise to keep the elephant from falling. But the tortoise was no more secure than the elephant, and after some twenty years of very arduous toil, I came to the conclusion that there was nothing more that I could do in the way of making mathematical knowledge indubitable."

David Hilbert, the most influential mathematician of the early 20th century, wrote: "The present state of affairs where we run up against the paradoxes is intolerable. Just think, the definitions and deductive methods which everyone learns, teaches and uses in mathematics, the paragon of truth and certitude, lead to absurdities! If mathematical thinking is defective, where are we to find truth and certitude?" He proposed that once mathematical statements are written as sequences of formal symbols, then mathematical proofs become mere rearrangements of these sequences, and to secure the foundation of mathematics, all we need is a proof that the formal "axioms" of set theory never lead to a contradiction.

Although Hilbert is classified as a formalist, in his writings and conversations he talked about mathematical problems as questions about real objects, with answers
that are true in the same sense that any statement about reality is true. Yet to achieve certainty, he advocated a formalist interpretation of mathematics. “The goal of my theory is to establish once and for all the certitude of mathematical methods.” But Kurt Godel’s incompleteness theorems showed that Hilbert’s goal is unattainable. “Any formal system strong enough to contain elementary arithmetic is unable to prove its own consistency.”

Russell’s formal logic and Hilbert’s proof theory succeeded in starting new branches of mathematics. But they failed to provide an indubitable foundation for mathematics. Model theory and proof theory became integrated into contemporary mathematics, and they need foundations just as much (or as little) as the rest of mathematics.

A third famous school competed with the logicists and the formalists. It was led by the intuitionist Luitzen Brouwer. He accepted the natural numbers as reliable, needing no deeper foundation, but preached that no mathematics should be accepted that isn’t derived constructively from the natural numbers. He rejected the real number system as it is usually understood. Brouwer’s views were accepted in part by Hermann Weyl and Henri Poincaré, but the majority of mathematicians continue to work both constructively and non-constructively.

Logicism, formalism and intuitionism are three versions of one ideology, which Imre Lakatos named “foundationalism”—the search for an absolutely reliable foundation for mathematics. They disagreed on what to sacrifice, for the sake of that goal. But the goal was never attained. Few still hope or yearn for its attainment.

At this point, we have an explanation for the “working mathematician’s” uneasy oscillation between formalism and Platonism. His inherited philosophical dogma says that mathematical truth must possess absolute certainty. But his actual mathematical experience offers uncertainty in plenty!

“Platonism” (or “realism”) and “formalism” (both understood in a simplistic way by “the ordinary mathematician”) each offers a nonhuman “reality” where one imagines that absolute certainty dwells. Each has a certain credibility, because it corresponds to one aspect of mathematical experience.

Kurt Godel wrote that “Despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don’t see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception . . . . They, too, may represent an aspect of objective reality.”

But Abraham Robinson wrote, “I cannot imagine that I shall ever return to the creed of the true Platonist, who sees the world of the actual infinite spread out before him and believes that he can comprehend the incomprehensible.”

The basis for Platonism is the awareness we all have, that the problems and concepts of mathematics exist independently of us as individuals. The zeroes of the zeta function are where they are, regardless of what I think or know on the subject. It is then easy to imagine this objectivity to be outside of human consciousness as a whole, outside of history and culture. “Full-blooded Platonism” requires two distinct realms or universes—the physical, material world that includes human flesh and blood, and a separate, pre-existing, changeless universe of Ideas or Ideals or Abstractions. This myth does fit one aspect of the mathematician’s daily experience. Yet it contradicts the standard world view, accepted by most scientists
including mathematicians: the world is a single, united entity, with mind existing as a complex manifestation of matter in motion.

Platonism in the sense of existence of ideal entities, prior to human consciousness and matter, is tenable within a religious world-view—belief in a divine Mind. Martin Gardner the great journalist of mathematics, was such a believer. He denounced my views three times, because to him, math was “out there”, beyond human influence, because earthly beings could never have created such surpassing beauty. (PP16)

For those whose view of the world excludes mysticism, full-blooded Platonism withers when scientific skepticism is focused on it. At this point, as Dieudonné and Cohen confess, the alternative becomes formalism. Instead of thinking our theorems are truths about eternal extra-human ideals, we pretend instead to think they are merely assertions about transformations of symbols (formal derivations). The leading U.S. mathematician Marshall Stone [1903–1989] wrote that modern mathematics is “the study of general abstract systems, each one of which is built of specified abstract elements and structured by the presence of arbitrary but unambiguously specified relations among them.”

But how, indeed, do we know that our latest theorem about diffusion on manifolds is formally deducible from Zermelo-Frankel set theory? Rarely indeed is such a formal deduction ever written down.

Platonism and formalism, each in its own way, falsifies part of the reality of our daily experience. We speak as formalists when we are compelled to face the mystical, antiscientific essence of Platonic idealism; we return to Platonism when we realize that formalism as a description of mathematics has only a distant resemblance to our actual practice of mathematics.

The formal-language picture of mathematics is an example of “mathematical modeling.” An applied mathematician models water waves by a system of non-linear differential equations. No one will ever confuse studying those differential equations with actually getting wet and swimming! In the formal-language model of mathematics, there is a role reversal. Now mathematics—an actual real-world activity, by real flesh-and-blood people—is being modeled. The formal system is the model. It is as different from actual mathematics, as a set of partial differential equations is different from the Atlantic Ocean. As a mathematical model of actual mathematics, it has given actual mathematics some wonderful gifts: Godel’s incompleteness theorems, the undecidability of the Continuum Hypothesis, nonstandard analysis, the impossibility of an algorithm for Diophantine equations. Magnificent accomplishments of mathematical logic and this formal model of mathematics.

There is a confusing reflexivity, like the reflections in the mirror in my study. The mirror reflects the room where it’s placed, as though the room is “in the mirror”. But if the room is in the mirror, and the mirror is in the room, then the mirror is inside itself, which is absurd. What’s in the mirror isn’t the actual room, of course, it’s only a reflection of the room. In a similar way, the formal-language picture of mathematics tries to image all of mathematics, and being itself a part of mathematics, is imaged by itself. But mathematics remains, independent of and prior to its formal image, just as the room is there, with or without the mirror.

In this first philosophical article, I proposed abandoning both Platonism and formalism. Platonism, because it is inconsistent with the scientific world view. Formalism, because it is nothing like the actual mathematical activity that we are
actually doing. I wrote, “It is reasonable to propose a new task for mathematical philosophy: not to seek indubitable truth, but to give an account of mathematical knowledge as it really is—fallible, corrigible, tentative and evolving, as is every other kind of human knowledge. Instead of looking in vain for foundations, or feeling disoriented and illegitimate for lack of foundations, let’s try to look at what mathematics really is, as a part of human knowledge in general. Let’s reflect honestly on what we do when we use, teach, invent, or discover mathematics—by studying history, by introspection, and by observing ourselves and each other with the unbiased eye of Martians or anthropologists.

“Such a program requires a philosophical position which is radically different from the three classical points of view (formalist, Platonist, intuitionist). It denies the right of any a priori philosophical dogma to tell mathematicians what they should do, or what they really are doing in spite of themselves or without knowing it. Rather, it sees mathematics, as it is done now and has evolved in history, as a reality which does not require justification or reinterpretation. That philosophical position will seek to explicate what mathematicians are doing from the outside, as part of general human culture, rather than from the inside, in mathematical language. The result will be a description of mathematics which mathematicians will recognize.”

This is not so very different from the recent program of the newly formed Association for the Philosophy of Mathematical Practice! But there’s a crucial difference between asking philosophy of mathematics to change its goals, and proposing a new enterprise, with a new name. Asking philosophy of mathematics to transform itself was naïve, quixotic and presumptuous. A philosopher of mathematics knows that what he is doing is, “by definition,” philosophy of mathematics. If a mathematician complains that what the philosopher is doing is unrelated to the interests of mathematicians, such a mathematician evidently misunderstands the nature and purpose of philosophy of mathematics, which need not relate to what mathematicians do.

In this article I was raising questions, not yet offering answers. It was presumptuous for me to call for a transformation in the philosophy of mathematics. “The philosophy of mathematics”, by definition, is “whatever is being done by philosophers of mathematics”. It would have been wiser instead to call for a new “philosophy of mathematical practice”. That would be something to which the practicing mathematician could contribute. Meanwhile, “the philosophy of mathematics” may just as well continue on as before.

Alvin White, a professor at Harvey Mudd College in California, created an organization, the Humanistic Mathematics Network, with a Newsletter. I found it congenial, and participated actively. When the American Mathematical Monthly rejected an article I wrote calling for attention to pedagogy in undergraduate math education, Alvin published it in his Newsletter. (PP15) It has been revived, and upgraded to an online Journal of Humanistic Mathematics, by Gizem Karaali and her colleagues.

At Barbara Rosenkrantz’s seminar on history of science at Harvard I gave an invited talk, and met a philosopher of mathematics who was interested in actual mathematics. Philip Kitcher, on leave from the University of Vermont, was surprised and pleased to meet a mathematician who shared his opposition to Platonism. His support and encouragement have been important to me.
For *Eureka*, an undergraduate math magazine at Cambridge University, I wrote the following short piece, which became a chapter in my book, *What is Mathematics, Really?* and has been quoted and referred to by subsequent authors.

A major event in my philosophical evolution was reading *Proofs and Refutations* by Imre Lakatos. *Proofs and Refutations* is a brilliant work of literature. It presents an imaginary classroom where bright students compete with each other, under a patient teacher, to find the right definition of “polyhedron”. The focus is on the Descartes-Euler formula: “Number of faces minus number of edges plus number of vertices equals 2”. In my opinion, this is the most interesting and innovative work in the philosophy of mathematics in decades. In 1978 I wrote an article introducing Imre Lakatos to the readers of *The Mathematical Intelligencer*. (PP22)

Lakatos is fighting against formalism. He is showing mathematics alive, in development. He does not try to explain what math is or how it works. Instead, he shows what mathematicians really do, with an eye to methodology, motivation, goals and criteria.

I was fortunate in establishing a partnership with Philip J. Davis. He wanted to write math for the intelligent non-mathematician, while I wanted to write philosophy of math, so we agreed to work in parallel, for mutual support. Unfortunately, I was overwhelmed by deep personal problems. I wrote what I could, and then collapsed. Amazingly, Phil and Hadassah were able to sew together Phil’s chapters and mine, into some kind of a coherent book. More amazingly, the book became a “best seller,” so far as math books go, and won a National Book Award.

*The Mathematical Experience* did something unprecedented, and seemingly impossible: it presented modern, contemporary mathematical content, in a way accessible to any interested reader. We included my article with Paul Cohen on the Continuum Hypothesis. We also included an article I had written with the logician Martin Davis, about non-standard analysis. I wrote a new article on modern and classical Fourier analysis. Since then, many other authors have followed that example. We situated mathematics in the setting of general culture—historical, educational, psychological. I took some philosophical positions that were noticed by the more acute reviewers. Martin Gardner strongly rejected our view that mathematics is man-made, a part of human culture. John Burgess resented our disrespect for academic philosophy of math.

At that stage, I still did not present a developed, explicit position on the nature of mathematical knowledge. But I did bring forward two basic facts of life that an acceptable philosophy of mathematical practice has to include. *Mathematical entities are ideas, or thoughts or concepts, and we have reliable knowledge about them.* They are created by mathematicians, they arise in the minds of mathematicians. And a mathematical statement can be “true,” meaning simply that a mathematical problem may have a “correct” solution.

These two statements are not at all radical or surprising, of course! They can hardly be denied. But they are incompatible with either Platonism or formalism. I summarized them in the slogan, “true facts about imaginary objects.” Today I regret the word “imaginary,” “mental” would have been better.

I was greatly helped by another piece of reading—an essay by the anthropologist Leslie White. I read it in the four-volume anthology by James Newman called *The World of Mathematics*. White regarded it as the merest obviousness, that mathematics is a cultural entity. He was a close friend of the leading topologist
Raymond Wilder, at the University of Michigan. White influenced Wilder’s writings about math as a cultural entity, and he was clearly influenced by Wilder in taking up the question of the nature of mathematical knowledge.

Simply putting the nature of mathematics into the context of culture is an important step forward. Mathematical entities are ideas! Whose ideas? Yours, mine, everybody’s who has learned enough math to understand them. I first encounter a mathematical entity as something external, until I learn about it. I have to learn something that is already known by others, and I have to learn it in a way that they will accept and recognize as correct. Once I have learned it, it is internal, it is available to me directly, like the cube. The cube is there, present in my mind, for me to work on. But although it is my thought, my mental model, it is what it is. It has four internal diagonals, whether I know it or not. Four, not three or five. I must deal with it as it is, not as I might wish it to be. It is exterior in the sense that its properties are what they are, not what I can choose at will.

White’s article has a major shortcoming. He fails to acknowledge the deep difficulties about the nature of mathematics. We have to explicate the special character of mathematics—its seeming universality and permanence, so different from other aspects of culture—and its unique power to win universal consensus among qualified people.

By 1988 I was finally able to write the book I had long been attempting to produce. I took a sabbatical leave from New Mexico, and accepted Professor Hao Wang’s kind invitation, to share his office at Rockefeller University in New York. Sam Goldberg of the Sloan Foundation kindly provided financial support. I was simultaneously a visiting scholar at the Courant Institute.

What is Mathematics, Really? has two parts. The first part is programmatic, explaining what mathematics is—meaning, what do we do when we do mathematics. The second part is historic, summarizing what leading philosophers have said it is.

I was forced to put the philosophy of math into a broader philosophical framework. I claim that mathematical entities are notions, or ideas, or concepts—not private incommunicable ones, of course, but public, shared ideas. I had to cope with the unwillingness of philosophers to accept this information. This is an issue broader than the philosophy of mathematics, it is an issue of general ontology. What kinds of entities are there? In philosophy of mathematics I encountered three sorts of entities. Material, or physical, of course. Also mental, in the sense of the private, internal, subjective world of the individual philosopher. And transcendental, whether overtly religious and spiritual as in Descartes, Spinoza, Leibnitz, or secular and abstract, often called Reason, in later writers.

What about ordinary daily life—family and school, buying and selling, war and peace? The social-cultural-historical realm is real. We govern our lives according to it. We can and do have reliable knowledge about it. This statement is as simple and obvious as any can be, yet it has to be made, because much of present-day analytic philosophy chooses to block out this level of reality. Outside the philosophy department, nothing is considered more “real world” than money and power. A brilliant astronomer was a failure as department chairman, and people said he needed to come down from the stars and the galaxies to “the real world”—budgets, hiring, promotion, tenure. The public, the inter-subjective, the socio-historical, is an aspect or level of reality, just as much as the physical-material level and
the mental-subjective level. This is a banal and obvious claim. Yet it is essential, because the puzzle about the nature of mathematical entities can be unraveled only in that context. Geometrical, arithmetical, and the other varieties of mathematical entities are concepts—ideas or thoughts shared in common by certain groups of people. Of course they are special, different kinds of concepts, not just like other kinds of concepts. The most salient difference is that they compel agreement. They achieve a unanimity, even universality, and a seeming permanence, that distinguish them from our other shared concepts. There is a lot to explain here, a lot to think about and figure out. But the very first step is seeing that we are talking about human concepts—not something “out there,” as my unwearying antagonist Martin Gardner kept writing, but “in here”, ”down here”.

When the legitimate reality of the social cultural world is acknowledged, one can ask philosophers to take seriously the possibility that mathematical reality is neither in a physical nor a mental nor a transcendental realm, but in the social-cultural realm.

In the second part of What is Mathematics, Really?, the historical part, I showed two different threads in the history of philosophy. The dominant trend, going back to Plato, and including the rationalists and Frege, saw math as transcendent, super-human. An opposing tradition, from Aristotle on through Locke and Hume and Wittgenstein, saw it as a human activity. The dilemma about the nature of math is created by the metaphysics which sees the quotidian, the daily, the social-historical, as illusory, unfit for philosophical attention.

In What is Mathematics, Really?, I again took it on myself to presumptuously argue against the prevailing state of philosophy of mathematics. I still had the naïve idea that philosophy of mathematics should be about actual mathematics (what mathematicians like me are engaged in). My provocative book received hostile or indifferent reviews. Some reviewers seized, with indignation, on a few pages where I considered a possible correlation between a philosopher’s views on metaphysics and on politics. “How dare you!” seemed to be their message to me.

The historical part stretches from Plato to Quine, and the treatment of each philosopher is brief. It is fair enough to complain about this brevity. I focused on the doctrines relevant to the topic of the book.

Almost at the same time as my publication of What is Mathematics, Really?, Paul Ernest, an English mathematics educator, published Social constructivism as a philosophy of mathematics. Our views are almost identical. His book takes much greater pains than mine to relate to the philosophical literature. He presents his philosophy as a successor to both Lakatos and Wittgenstein. I had named my own views “humanism.” It seemed unavoidable to give them some label, and I wasn’t completely comfortable with “social constructivism”. I am trying to describe what mathematicians do, not to found or lead a “school” competing with logicism, constructivism, formalism, naturalism, structuralism, and nominalism. In the end I have realized, I am a pragmatist in the style of John Dewey.

In 18 Unconventional Essays on the Nature of Mathematics I noticed several articles on the Internet, by various authors in various countries, who had interesting, unconventional things to say about the nature of mathematics. I collected these articles together, to make the different authors aware of each other’s work, and reprinted a few journal articles that were somewhat buried, and should be more widely known, especially Leslie White’s article on the anthropological perspective.
on math, and Alfred Renyi’s Socratic dialogue on math. I showed Carlo Cellucci to Anglophone audiences, and reprinted Bill Thurston’s much referenced article. Other articles bring in semiotics (Rotman), the sociology of science (Pickering), evolutionary epistemology (Rav), the embodied basis of mathematical continuity (Nunez), and formal proof verification (Mackenzie). Two articles in the present volume are taken from that book.

In 2011 Vera John-Steiner and I published *Loving and Hating Mathematics*, to bring forward the emotional and political sides of mathematical life. We were thrilled that within days of publication, our book was picked as the Book of the Week by the *London Times*. Enthusiastic reviews appeared in the *American Mathematical Monthly*, *The Mathematical Intelligencer* and the *SIAM Review*.

In the last few years, I was invited to several conferences, and contributed to several edited volumes. This stimulation led me to write much more specifically and concretely than I had done before. Jean van Bendegem and Bart van Kerkhove invited me to speak in Brussels, and published my contribution (8). To elaborate on my description of the nature of mathematics, I showed a sequence of small steps that result in a simple new derivation of Heron’s formula for the area of a triangle as a function of the lengths of the three sides. (PP18, 19, 20, and the articles *Mathematical Intuition* ([Poincaré, Polya, Dewey]) and *To Establish New Mathematics, We Use Our Mental Models and Build on Established Mathematics* below.)

*Mathematical Intuition* ([Poincaré, Polya, Dewey]) is a talk that I gave twice, first at a celebration in Rio de Janeiro, led by Carlos de Moura, by numerically oriented applied mathematicians, honoring my mentor Peter Lax and the 80th anniversary of the Courant-Friedrichs-Lewy criterion, and for a second time in Rome, in the philosophy department of the University of Rome, at a meeting on knowledge and logic, led by Carlo Cellucci. It was appropriate for both occasions, because it uses the actual practice of digital computing, in pure and applied mathematics, to show that mathematical knowledge includes not only the results of deductive reasoning, but also “common knowledge,” based on several kinds of plausible reasoning. (An unpublished article by Philip J. Davis made this point several years ago.) This reality supports the position taken by pragmatism, that observation and reasoning provide “warranted assertability.” “Truth” in the sense of unqualified certainty is not available and not necessary.

In this article I introduced and elaborated on the term “mental model” to explain how mathematicians access and manipulate mathematical concepts, by means of their own personal representatives of those concepts, which they possess internally.

Suppose someone asks me, “How many interior diagonals does a cube have?” I think for a minute, and say “four.” I don’t look in a book. I don’t construct a list of sentences, starting from some “axioms” and concluding “4.” I use my “mental model” of the cube. “Visualizing” the cube, and reasoning about what I “see” mentally, makes it clear that there are four interior diagonals.

“Do they intersect?”

“Yes, of course. By symmetry, I see their four midpoints in common, all at the center of the cube.”
And so on. If the cube is made of iron or copper, with three adjacent faces at
temperature 100 degrees, and three opposite faces at temperature zero, then the
temperature at the center is 50.

The cube is an elementary mathematical entity easy to visualize. Its equilib-
rium temperature distribution, not quite so elementary. We could go on to the
octahedron, which has three internal diagonals, and then the other Platonic solids,
and then regular polytopes in four dimensions, and on and on. My mental models
of these other mathematical entities would also enable me to answer questions.

Mathematical research is based on reasoning about our mental models of math-
ematical entities, starting from established mathematics, and seeking to add to it
or improve it (not by syntactic transformations of formal “axioms”). It is carried
out in human brains and bodies, historically developed in societies on the surface
of this planet, and assisted by scraps of paper, blackboards, printed matter, and
digital computers.

To Establish New Mathematics, We Use Our Mental Models and Build on Es-
tablished Mathematics was invited for Carlo Cellucci’s Festschrift. Stimulated by
Carlo Cellucci’s writings for and against mathematical proof, it became clear to
me that the model of formal proof, where unproved axioms are stated, and then
syntactic derivation proceeds to conclusions, is not similar to the usual, and typical
mathematicians’ proof. Mathematicians’ proof seldom states axioms; it starts from
some chosen facts from established mathematics, and proceeds by means of the
properties of the mathematical entities in question (rather than the syntactic form
of the sentences in which these are expressed). No axioms need be stated; it is
sufficient to refer to established mathematics, as it is needed. The reasoning makes
no reference to the rules of logic.

It is commonly taken as factual that mathematical reasoning is based on the ax-
ioms of Zermelo-Frankel set theory, and uses first-order logic. But mathematicians
don’t refer to Zermelo-Frankel and don’t know the rules of first-order logic. Their
reasoning can be formalized, and sometimes actually is formalized, to be checked
on a computer. Interesting and worthy as such a project certainly is, it can by no
means claim to describe what mathematicians do.

A question then arises, for which I thank Brendan Larvor. What justifies
mathematicians’ claim that their reasoning is actually a proof? (See How Mathe-
maticians Convince Each Other or “The Kingdom of Math is Within You”.) Since
it is not based on logic, why does it work, why does it carry conviction? When I
read Euclid’s proof, that “Given any finite set of prime numbers, there is another
prime number not in that set”, I find the proof convincing because I have available
directly, by reflection or mental effort, the relevant facts about prime numbers.
Just as the proof of Euclid’s Proposition 1 (constructing an equilateral triangle on
a given base) is convincing, because I can “see” that the two circular arcs must
cross. We perceive, as Godel said, we say we “see”, that something is so, referring
to a mathematical concept which we possess mentally. We know it, not only by a
verbal definition, but also by a whole collection of facts, properties, and connec-
tions to other mathematical entities. All this information together, coherent and
interconnected, constitutes what I call our “mental model of a mathematical en-
tity”. Quoting G. H. Hardy and other authors, I argue that mathematicians’ proof
works by showing us how to connect some claimed mathematical result to some
mathematical models we possess, which we directly manipulate and inspect.
The mathematicians’ proof carries conviction by a process analogous to that in empirical science. The chemist, physicist or engineer observes the results of an experiment. His conviction is ultimately based on accepting the testimony of his eyes. “Seeing is believing.” The mathematician accepts a proof because of what he “sees,” perceives internally by examining his mental model.

There is a two-sidedness, a kind of duality, that has to be recognized. Mathematical entities are both in the individual minds of mathematicians, and in a public, shared, inter-subjective consciousness.

**Pragmatism** is the philosophy that grounds knowledge in human practice. Considering something as grounds for action, that is regarding it as true in practice. Mathematical practice is an aspect of general human practice.

Once you have absorbed a mathematical concept, learned it and it’s really yours, it’s available in your own head to work with, play with, match or connect to other ideas. You SEE that something is so; you have direct mental access to it.

The mathematical term “equivalence class” clarifies the nature of a mathematical concept. A mathematical concept is a collection of equivalent ideas in the minds of individuals. Its presence in many individual minds makes it part of culture, independent of my individual consciousness. The “objectivity” of a mathematical concept is this external existence. The “certainty” of the mathematical concept is our direct access, our personal representative of that concept. The matching, the congruence between the social and the individual, is created and maintained by our conversation—our schools, publications, professional rewards and punishments.

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Mathematics Has a Front and a Back

Abstract It is explained that, in the sense of the sociologist Erving Goffman, mathematics has a front and a back. Four pervasive myths about mathematics are stated. Acceptance of these myths is related to whether one is located in the front or the back.

In the famous book, *The Presentation of Self in Everyday Life*, by American sociologist Erving Goffman, there is a chapter called ‘Regions and Region Behavior’. There Goffman introduces the concept of the “front” and the “back”: regions to which the public is admitted, and from which it is excluded. In a restaurant, for example, the serving area is the “front”; the kitchen is the “back”. In a theater, of course, the front of the stage is for the audience; backstage is for the actors, stagehands, props, and costumes. In front, the actors (waiters) wear costumes (uniforms); in back, they change clothes or rest in their casual dress. In general, the front is the region to which the public is admitted, where service is performed; the back is a region restricted to professionals, where preparations are made to provide a service.

Goffman’s contribution was to extend this concept of the “front region” and the “back region” from restaurants and theaters to all, or almost all, institutions of modern life. In universities, the classrooms and certain parts of the library are the “fronts” where the “public” (the students) are served. The Chairman’s or the Dean’s offices are the “backs”, where the products (classes and courses) are prepared “behind the scenes”.

There is nothing sinister in this separation; it is a practical necessity. Goffman gives examples of the distress that can arise from blurring the line between “front” and “back”; for instance, a gasoline (petrol) station whose customers feel free to wander into the parts department and help themselves to wrenches and hammers.

Goffman quotes Orwell:

> It is an instructive sight to see a waiter going into a hotel dining room. As he passes the door a sudden change comes over him. The set of his shoulders alters; all the dirt and hurry and irritation have dropped off in an instant. He glides over the carpet, with a solemn, priest-like air . . . he entered the dining room and sailed across it, dish in hand, graceful as a swan. (Goffman, p. 121)

The waiter who does this is performing automatically. If his split persona were brought to his attention, he would acknowledge its existence. But in the ordinary course of things, he just waits on tables. He is not conscious of putting on an act, or fooling anybody.

My purpose here is to point out that, like other social institutions, mathematics too has its “front” and its “back”, and to identify and describe them. It should be clear that now we are not speaking of “regions” in the literal, physical sense, as in a dining room and kitchen: mathematics is not necessarily associated with any particular physical setting; it is just a certain sort of activity. So its “front” and “back” will be particular kinds or aspects of mathematical activity, the public and private, or the part offered to “outsiders” (down front) versus the part normally restricted to “insiders” (backstage).

In this sense of the term, the “front” of mathematics is mathematics in “finished” form, as it is presented to the public in classrooms, textbooks, and journals. The “back” would be mathematics as it appears among working mathematicians, in informal settings, told to one another in an office behind closed doors.

Compared to “backstage” mathematics, “front” mathematics is formal, precise, ordered and abstract. It is separated clearly into definitions, theorems, and remarks. To every question there is an answer, or at least, a conspicuous label: “open question”. The goal is stated at the beginning of each chapter, and attained at the end.

Compared to “front” mathematics, mathematics “in back” is fragmentary, informal, intuitive, tentative. We try this or that, we say “maybe” or “it looks like”.

Observe that in all our examples the front is divided into subregions, of first, second, and even third class. A restaurant, for example, may include both a banquet hall and a snack bar. A theater has box, orchestra, and balcony seats. And the public for mathematics includes, among others, professional mathematicians themselves, graduate students, and undergraduates.

The back is also divided, for efficiency and convenience, into subregions. In a restaurant, there are the domains of the salad chef, pastry chef, dishwasher, and so on. The reader can fill in the analogous divisions among working mathematicians.

The purpose of a separation between front and back is not just to keep the customers from interfering with the cooking; it is also to keep the customers from knowing too much about the cooking.

Everybody down front knows that the heroine of the melodrama is wearing rouge. They probably don’t quite know what she looks like without it. The diners know what’s supposed to go into the ragout, but they don’t know for sure what does go into it.

We can describe this state of affairs by saying that the front/back separation makes possible the preservation of a myth—whether it be the flavoring of the food or the beauty of the actress.

By a myth we shall mean simply taking the performance seen from up front at face value; failing to be aware that the performance seen “up front” is created or concocted “behind the scenes” in back. This myth, in many cases, adds to the customer’s enjoyment of the performance; it may even be essential.

More generally, a myth is a story that possesses a certain allegorical or metaphorical power; it is not literally true, but it survives while the generations pass by. Such, for instance, was the myth of the divine right of kings. Such are the myths of Christmas and Easter, and of course, the corresponding myths of other religions.

Mathematics, too, has its myths. One of the unwritten criteria separating the professional from the amateur, the insider from the outsider, is that the outsiders are taken in (deceived), the insiders are not taken in.
It would be straining patience to try to compile a complete dictionary of myths in mathematics. We list a few; enough to illustrate our point, and to enable the reader (as an exercise) to extend the list at pleasure.

To present, describe and refute all of these myths would generate a thick volume. We content ourselves with some provocative comments; the reader can follow them up with the readings listed in the bibliography.

First, the myth of Euclid. This is discussed on pages 322–30 of *The Mathematical Experience* (see Davis and Hersh). The Euclid myth is defined there as the belief that the books of Euclid contain truths about the universe which are clear and indubitable. In view of the general availability of *The Mathematical Experience*, we need not go into a detailed discussion of the Euclid myth here. We merely point out that advanced students of geometry, and certainly professional mathematicians, are all aware that Euclid’s axioms are unintelligible, his proofs incomplete, and his results limited to very restricted and special cases. Nevertheless, in secondary schools, in watered-down versions that fail even to mention his impressive achievements in solid geometry, Euclid continues to be upheld as the ideal model of pure mathematics and rigorous proof.

In a similar way, the plaster Newton created in the eighteenth century ("God said, Let Newton be—and all was light") is intact as a myth; the complex historical reality of Newton is almost unknown, even among the mathematically literate.

The myths of Russell, Brouwer and Bourbaki—of logicism, intuitionism, and formalism—have also been treated in *The Mathematical Experience*. Formalism is the subject (object?) of a beautiful diatribe in the preface to Lakatos’ *Proofs and Refutations*. Therefore, in the hope of encouraging the circulation of *The Mathematical Experience*, we pass on to the more general myths on our list.

1. **Unity**: There is only one mathematics, indivisible, now and forever. Mathematics is a single/inseparable whole.
2. **Objectivity**: Mathematical truth or knowledge is the same for everyone. It does not depend on who in particular discovers it; in fact, it is true whether or not anybody ever discovers it.
3. **Universality**: Mathematics as we know it is the only mathematics that there can be. If the little green men (and women?) from Quasar X9 sent us their math textbooks, we would find again $A = \pi r^2$.
4. **Certainty**: mathematics possesses a method, called “proof” or sometimes “rigorous proof”, by which one attains absolute certainty of the conclusions, given the truth of the premises.

It would not be hard to find quotations to show that these beliefs are indeed widely held. Fortunately *Eureka* strives for entertainment, not pedantry, so we dispense with references.

By calling these beliefs myths, I am not declaring them to be false. A myth need not be false to be a myth. The point is that it serves to support or validate some social institution; its truth is irrelevant, and most likely not determinable.

Who can say, for example, that the doctrine of the divine right of kings is false? In the absence of a clear channel to the mind of God, this dogma can never be absolutely proved or disproved. But it was a useful belief, which in its time was credible, and served a purpose.

In a similar way, the unity, universality, objectivity, and certainty of mathematics are beliefs that support and justify the institution of mathematics. (For
mathematics, which is an art and a science, is also an institution, with budgets, administrations, publications, conferences, rank, status, awards, grants, etc.)

Part of the job of preparing mathematics for public presentation—in print or in person—is to get rid of all the loose ends. If there is disagreement whether a theorem has really been proved, then that theorem will not be included in the text or the lecture course. The standard style of expounding mathematics purges it of the personal, the controversial, and the tentative, producing a work that acknowledges little trace of humanity, either in the creators or the consumers. This style is the mathematical version of "the front".

Without it, the myths would lose much of their aura. If mathematics were presented in the same style in which it is created, few would believe in its universality, unity, certainty, or objectivity.

Beliefs (1) through (4) are not self-evident or self-proving; they can be questioned, doubted, or rejected. Indeed, by some people they are rejected. Standard and "official" as these doctrines are, they are not taken so literally, so naively, by the backstage people. (A busboy or a stagehand is likely to be skeptical about the contents of the stew or the complexion of the ingenue.) Let us examine them critically, in order from (1) to (4), to justify our calling them myths.

From a backstage point of view, then, what about myth (1), unity? We see pure and applied mathematicians cooperating sometimes, but more often unaware of each other's work, and usually working to quite different standards and criteria. The pure may even declare that applied mathematics is not mathematics at all ("Where are the definitions? Where are the theorems?"). Or even worse, it is bad mathematics. (See Halmos. This article is a landmark piece for having the courage to express an attitude, common but unspoken, among "pure" mathematicians.) And even within pure mathematics, it is plainly visible at meetings of the American Mathematical Society that any contributed talk is understood by only a small fraction of those present at the meeting. The "unity" claimed in principle does not exist in practice.

As to myth (2), objectivity—yes, there is an amazingly high consensus in mathematics as to what is "correct" or "accepted". But beside this, and equally important, is the issue of what is "interesting" or "important" or "deep" or "elegant". These aesthetic or artistic criteria vary widely, from person to person, specialty to specialty, decade to decade. They are perhaps no more objective than aesthetic judgments in art or music.

And universality (myth (3))—who is to say? If there is "intelligent life" in Quasar X9, whatever we should mean by that, it might not be little green women and men. It might be blobs of plasma which we could not even recognize as intelligent beings. What would it mean to talk about their literature, or art, or mathematics? The very notion of comparing presupposes beings enough like us to make communication conceivable. But then the possibility of comparison is not universal; it's conditional on their being "enough like us".

And last of all, myth (4), certainty. Most of us are certain that $2 + 2 = 4$, though we probably would find we don't all mean exactly the same thing by that equation. But it's quite another matter to claim equal certainty for the theorems of contemporary mathematics.

Many of them have proofs which fill dozens of pages, which rely on other theorems whose proofs have not been rigorously checked by their users. These
proofs do not pretend to be complete but often contain such phrases as, “it is easily seen” or “a standard argument then yields” or “a short calculation gives” and so on. Moreover, more and more often, the paper will have several co-authors, not one of whom has carefully read the whole paper; and very possibly it will use the result of some calculations of a computing machine that none of the authors, and possibly no living human being, completely understands. Certainty, like unity, can be claimed only “in principle”, not in practice.

Myths, of course, need not be true; they need to be useful. Whatever the reason, it is clear that mathematicians want to believe in unity, objectivity, universality, and certainty, somewhat as Americans want to believe in the Constitution and free enterprise, or other nations, in their Queen or their Revolution. But even while they believe, they know better.

An important part of becoming a professional, in mathematics or anywhere else, is to move from the “front” to the “back”. And part of this transition is to develop a less naive, more sophisticated attitude toward the myths of the profession. The leading lady needs her rouge; the stagehands know that she is the same actress they see behind the scenes with an ordinary, everyday face.

Acknowledgment

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References

Part 1

“Mostly for the right hand”
Introduction

These articles explain that there is a problem about what we do, when we do mathematics, and begin untangling and clarifying that problem. *True Facts About Imaginary Objects* is the last chapter of my 1981 book (co-authored with Philip J. Davis). There I explain why none of the three textbook philosophies on offer (logicism, intuitionism, formalism) are compatible with mathematical experience. A philosophical stance is needed that accepts two facts of life: mathematical entities are created by people (mainly, by mathematicians); and we have reliable factual knowledge of these entities. I had put that in shorthand as “true facts about imaginary objects,” language which I now regret. Mental entities are not “imaginary,” they are real, in the ordinary usage of the standard English word “real.”

The next three articles, *Mathematical Intuition (Poincaré, Polya, and Dewey)*, *To Establish New Mathematics, We Use Our Mental Models and Build on Established Mathematics*, and *How Mathematicians Convince Each Other or “The Kingdom of Math is Within You”*, are the most recent ones, and they fit together. They all were invited contributions to conferences or collections. They try to be specific and detailed about what we do when we do mathematics. *Mathematical Intuition (Poincaré, Polya, and Dewey)* was delivered twice; first in Rio de Janeiro, Brazil, in May 2010, where Carlos Antonio de Moura and other hard-core numerical applied mathematicians were celebrating the 80th anniversary of one of their favorite theorems, and then in June 2010, in the philosophy department of the University of Rome, at a meeting on “logic and knowledge,” organized by Carlo Cellucci, Emily Grosholz and Emiliano Ippoliti. In this twice-born paper, I speak not about mathematical truth, but about “warranted assertions” (thanks to John Dewey). Knowledge about mathematics can be either “rigorous” knowledge based on deductive argument, or “common knowledge,” based on experience, either physical or computational. I explain what mathematicians mean by “mathematical intuition”, where knowledge of mathematical entities comes from direct access to personal mental models (argument supported by the writings of George Polya, Jacques Hadamard, Henri Poincaré and Kurt Godel). These personal models of numbers, functions, operators, spaces, and so on, are kept mutually congruent or equivalent, by the work of the mathematical community’s classes and schools, journals and editors, departments and societies.

*To Establish New Mathematics, We Use Our Mental Models and Build on Established Mathematics* is from a Festschrift for Carlo Cellucci. There I show that the model called “axiomatic proof” doesn’t apply to most mathematicians’ proof. Mathematicians’ proof draws freely from the body of established mathematics, and its aim is to add to that body, and to strengthen it and clarify it. Mathematical arguments are semantic, not syntactic. That is, they are not about the structure of sentences, they’re about the properties of mathematical entities. (This argument
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is supported with careful reports from Yuri Manin, Bill Thurston, and Vaughan Jones.)

How Mathematicians Convince Each Other or “The Kingdom of Math is Within You” was inspired by a question of Brendan Larvor: “What qualifies the usual informal mathematical arguments as ‘proof’?” I preferred a somewhat different question. Not “what qualifies them?”, but rather, “What makes informal proofs work? How do they compel agreement?”

By informal proof, I mean the usual mathematician’s proof, which does not use a formal language, but rather a mixture or alternation between ordinary language and mathematical calculations. These calculations have some resemblance to formal logical manipulations, but they are not at all the same thing. Using a well-known statement by G. H. Hardy, I argue, in answer to Larvor, that mathematicians’ proof compels agreement because it’s based, in important part, on observation of our personal mental models, which we experience as direct perception of actual entities—in this case, of our personal representatives or samples of socio-cultural entities, which are usually called “concepts.” I support my argument with very extensive statements by a substantial number of distinguished mathematicians. Bart van Kerkhove in Brussels offered to include this article in his Festschrift for Jean Paul van Bendegem.

I had earlier, in On the Interdisciplinary Study of Mathematical Practice, with a Real Live Case Study and Wings not Foundations!, tried to explain what mathematicians really do, by an example from elementary mathematics—a high-school algebra derivation of Heron’s area theorem, which classically required an ingenious, elaborate construction of related triangles. I break up my elementary derivation into tiny steps, trying to provide an instructive case history of mathematical research, understandable to ordinary philosophers. On the Interdisciplinary Study of Mathematical Practice, with a Real Live Case Study was given at a meeting in Brussels organized by Jean Paul van Bendegem. Wings not Foundations! was invited for a publication in Milan, Italy.

The “definition of mathematics”, Definition of Mathematics, was also invited for publication in Milan. My definition of mathematics does not refer to any particular mathematical content, but only to the irresistible consensus that mathematical proof achieves. The argument is based on the question, “How do we decide when some new field of abstract reasoning is to be considered part of mathematics?”

Mathematical Practice as a Scientific Problem was requested by Bonnie Gold for her anthology published by the Mathematical Association of America. This collection attempted to bring together on an equal footing, contributions by philosophers and by mathematicians.

Inner Vision, Outer Truth and the Introduction are from my own anthology, 18 Unconventional Essays on the Nature of Mathematics (18UENM). Inner Vision, Outer Truth deals with a well-known, very difficult question. Why do the mathematical concepts that we seem to create freely, simply in order to please each other and ourselves, sometimes turn out to be amazingly powerful in utterly unforeseen physical applications? I try to clarify this problem by looking at the very simplest possible instances. The Introduction to 18UENM glances over the developments in the philosophy of mathematical practice from the 1970’s, when I published my first article on the topic, to the early 21st century. That first article could not be
included here, but it is available in Tymoczko’s collection, as well as in the original publication by Gian-Carlo Rota in his journal *Advances in Mathematics*.

*Proving is Convincing and Explaining* and *Fresh Breezes in the Philosophy of Mathematics* are attempts, during this passage of time, to explain that math is real, and not “up there” but “down here,” with us.
True Facts About Imaginary Objects

The unspoken assumption in the three traditional foundationist viewpoints is that mathematics must be a source of indubitable truth. The actual experience of all schools—and the actual daily experience of mathematicians—shows that mathematical truth, like other kinds of truth, is fallible and corrigible.

Do we really have to choose between a formalism that is falsified by our everyday experience, and a Platonism that postulates a mythical fairyland where the uncountable and the inaccessible lie waiting to be observed by the mathematician whom God blesses with a good enough intuition? It is reasonable to propose a different task for mathematical philosophy, not to seek indubitable truth, but to give an account of mathematical knowledge as it really is—fallible, corrigible, tentative, and evolving, as is every other kind of human knowledge. Instead of continuing to look in vain for foundations, or feeling disoriented and illegitimate for lack of foundations, we have tried to look at what mathematics really is, and account for it as a part of human knowledge in general. We have tried to reflect honestly on what we do when we use, teach, invent or discover mathematics.

The essence of mathematics is its freedom, said Cantor. Freedom to construct, freedom to make assumptions. These aspects of mathematics are recognized in constructivism and formalism. Yet Cantor was a Platonist, a believer in a mathematical reality that transcends the human mind. These constructions, these imagined worlds, then impose their order on us. We have to recognize their objectivity; they are partly known, partly mysterious and hard to know; partly, perhaps, unknowable. This is the truth that the Platonist sees.

What happens when we observe two contradictory facts in the world, both of which are undeniably true? We are forced to change our way of looking at the world, we are forced to find a viewpoint from which the facts are not contradictory but compatible.

Another way of saying the same thing is this. If a fact goes against common sense, and we are nevertheless compelled to accept and deal with this fact, we learn to alter our notion of common sense.

There are two pieces of information which we have about the nature of mathematics.

Fact 1 is that mathematics is a human invention. Mathematicians know this, because they do the inventing.

The arithmetic and elementary geometry that everybody knows seem to be God-given, for they are present everywhere, and seemingly always were. The latest algebraic gadget used by topologists, the newest variation of pseudodifferential operators, have been invented recently enough so that we know the names and addresses of the inventors. They are still shiny from the mint. But we can see the line
of descent. The family resemblance is unmistakable, from the latest new thing to the most ancient. Arithmetic and geometry came from the same place as homotopy theory—from the human brain. Every day, millions of us labor to instill these into other human brains.

Fact 2 is that these things we bring into the world, these geometric figures and arithmetical functions and algebraic operators, are mysterious to us, their creators. They have properties which we discover only by dint of great effort and ingenuity; they have other properties which we try in vain to discover; they have properties which we do not even suspect. The whole activity of mathematical problem solving is evidence for Fact 2.

Formalism is built on Fact 1. It recognizes that mathematics is the creation of the human mind. Mathematical objects are imaginary.

Platonism is built on Fact 2. The Platonist recognizes that mathematics has its own laws which we have to obey. Once we construct a right triangle, with sides $a$, $b$, and hypotenuse $h$, then $a^2 + b^2 = h^2$ whether we like it or not. I don’t know whether 1375803627 is a prime number or not, but I do know that it is not up to me to choose which it is; that is already decided as soon as I write the number down.

These imaginary objects have definite properties. There are true facts about imaginary objects.

From the Platonist point of view, Fact 1 is unacceptable. Since mathematical objects are what they are, in defiance of our ignorance or preferences, they must be real in a sense independent of human minds. In some way we cannot explain or understand, they exist, outside of the material world, and outside the human mind.

From the constructivist viewpoint, Fact 2 is unacceptable. Since mathematics is our creation, nothing in mathematics is true until it is known—in fact, until it is proved by constructive methods.
As for the formalist, he escapes the dilemma by simply denying everything. There are no mathematical objects, so there are no problems about the nature of mathematical objects.

If we are willing to forget about Platonism, constructivism, and formalism, we can take as our starting point the two facts that we learn from mathematical experience:

Fact 1: Mathematics is our creation; it is about ideas in our minds.

Fact 2: Mathematics is an objective reality, in the sense that mathematical objects have definite properties, which we may or may not be able to discover.

If we believe our own experience and accept these two facts, then we have to ask how they can be reconciled; how we can look at them as compatible, not contradictory. Or better said, we have to see what presumptions we are making that force us to find something incompatible or contradictory about these two facts. Then we can try to dispose of these presuppositions, in order to develop a point of view broad enough to accept the reality of mathematical experience.

We are accustomed, in a philosophical context, to think of the world as containing only two kinds of material—matter, meaning physical substance, what you study in a physics lab—and mind, meaning my mind or your mind, the private psyche each of us has someplace inside our skulls. But these two categories are inadequate. Just as inadequate as the four categories, earth, air, fire, and water of ancient Greece were inadequate for physics.

Mathematics is an objective reality that is neither subjective nor physical. It is an ideal (i.e., nonphysical) reality that is objective (external to the consciousness of any one person). In fact, the example of mathematics is the strongest, most convincing proof of the existence of such an ideal reality.

This is our conclusion, not to truncate mathematics to fit into a philosophy too small to accommodate it—rather, to demand that the philosophical categories be enlarged to accept the reality of our mathematical experience.

The recent work of Karl Popper provides a context in which mathematical experience fits without distortion. He has introduced the terms World 1, 2, and 3, to distinguish three major levels of distinct reality.

World 1 is the physical world, the world of mass and energy, of stars and rocks, blood and bone.

The world of consciousness emerges from the material world in the course of biological evolution. Thoughts, emotions, awareness are nonphysical realities. Their existence is inseparable from that of the living organism, but they are different in kind from the phenomena of physiology and anatomy; they have to be understood on a different level. They belong to World 2.

In the further course of evolution, there appear social consciousness, traditions, language, theories, social institutions, all the nonmaterial culture of mankind. Their existence is inseparable from the individual consciousness of the members of the society. But they are different in kind from the phenomena of individual consciousness. They have to be understood on a different level. They belong to World 3. Of course, this is the world where mathematics is located.

Mathematics is not the study of an ideal, preexisting nontemporal reality. Neither is it a chess-like game with made-up symbols and formulas. Rather, it is the part of human studies which is capable of achieving a science-like consensus, capable of establishing reproducible results. The existence of the subject called mathematics
is a fact, not a question. This fact means no more and no less than the existence of modes of reasoning and argument about ideas which are compelling and conclusive, “noncontroversial when once understood.”

Mathematics does have a subject matter, and its statements are meaningful. The meaning, however, is to be found in the shared understanding of human beings, not in an external nonhuman reality. In this respect, mathematics is similar to an ideology, a religion, or an art form; it deals with human meanings, and is intelligible only within the context of culture. In other words, mathematics is a humanistic study. It is one of the humanities.

The special feature of mathematics that distinguishes it from other humanities is its science-like quality. Its conclusions are compelling, like the conclusions of natural science. They are not simply products of opinion, and not subject to permanent disagreement like the ideas of literary criticism.

As mathematicians, we know that we invent ideal objects, and then try to discover the facts about them. Any philosophy which cannot accommodate this knowledge is too small. We need not retreat to formalism when attacked by philosophers. Neither do we have to admit that our belief in the objectivity of mathematical truth is Platonic in the sense of requiring an ideal reality apart from human thought. Lakatos’ and Popper’s work shows that modern philosophy is capable of accepting the truth of mathematical experience. This means accepting the legitimacy of mathematics as it is: fallible, correctible, and meaningful.

Mathematical Intuition (Poincaré, Polya, Dewey)

**Summary.** Practical calculation of the limit of a sequence often violates the definition of convergence to a limit as taught in calculus. Together with examples from Euler, Polya and Poincaré, this fact shows that in mathematics, as in science and in everyday life, we are often obligated to use knowledge that is derived, not rigorously or deductively, but simply by making the best use of available information—plausible reasoning. The “philosophy of mathematical practice” fits into the general framework of “warranted assertibility,” the pragmatist view of the logic of inquiry developed by John Dewey.

**Keywords:** intuition, induction, pragmatism, approximation, convergence, limits, knowledge.

In Rio de Janeiro in May 2010, I spoke at a meeting of numerical analysts honoring the 80th anniversary of the famous paper by Courant, Friedrichs and Lewy. In order to give a philosophical talk appropriate for hard-core computer-oriented mathematicians, I focused on a certain striking paradox that is situated right at the heart of analysis, both pure and applied. (That paradox was presented, with considerable mathematical elaboration, in Phil Davis’s excellent article, “The Paradox of the Irrelevant Beginning.”) In order to make this paradox cut as sharply as possible, I performed a little dialogue, with help from Carlos Motta. With the help of Jody Azzouni, I used that dialogue again, to introduce this talk in Rome.

To set the stage, recall the notion of a convergent sequence, which is at the heart of both pure analysis and applied mathematics. In every calculus course, the student learns that whether a sequence converges to a limit, and what that limit is, depend only on the “end” of the sequence—that is, the part that is “very far out”—in the tail, so to speak, or in the infinite part. Yet, in a specific instance when the limit is actually needed, usually all that is considered is the beginning of the sequence—the first few terms—the finite part, so to speak. (Even if the calculation is carried out to a hundred or a thousand iterations, this is still only the first few, compared to the remaining, neglected, infinite tail.)

In this little drama of mine, the hero is a sincere, well-meaning student, who has not yet learned to accept life as it really is. A second character is the Successful Mathematician—the Ideal Mathematician’s son-in-law. His mathematics is ecumenical: a little pure, a little applied, and a little in-between. He has grants from federal agencies, a corporation here and there, and a private foundation or two. His conversation with the Stubborn Student is somewhat reminiscent of a famous conversation between his Dad, the Ideal Mathematician, and a philosophy grad student, who long ago asked, “What is a mathematical proof, really?”

The Successful Mathematician (SM) is accosted by the Stubborn Student (SS) from his Applied Analysis course.

**SS:** Sir, do you mind if I ask a stupid question?
SM: Of course not. There is no such thing as a stupid question.

SS: Right. I remember, you said that. So here’s my question. What is the real definition of “convergence”? Like, convergence of an infinite sequence, for instance?

SM: Well, I’m sure you already know the answer. The sequence converges to a limit, L, if it gets within a distance epsilon of L, and stays there, for any positive epsilon, no matter how small.

SS: Sure, that’s in the book, I know that. But then, what do people mean when they say, keep iterating till the iteration converges? How does that work?

SM: Well, it’s obvious, isn’t it? If after a hundred terms your sequence stays at 3, correct to four decimal places, then the limit is 3.

SS: Right. But how long is it supposed to stay there? For a hundred terms, for two hundred, for a hundred million terms?

SM: Of course you wouldn’t go on for a hundred million. That really would be stupid. Why would you waste time and money like that?

SS: Yes, I see what you mean. But what then? A hundred and ten? Two hundred? A thousand?

SM: It all depends on how much you care. And how much it is costing, and how much time it is taking.

SS: All right, that’s what I would do. But when does it converge?

SM: I told you. It converges if it gets within epsilon—

SS: Never mind about that. I am supposed to go on computing “until it converges,” so how am I supposed to recognize that “it has converged”?

SM: When it gets within four decimal points of some particular number and stays there.

SS: Stays there how long? Till when?

SM: Whatever is reasonable. Use your judgment! It’s just plain common sense, for Pete’s sake!

SS: But what if it keeps bouncing around within four decimal points and never gets any closer? You said any epsilon, no matter how small, not just point 0001. Or if I keep on long enough, it might finally get bigger than 3, even bigger than 4, way, way out, past the thousandth term.

SM: Maybe this, maybe that. We haven’t got time for all these maybes. Somebody else is waiting to get on that machine. And your bill from the computing center is getting pretty big.

SS: (mournfully) I guess you’re not going to tell me the answer.

SM: You just don’t get it, do you? Why don’t you go bother that Reuben Hersh over there, he looks like he has nothing better to do.
**SS:** Excuse me, Professor Hersh. My name is—

**RH:** That’s OK. I overheard your conversation with Professor Successful over there. Have a seat.

**SS:** Thank you. So, you already know what my question is.

**RH:** Yes, I do.

**SS:** So, what is the answer?

**RH:** He told you the truth. The real definition of convergence is exactly what he said, with the epsilon in it, the epsilon that is arbitrarily small but positive.

**SS:** So then, what does it mean, “go on until the sequence converges, then stop”?

**RH:** It’s meaningless. It’s not a precise mathematical statement. As a precise mathematical statement, it’s meaningless.

**SS:** So, if it’s meaningless, what does it mean?

**RH:** He told you what it means. Quit when you can see, when you can be pretty sure, what the limit must be. That’s what it means.

**SS:** But that has nothing to do with convergence!

**RH:** Right.

**SS:** Convergence only depends on the last part, the end, the infinite part of the sequence. It has nothing to do with the front part. You can change the first hundred million terms of the sequence, and that won’t affect whether it converges, or what the limit is.

**RH:** Right! Right! Right! You really are an A student.

**SS:** I know.... So it all just doesn’t make any sense. You teach us some fancy definition of convergence, but when you want to compute a number, you just forget about it and say it converges when common sense, or whatever you call it, says something must be the answer. Even though it might not be the answer at all!

**RH:** Excellent. I am impressed.

**SS:** Stop patronizing me. I’m not a child.

**RH:** Right. I will stop patronizing me, because you are not a child.

**SS:** You’re still doing it.

**RH:** It’s a habit. I can’t help it.

**SS:** Time to break a bad habit.

**RH:** OK. But seriously, you are absolutely right. I agree with every word you say.

**SS:** Yes, and you also agree with every word Professor Successful says.

**RH:** He was telling the truth, but he couldn’t make you understand.

**SS:** All right. **You** make me understand.

**RH:** It’s like theory and practice. Or the ideal and the actual. Or Heaven and Earth.
SS: How is that?
RH: The definition of convergence lives in a theoretical world. An ideal world. Where things can happen as long as we can clearly imagine them. As long as we can understand and agree on them. Like really being positive and arbitrarily small. No number we can write down is positive and arbitrarily small. It has to have some definite size if it is actually a number. But we can imagine it getting smaller and smaller and smaller while staying positive, and we can even express that idea in a formal sentence, so we accept it and work with it. It seems to convey what we want to mean by converging to a limit. But it’s only an ideal, something we can imagine, not something we can ever really do.
SS: So you’re saying mathematics is all a big fairy tale, a fiction, it doesn’t actually exist?
RH: NO! I never said fairy tale or fiction. I said imaginary. Maybe I should have said consensual. Something we can all agree on and work with, because we all understand it the same way.
SS: That’s cool. We all. All of you. Does that include me?
RH: Sure. Stay in school a few more years. Learn some more. You’ll get into the club. You’ve got what it takes.
SS: I’m not so sure. I have trouble believing two opposite things at once.
RH: Then how do you get along in daily life? How do you even get out of bed in the morning?
SS: What are you talking about?
RH: How do you know someone hasn’t left a bear trap by your bedside that will chop off your foot as soon as you step down?
SS: That’s ridiculous.
RH: It is. But how do you know it is?
SS: Never mind how I know. I just know it’s ridiculous. And so do you.
RH: Exactly. We know stuff, but we don’t always know how we know it. Still, we do know it.
SS: So you’re saying, we know that what looks like a limit really is a limit, even though we can’t prove it, or explain it, still we know it.
RH: We know it the same way you know nobody has left a bear trap by your bedside. You just know it.
SS: Right.
RH: But it’s still possible that you’re wrong. It is possible that something ridiculous actually happens. Not likely, not worth worrying about. But not impossible.
SS: Then math is really just like everything else. What a bummer! I like math because it’s not like everything else. In math, we know for sure. We prove things. One and one is two. Pi is irrational. A circle is round, not square. For sure.
RH: Then why are you upset? Everything is just fine, isn’t it?

SS: Why don’t you admit it? If you don’t have a proof, you just don’t know if L is the limit or not.

RH: That’s a fair question. So what is the answer?

SS: Because you really want to think you know L is the limit, even if it’s not true.

RH: Not that it’s not true, just that it might not be true.

End of dialogue

Thanks for your kind attention. What is supposed to be the meaning of this performance? What am I getting at? In this talk I am NOT attempting to make a contribution to the “problem of induction.” Therefore I may be allowed to omit a review of its 2,500-year literature. I am reporting and discussing what people really do, in practical convergence calculations, and in the process of mathematical discovery. I am going into a discussion of practical knowledge in mathematics, as a kind of real knowledge, even though it is not demonstrative or deductive knowledge. I try to explain why people must do what they do, in order to accomplish what they are trying to accomplish. I will conclude by arguing that the right broader context for the philosophy of mathematical practice is actually the philosophy of pragmatism, as expounded by John Dewey.

But first of all, just this remarkable fact. What we do when we want actual numbers may be totally unjustified, according to our theory and our definition. And even more remarkable—nobody seems to notice, or to worry about it!

Why is that? Well, the definition of convergence taught in calculus classes, as developed by those great men Augustin Cauchy and Karl Weierstrass, seems to actually convey what we want to mean by limit and convergence. It is a great success. Just look at the glorious edifice of mathematical analysis! On the other hand, in specific cases, it often is beyond our powers to give a rigorous error estimate, even when we have an approximation scheme that seems perfectly sound. As in the major problems of three-dimensional continuum mechanics with realistic nonlinearities, such as oceanography, weather prediction, stability of large complex structures like big bridges and airplanes....And even when we could possibly give a rigorous error estimate, it often would require great expenditure of time and labor. Surely it’s OK to just use the result of a calculation when it makes itself evident and there’s no particular reason to expect any hidden difficulty.

Here is an excerpt from an interview between the well-known numerical analyst, Phil Colella, and the leading mathematician Peter Lax, who is perhaps unique in standing at the forefront of both of the two different kinds of mathematics, theoretical and numerical. It is part of the oral history of numerical analysis, archived by the Society for Industrial and Applied Mathematics. [http://history.siam.org/pdfs2/Lax_final.pdf]

Colella: One of the unspoken rifts between the computational mathematicians or numerical mathematicians and the more traditional pure mathematicians, is that in computing there are, if not no proofs, precious few. And that makes us a breed apart, from the point of view of traditional—or what is viewed as traditional—mathematics. Is there some hope that maybe that rift will be closed as pure mathematicians use computers to manage their complexity as well?
Lax: Well, at one point there is really a different point of view. If you take, say, three-dimensional or even two-dimensional compressible-flow calculations in air, it produces complicated patterns. I don’t think we will ever be able to prove that this is within a prescribed epsilon of the true flow. That’s hopeless, and if there were such a proof it would be unreadable. It’s far more convincing that, say, a calculation carried out by an entirely different numerical method may give very similar results. And in that respect the computational mathematician does indeed differ from a theoretical mathematician. It’s a different style. I don’t think it needs to be reconciled. Von Neumann, who was certainly a marvelous theoretical mathematician, also enjoyed doing calculations. It never entered his head to prove rigorously that they were within epsilon [of the solution.]

In brief, we are virtually compelled by the practicalities to accept the number that computation seems to give us, even though, by the standards of rigorous logic, there is still an admitted possibility that we may be mistaken. This computational result is a kind of mathematical knowledge! It is practical knowledge, knowledge sound enough to be the basis of practical decisions about things like designing bridges and airplanes—matters of life and death.

In short, I am proclaiming that in mathematics, apart from and distinct from so-called deductive or demonstrative knowledge, there is also ordinary, fallible knowledge, of the same sort as our daily knowledge of our physical environment and our own bodies. “Anything new that we learn about the world involves plausible reasoning, which is the only kind of reasoning for which we care in everyday affairs.” (Polya, 1954). This sentence of his makes an implicit separation between mathematics and everyday affairs. But nowadays, in many different ways, for many different kinds of people, mathematics blends into everyday affairs. In these situations, the dominance of plausible over demonstrative reasoning applies even to mathematics itself, as in the daily labors of numerical analysts, applied mathematicians, design engineers... Controlling a rocket trip to the moon is not an exercise in mathematical rigor. It relies on a lack of malice on the part of that Being referred to by Albert Einstein as der lieber Gott.

(For fear of misunderstanding, I explain—this is not a confession of belief in a Supreme Being. It’s just Einstein’s poetic or metaphoric way of saying, Nature is not an opponent consciously trying to trick us.)

But it’s not only that we have no choice in the matter. It’s also that, truth to tell, it seems perfectly reasonable! Believing what the computation tells us is just what people have been doing all along, and (nearly always) it does seem to be OK. What’s wrong with that?

This kind of reasoning is sometimes called “plausible,” and sometimes called “intuitive.” I will say a little more about those two words pretty soon. But I want to draw your attention very clearly to two glaring facts about this kind of plausible or intuitive reasoning. First of all, it is pretty much the kind of reasoning that we are accustomed to in ordinary empirical science, and in technology, and in fact in everyday thinking, dealing with any kind of practical or realistic problem of human life. Secondly, it makes no claim to be demonstrative, or deductive, or conclusive, as is often said to be the essential characteristic of mathematical thinking. We are face to face with mathematical knowledge that is not different in kind from ordinary everyday commonplace human knowledge. Fallible! But knowledge, nonetheless!
Never mind the pretend doubt of philosophical skepticism. We are adults, not infants. Human adults know a lot! How to find their way from bed to breakfast—and people’s names and faces—and so forth and so on. This is real knowledge. It is not infallible, not eternal, not heavenly, not Platonic, it is just what daily life depends on, that’s all. That’s what I mean by ordinary, practical, everyday knowledge. Based not mainly on rigorous demonstration or deduction, but mainly on experience properly interpreted. And here we see mathematical knowledge that is of the same ordinary, everyday kind, based not on infallible deduction, but on fallible, plausible, intuitive thinking.

Then what justifies it in a logical sense? That is, what fundamental presupposition about the world, about reality, lies behind our willingness to commit this logical offense, of believing what isn’t proved?

I have already quoted the famous saying of Albert Einstein that supplies the key to unlocking this paradox.

My friend Peter Lax supplied the original German, I only remembered the English translation.

*Raffiniert ist der lieber Gott, aber boshaf ist Er nicht.*
*Subtle is the Lord God, but malicious is he not.*

Of course, Einstein was speaking as a physicist struggling to unravel the secrets of Nature. The laws of Nature are not always obvious or simple, they are often subtle. But we can believe, we *must* believe, that Nature is not set up to trick us, by a malicious opponent. God, or Nature, must be playing fair. How do we know that? We really *don’t* know it, as a matter of certainty! But we must believe it, if we seek to understand Nature with any hope of success. And since we *do have some success* in that search, our belief that Nature is subtle but not malicious is justified.

This problem of inferring generalizations from specific instances is known in logic as “the problem of induction.” My purpose is to point out that such generalizations in fact are made, and must be made, not only in daily life and in empirical science, but also in mathematics.

That is, in the practice of mathematics also we must believe that we are not dealing with a malicious opponent who is seeking to trick us. We experiment, we calculate, we draw diagrams. And eventually, using caution and the experience of the ages, we see the light. Gauss famously said, “I have my theorems. Now I have to find my proofs.”

But is it not naïve, for people who have lived through the hideous twentieth century, to still hope that God is not malicious? Consider, for example, a people who for thousands of years have lived safely on some atoll in the South Pacific. Today an unforeseen tsunami drowns them all. Might they not curse God in their last breath?

Here is an extensive quote from Leonhard Euler, by way of George Polya. Euler is speaking of a certain beautiful and surprising regularity in the sum of the divisors of the integers.

This law, which I shall explain in a moment, is in my opinion, so much more remarkable as it is of such a nature that we can be assured of its truth without giving it a perfect demonstration. Nevertheless, I shall present such evidence for it as
might be regarded as almost equivalent to a rigorous demonstration....anybody can satisfy himself of its truth by as many examples as he may wish to develop. And since I must admit that I am not in a position to give it a rigorous demonstration, I will justify it by a sufficiently large number of examples...I think these examples are sufficient to discourage anyone from imagining that it is by mere chance that my rule is in agreement with the truth...The examples that I have just developed will undoubtedly dispel any qualms which we might have had about the truth of my formula....it seems impossible that the law which has been discovered to hold for 20 terms, for example, would not be observed in the terms that follow. (Polya, 1954).

Observe two things about this quote from Euler. First of all, for him the plausible reasoning in this example is so irresistible that it leaves no room for doubt. He is certain that anyone who looks at his examples is bound to agree. Yet secondly, he strongly regrets his inability to provide a demonstration of the fact, and still hopes to find one.

But since he is already certain of the truth of his finding, why ask for a demonstrative proof? The answer is easy, for anyone familiar with mathematical work. The demonstration would not just affirm the truth of the formula, it would show why the formula MUST be true. That is the main importance of proof in mathematics! A plausible argument, relying on examples, analogy and induction, can be very strong, can carry total conviction. But if it is not demonstrative, it fails to show why the result MUST be true. That is to say, it fails to show that it is rigidly connected to established mathematics.

At the head of Chapter V of his (1954) Polya placed the following apocryphal quotation, attributed to “the traditional mathematics professor”: “When you have satisfied yourself that the theorem is true, you start proving it.” (Polya 1954)

This faith—that experience is not a trap laid to mislead us—is the unstated axiom. It lets us believe the numbers that come out of our calculations, including the canned programs that engineers use every day as black boxes. We know that it can sometimes be false. But even as we keep possible tsunamis in mind, we have no alternative but to act as if the world makes sense. We must continue to act on the basis of our experience. (Including, of course, experiences of unexpected disasters.)

Consider this recollection of infantile mathematical research by the famous physicist Freeman Dyson, who wrote in 2004:

One episode I remember vividly, I don’t know how old I was; I only know that I was young enough to be put down for an afternoon nap in my crib...I didn’t feel like sleeping, so I spent the time calculating. I added one plus a half plus a quarter plus an eighth plus a sixteenth and so on, and I discovered that if you go on adding like this forever you end up with two. Then I tried adding one plus a third plus a ninth and so on, and discovered that if you go on adding like this forever you end up with one and a half. Then I tried one plus a quarter and so on, and ended up with one and a third. So I had discovered infinite series. I don’t remember talking about this to anybody at the time. It was just a game I enjoyed playing. (Dyson 2004)
Yes, he knew the limit! How did he know it? Not the way we teach it in high school (by getting an exact formula for the sum of \( n \) terms of a geometric sequence, and then proving that as \( n \) goes to infinity, the difference from the proposed limit becomes and remains arbitrarily small.) No, just as when we first show this to tenth-graders, he saw that the sums follow a simple pattern that clearly is “converging” to 2. The formal, rigorous proof gives insight into the reason for a fact we have already seen plainly.

Can we go wrong this way? Certainly we can. Another quote from Euler.

There are even many properties of the numbers with which we are well acquainted, but which we are not yet able to prove; only observations have led us to their knowledge...the kind of knowledge which is supported only by observations and is not yet proved must be carefully distinguished from the truth; it is gained by induction, as we usually say. Yet we have seen cases in which mere induction led to error. Therefore, we should take great care not to accept as true such properties of the numbers which we have discovered by observation and which are supported by induction alone. Indeed, we should use such a discovery as an opportunity to investigate more exactly the properties discovered and to prove or disprove them. (Polya 1954, p. 3)

Notice how Euler distinguishes between “knowledge” and “truth”? He does say “knowledge,” not mere “conjecture.”

There is a famous theorem of Littlewood concerning a pair of number-theoretic functions \( \Pi(x) \) and \( \text{Li}(x) \). All calculation shows that \( \text{Li}(x) \) is greater than \( \Pi(x) \), for \( x \) as large as we can calculate. Yet Littlewood proved that eventually \( \Pi(x) \) becomes greater than \( \text{Li}(x) \), and not just once, but infinitely often! Yes, mathematical truth can be very subtle. While trusting it not to be malicious, we must not underestimate its subtlety. (\( \Pi(x) \) is the prime counting function and \( \text{Li}(x) \) is the logarithmic integral function.)

Mathematical Intuition

We are concerned with “the philosophy of mathematical practice.” Mathematical practice includes studying, teaching and applying mathematics. But I suppose we have in mind first of all the discovery and creation of mathematics— mathematical research. We start with Jacques Hadamard, go on to Henri Poincaré, move on to George Polya, and then to John Dewey.

Hadamard had a very long life and a very productive career. His most noted achievement (shared independently by de la Vallée Poussin) was proving the logarithmic distribution of the prime numbers. I want to recall a famous remark of Hadamard’s. “The object of mathematical rigor is to sanction and legitimize the conquests of intuition, and there never was any other object for it.” (Polya 1980)

From the viewpoint of standard “philosophy of mathematics,” this is a very surprising, strange remark. Isn’t mathematical rigor—that is, strict deductive reasoning—the most essential feature of mathematics? And indeed, what can Hadamard even mean by this word, “intuition”? A word that means one thing to Descartes, another thing to Kant. I think the philosophers of mathematics have pretty unanimously chosen to ignore this remark of Hadamard. Yet Hadamard did know a lot of mathematics, both rigorous and intuitive. And this remark was
quoted approvingly by both Borel and Polya. It seems to me that this bewildering remark deserves to be taken seriously.

Let’s pursue the question a step further, by recalling the famous essay “Mathematical Discovery,” written by Hadamard’s teacher, Henri Poincaré. (Poincaré 1952) Poincaré was one of the supreme mathematicians of the turn of the 19th and 20th century. We’ve been hearing his name recently, in connection with his conjecture on the 3-sphere, just recently proved by Grisha Perelman of St. Petersburg. Poincaré was not only a great mathematician, he was a brilliant essayist. And in the essay “Mathematical Discovery,” Poincaré makes a serious effort to explain mathematical intuition. He tells the famous story of how he discovered the Fuchsian and Theta-Fuchsian functions. He had been struggling with the problem unsuccessfully when he was distracted by being called up for military service:

At this moment I left Caen, where I was then living, to take part in a geological conference arranged by the School of Mines. The incidents of the journey made me forget my mathematical work. When we arrived at Coutances, we got into a bus to go for a drive, and, just as I put my foot on the step the idea came to me, though nothing in my former thoughts seemed to have prepared me for it, that the transformations I had used to define Fuchsian functions were identical with those of non-Euclidean geometry. I made no verification, and had no time to do so, since I took up the conversation again as soon as I had sat down in the bus, but I felt absolute certainty at once. When I got back to Caen, I verified the result at my leisure to satisfy my conscience. (Poincaré 1952)

What a perfect example of rigor “merely legitimizing the conquests of intuition”! How does Poincaré explain it? First of all, he points out that some sort of subconscious thinking must be going on. But if it is subconscious, he presumes it must be running on somehow at random. How unlikely, then, for it to find one of the very few good combinations, among the huge number of useless ones! To explain further, he writes:

If I may be permitted a crude comparison, let us represent the future elements of our combinations as something resembling Epicurus’s hooked atoms. When the mind is in complete repose these atoms are immovable; they are, so to speak, attached to the wall...On the other hand, during a period of apparent re-pose, but of unconscious work, some of them are detached from the wall and set in motion. They plough through space in all directions, like a swarm of gnats, for instance, or, if we prefer a more learned comparison, like the gaseous molecules in the kinetic theory of gases. Their mutual impacts may then produce new combinations. (Poincaré 1952)

The preliminary conscious work “detached them from the wall.” The mobilized atoms, he speculated, would therefore be “those from which we might reasonably expect the desired solution....My comparison is very crude, but I cannot well see how I could explain my thought in any other way.” (Poincaré 1952)

What can we make of this picture of “Epicurean hooked atoms,” flying about somewhere—in the mind? A striking, suggestive image, but one not subject even
in principle to either verification or disproof. Our traditional philosopher remains little interested. This is fantasy or poetry, not science or philosophy. But this is Poincaré! He knows what he’s talking about. He has something important to tell us. It’s not easy to understand, but let’s take him seriously, too.

To be fair, Poincaré proposed his image of gnats or gas molecules only after mentioning the possibility that the subconscious is actually more intelligent than the conscious mind. But this, he said, he was not willing to contemplate. However, other writers have proposed that the subconscious is less inhibited, more imaginative, more creative than the conscious. (Poincaré’s essay title is sometimes translated as “Mathematical Creation” rather than “Mathematical Discovery.”) David Hilbert supposedly once said of a student who had given up mathematics for poetry, “Good! He didn’t have enough imagination for mathematics.” Hadamard (1949) carefully analyzes the role of the subconscious in mathematical discovery and its connection with intuition. It is time for contemporary cognitive psychology to pay attention to Hadamard’s insights. See the reference below about current scientific work on the creative power of the subconscious.

Before going on, I want to mention the work of Carlo Cellucci, Emily Grosholz and Andrei Rodin. Cellucci strongly favors plausible reasoning, but he rejects intuition. However, the intuition he rejects isn’t what I’m talking about. He’s rejecting the old myth, of an infallible insight straight into the Transcendental. Of course I’m not advocating that outdated myth. Emily Grosholz, on the other hand, takes intuition very seriously. Her impressive historical study of what she calls “internal intuition” is in the same direction as my own thinking being presented here. Andrei Rodin has recently written a remarkable historical study of intuition (Rodin 2010). He shows that intuition played a central role in Lobachevsky’s non-Euclidean geometry, in Zermelo’s axiomatic set theory, and even in up-to-date category theory. (By the way, in category theory he could also have cited the standard practice of proof by “diagram chasing” as a blatant example of intuitive, visual proof.) His exposition makes the indispensable role of intuition clear and convincing. But his use of the term “intuition” remains, one might say, “intuitive,” for he offers no definition of the term, nor even a general description, beyond his specific examples.

Polya

My most helpful authority is George Polya. I actually induced Polya to come give talks in New Mexico, for previously, as a young instructor, I had met him at Stanford where he was an honored and famous professor. Polya was not of the stature of Poincaré or Hilbert, but he was still one of the most original, creative, versatile and influential mathematicians of his generation. His book with Gabor Szegö (Polya-Szegö 1970) made them both famous. It expounds large areas of advanced analytic function theory by means of a carefully arranged, graded sequence of problems with hints and solutions. Not only does it teach advanced function theory, it also teaches problem-solving. And by example, it shows how to teach mathematics by teaching problem-solving. Moreover, it implies a certain view of the nature of mathematics, so it is a philosophical work in disguise.

Later, when Polya wrote his very well-known, influential books on mathematical heuristic, he admitted that what he was doing could be regarded as having philosophical content. He writes, “I do not know whether the contents of these
four chapters deserve to be called philosophy. If this is philosophy, it is certainly a pretty low-brow kind of philosophy, more concerned with understanding concrete examples and the concrete behavior of people than with expounding generalities.” (Polya 1954 page viii) Unpretentious as Polya was, he was still aware of his true stature in mathematics. I suspect he was also aware of the philosophical depth of his heuristic. He played it down because, like most mathematicians (I can only think of one or two exceptions), he disliked controversy and arguing, or competing for the goal of becoming top dog in some cubbyhole of academia. The Prince of Mathematicians, Carl Friedrich Gauss, kept his monumental discovery of non-Euclidean geometry hidden in a desk drawer to avoid stirring up the Boeotians, as he called them,—meaning the post-Kantian German philosophy professors of his day. (In ancient Athens, “Boeotian” was slang for “ignorant country hick.”) Raymond Wilder was a leading topologist who wrote extensively on mathematics as a culture. He admitted to me that his writings implicitly challenged both formalism and Platonism. “Why not say so?” I asked. Because he didn’t relish getting involved in philosophical argument.

Well, how does Polya’s work on heuristic clarify mathematical intuition? Polya’s heuristic is presented as pedagogy. Polya is showing the novice how to solve problems. But what is “solving a problem”? In the very first sentence of the preface to (Polya 1980) he writes, “Solving a problem means finding a way out of a difficulty, a way around an obstacle, attaining an aim which was not immediately attainable. Solving problems is the specific achievement of intelligence, and intelligence is the specific gift of mankind: solving problems can be regarded as the most characteristically human activity.” “Problem” is simply another word for any project or enterprise which one cannot immediately take care of with the tools at hand. In mathematics, something more than a mere calculation. Showing how to solve problems amounts to showing how to do research!

Polya’s exposition is never general and abstract, he always uses a specific mathematical problem for the heuristic he wants to teach. His mathematical examples are always fresh and attractive. And his heuristic methods? First of all, there is what he calls “induction.” That is, looking at examples, as many as necessary, and using them to guess a pattern, a generalization. But be careful! Never just believe your guess! He insists that you must “Guess and test, guess and test.” Along with induction, there is analogy, and there is making diagrams, graphs and every other kind of picture, and then reasoning or guessing from the picture. And finally, there’s the “default hypothesis of chance”—that an observed pattern is mere coincidence.

(Mark Steiner has the distinction among philosophers of paying serious attention to Polya. After quoting at length from Polya’s presentation of Euler’s heuristic derivation of the sum of a certain infinite series, Steiner comes to an important conclusion: in mathematics we can have knowledge without proof! Based on the testimony of mathematicians, he even urges philosophers to pay attention to the question of mathematical intuition.)

I have two comments about Polya’s heuristic that I think he would have accepted. First of all, the methods he is presenting, by means of elementary examples, are methods he used himself in research. “In fact, my main source was my own research, and my treatment of many an elementary problem mirrors my experience with advanced problems.” (Polya, 1980, page xi). In teaching us how to solve problems, he’s teaching us about mathematical practice: How it works. What is
done. To find out "What is mathematics?" we must simply reinterpret Polya’s examples as descriptive rather than pedagogical.

Secondly, with hardly any stretching or adjustment, the heuristic devices that he’s teaching can be applied for any other kind of problem-solving, far beyond mathematics. He actually says that he is bringing to mathematics the kind of thinking ordinarily associated with empirical science. But we can go further. These ways of thinking are associated with every kind of problem-solving, in every area of human life! Someone needed to get across a river or lake and had the brilliant idea of “a boat”—whether it was a dugout log or a birch bark canoe. Someone else, needing shelter from the burning sun in the California Mojave, thought of digging a hole in the ground. And someone else, under the piercing wind of northern Canada, thought of making a shelter from blocks of ice.

How does anyone think of such a thing, solve such a concrete problem? By some kind of analogy with something else he has seen, or perhaps been told about. By plausible thinking. And often by a sudden insight that arises “from below.”

Intuitively, you might say.

**Mental Models**

It often happens that a concrete problem, whether in science or in ordinary daily life, is pressing on the mind, even when the particular materials or objects in question are not physically present. You keep on thinking about it, while you’re walking, and when you’re waking from sleep. Productive thought commonly takes place in the absence of the concrete objects or materials being thought about. This thinking about something not present to sight or touch can be called “abstract thinking.” Abstract thinking about a concrete object. How does that work? How can our mind/brain think productively about something that’s not there in front of the eyes? Evidently, it operates on something mental, what we may call a mental image or representation. In the current literature of cognitive psychology, one talks about “a mental model.” In this article, I use the term “mental model” to mean a mental structure built from recollected facts (some expressed in words), along with an ensemble of sensory memories, perhaps connected, as if by walking around the object in question, or by imagining the object from underneath or above, even if never actually seen in these views. A rich complex of connected knowledge and conjecture based on verbal, visual, kinesthetic, even auditory or olfactory information, but simplified, to exclude irrelevant details. Everything that’s helpful for thinking about the object of interest when the object isn’t here. Under the pressure of a strong desire or need to solve a specific problem, we assemble a **mental model** which the mind-brain can manipulate or analyze.

Subconscious thinking is not a special peculiarity of mathematical thinking, but a common, taken-for-granted, part of every-day problem-solving. When we consider this commonplace fact, we aren’t tempted to compare it to a swarm of gnats hooking together at random. No, we assume, as a matter of course, that this subconscious thinking follows rules, methods, habits or pathways, that somehow, to some extent, correspond to the familiar plausible thinking we do when we’re wide awake. Such as thinking by analogy or by induction. After all, if it is to be productive, what else can it do? If it had any better methods, then those better methods would also be what we would follow in conscious thinking! And subconscious thinking in mathematics must be much like subconscious thinking in
any other domain, carrying on plausible reasoning as enunciated by various writers, above all by George Polya. This description of subconscious thinking is not far from Michael Polanyi’s “tacit dimension.”

When applied to everyday problem solving, all this is rather obvious, perhaps even banal. My goal is to clarify mathematical intuition, in the sense of Hadamard and Poincaré. “Intuition” in the sense of Hadamard and Poincaré is a fallible psychological experience that has to be accounted for in any realistic philosophy of mathematics. It simply means guesses or insights attained by plausible reasoning, either fully conscious or partly subconscious. In this sense it is a specific phenomenon of common experience. It has nothing to do with the ancient mystical myth of an intuition that surpasses logic by making a direct connection to the Transcendental.

The term “abstract thinking” is commonplace in talk about mathematics. The triangle, the main subject of Euclidean geometry, is an abstraction, even though it’s idealized from visible triangles on the blackboard. Thinking of a physical object in its absence, like a stream to be crossed or a boat to be imagined and then built, is already “abstract” thinking, and the word “abstract” connects us to the abstract objects of mathematics.

Let me be as clear and simple as I can be about the connection. After we have some practice drawing triangles, we can think about triangles, we discover properties of triangles. We do this by reasoning about mental images, as well as images on paper. This is already abstract thinking. When we go on to regular polygons of arbitrarily many sides, we have made another departure. Eventually we think of the triangle as a 2-simplex, and abstract from the triangle to the n-simplex. For $n = 3$ this is just the tetrahedron, but for $n = 4$ or 5 or 6, it is something never yet seen by human eye. Yet these higher simplexes also can become familiar, and, as it were, concrete-seeming. If we devote our waking lives to thinking about them, then we have some kind of “mental model” of them. Having this mental model, we can access it, and thereby we can reason intuitively—have intuitive insights—by which I mean simply insights not based on consciously known reasoning. An “intuition” is then simply a belief (possibly mistaken!) arising from internal inspection of a mental image or representation—a “model.” It may be assisted by subconscious plausible reasoning, based on the availability of that mental image. We do this in practical life. We do it in empirical science, and in mathematics. In empirical science and ordinary life, the image may stand for either an actual object, a physical entity, or a potential one that could be realized physically. In mathematics, our mental model is sometimes idealized from a physical object—for example, from a collection of identical coins or buttons when we’re thinking about arithmetic. But in mathematics we also may possess a mental model with no physical counterpart. For example, it is generally believed that Bill Thurston’s famous conjectures on the classification of four-manifolds were achieved by an exceptional ability, on the part of Thurston, to think intuitively in the fourth dimension. Perhaps Grisha Perelman was also guided by some four-dimensional intuition, in his arduous arguments and calculations to prove the Thurston program.

To summarize, mathematical intuition is an application of conscious or subconscious heuristic thinking of the same kind that is used every day in ordinary life by ordinary people, as well as in empirical science by scientists. This has been said before, by both Hadamard and Polya. In fact, this position is similar to Kurt
Gödel’s, who famously wrote, “I don’t see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception.” Why, indeed? After all, both are fallible, but both are plausible, and must be based on plausible reasoning.

For Gödel, however, as for every writer in the dominant philosophy of mathematics, intuition is called in only to justify the axioms. Once the axioms are written down, the role of mathematical intuition is strictly limited to “heuristic”—to formulating conjectures. These await legitimation by deductive proof, for only deductive proof can establish “certainty.” Indeed, this was stated as firmly by Polya as by any analytic philosopher. But what is meant by “mathematical certainty”? If it simply means deductive proof, this statement is a mere circular truism. However, as I meant to suggest by the little dialog at the beginning of this paper, there is also practical certainty, even within mathematics! We are certain of many things in ordinary daily life, without deductive proof, and this is also the case in mathematics itself. Practical certainty is a belief strong enough to lead to serious practical decisions and actions. For example, we stake our lives on the numerical values that went into the engineering design of an Airbus or the Golden Gate Bridge. Mainstream philosophy of mathematics does not recognize such practical certainty. Nevertheless, it is an undeniable fact of life.

It is a fact of life not only in applied mathematics but also in pure mathematics. For example, the familiar picture of the Mandelbrot set, a very famous bit of recent pure mathematics, is generated by a machine computation. By definition, any particular point in the complex plane is inside the Mandelbrot set if a certain associated iteration stays bounded. If that iteration at some stage produces a number with absolute value greater than two, then, from a known theorem, we can conclude that the iteration goes to infinity, and the parameter point in question is outside the Mandelbrot set. What if the point is inside the Mandelbrot set? No finite number of iterations in itself can guarantee that the iteration will never go beyond absolute value 2. If we do eventually decide that it looks like it will stay bounded, we may be right, but we are still cheating. This decision is opportunistic and unavoidable, just as in an ordinary calculation about turbulent flow.

Computation (numerics) is accepted by purists only as a source of conjectures awaiting rigorous proof. However, from the pragmatic, non-purist viewpoint, if numerics is our guide to action, then it is in effect a source of knowledge. Dewey called it “warranted assertibility.” (Possibly even a “truth.” A “truth” that remains open to possible reconsideration.)

Another example from pure mathematics appeared on John Baez’s blog (Baez 2010) where it is credited to Sam Derbyshire. His pictures plot the location in the complex plane of the roots of all polynomials of degree 24 with coefficients plus one or minus one. The qualitative features of these pictures are absolutely convincing—i.e., impossible to disbelieve. Baez wrote, “That’s $2^{24}$ polynomials, and about $24 \times 2^{24}$ roots—or about 400 million roots! It took Mathematica 4 days to generate the coordinates of the roots, producing about 5 gigabytes of data.” (Figure 1 shows the part of the plot in the first quadrant, for complex roots with non-negative real and imaginary parts.)

There is more information in this picture than can even be formulated as conjectures, let alone seriously attacked with rigor. Since indeed we cannot help believing them (perhaps only believing with 99.999% credence) then (pragmatically)
we give them “warranted assertibility,” just like my belief that I can walk out my
door without encountering sudden death in one form or another. The distinction
between rigorous math and plausible math, pure math and applied math, etc, be-
comes blurred. It is still visible, certainly, but not so sharp. It’s a little fuzzy.
Purely computational results in pure mathematics, when backed up by sophisti-
cated checking against a relevant theory, have a factual status similar to that of
accepted facts from empirical science. The distinction between what is taken to be
“known,” and what is set aside as merely guessed or “conjectured”, is not so cut
and dried as the usual discussions claim to believe.

Mental Models Subject to Social Control

“Plausible” or “heuristic” thinking is applied, either consciously or subcon-
sciously, to mental models. These mental models may correspond to tangible or
visible physical objects in ordinary life and empirical science. Or they may not cor-
respond to any such things, but may be pure mental representations, as in much of
contemporary analysis, algebra, and even geometry. By pure mental models I mean
models not obtained directly by idealization of visual or other sensual experience.

But what controls these mental models? If they have no physical counterpart,
what keeps them from being wildly idiosyncratic and incommunicable? What we
have omitted up to this point, and what is the crux of the matter: mathematical

Figure 1.
images are not private, individual entities. From the origin of mathematics in bartering, buying and selling, or in building the Parthenon and the Pyramids, this subject has always been a social, an “inter-subjective” activity. Its advances and conquests have always been validated, corrected and absorbed in a social context—first of all, in the classroom. Mathematicians can and must talk to each other about their ideas. One way or the other, they do communicate, share and compare their conceptions of mathematical entities, which means precisely these models, these images and representations I have been describing. Discrepancies are recognized and worked out, either by correcting errors, reconciling differences, or splitting apart into different, independent pathways. Appropriate terminology and symbols are created as needed.

Mathematics depends on a mutually acknowledging group of competent practitioners, whose consensus decides at any time what is regarded as correct or incorrect, complete or incomplete. That is how it always worked, and that is how it works today. This was made very clear by the elaborate process in which Perelman’s proposed proof of the Thurston program (including the Poincaré conjecture) was vetted, examined, discussed, criticized and finally accepted by the “Ricci flow community,” and then by its friends in the wider communities of differential geometry and low-dimensional topology, and then by the prize committees of the Fields Medal and the Clay Foundation.

Thus, when we speak of a mathematical concept, we speak not of a single isolated mental image, but rather of a family of mutually correcting mental images. They are privately owned, but publicly checked, examined, corrected, and accepted or rejected. This is the role of the mathematical research community, how it indoctrinates and certifies new members, how it reviews, accepts or rejects proposed publication, how it chooses directions of research to follow and develop, or to ignore and allow to die. All these social activities are based on a necessary condition: that the individual members have mental models that fit together, that yield the same answers to test questions. A new branch of mathematics is established when consensus is reached about the possible test questions and their answers. That collection of possible questions and answers (not necessarily explicit) becomes the means of accepting or rejecting proposed new members.

If two or three mathematicians do more than merely communicate about some mathematical topic, but actually collaborate to dig up new information and understanding about it, then the matching of their mental models must be even closer. They may need to establish a congruence between their subconscious thinking about it as well as their conscious thinking. This can be manifested when they are working together, and one speaks the very thought that the partner was about to speak.

And to the question “What is mathematics?” the answer is “It is socially validated reasoning about these mutually congruent mental models.”

What makes mathematics possible? It is our ability to create mental models which are “precise,” meaning simply that they are part of a shared family of mutually congruent models. In particular, such an image as a line segment, or two intersecting line segments, and so on. Or the image of a collection of mutually interchangeable identical objects (ideal coins or buttons). And so on. To understand better how that ability exists, both psychologically and neurophysiologically, is a worthy goal for empirical science. The current interactive flowering of developmental psychology, language acquisition, and cognitive neuroscience shows that
this hope is not without substance. (See, e.g., Carey, Dehaene, Johnson-Laird, Lakoff/Nunez, Zwaan.)

The existence of mathematics shows that the human mind is capable of creating, refining, and sharing such precise concepts, which admit of reasoning that can be shared, mutually checked, and confirmed or rejected. There are great variations in the vividness, completeness, and connectivity of different mental images of the “same” mathematical entity as held by different mathematicians. And, also great variations in their ability to concentrate on that image and squeeze out all of its hidden information. Recall that well-known mathematician, Sir Isaac Newton. When asked how he made his discoveries in mathematics and physics, he answered simply, “By keeping the problem constantly before my mind, until the light gradually dawns.” Indeed, neither meals nor sleep were allowed to interrupt Newton’s concentration on the problem. Mathematicians are notoriously absent-minded. Their concentration, which outsiders call “absent-mindedness,” is just the open secret of mathematical success.

Their reasoning is qualitatively the same as the reasoning carried out by a hunter tracking a deer in the Appalachian woodland a thousand years ago. “If the deer went to the right, I would see a hoof print here. But I don’t see it. There’s only one other way he could have gone. So he must have gone to the left.” Concrete deductive reasoning, which is the basis for abstract deductive reasoning.

To sum up! I have drawn a picture of mathematical reasoning which claims to make sense of intuition according to Hadamard and Poincaré, and which interprets Polya’s heuristic as a description of ordinary practical reason, applied to the abstract situations and problems of mathematics, working on mental models in the same way that ordinary practical reasoning in absentia works on a mental model. (We may assist our mental images by creating images on paper—drawing pictures—that to some extent capture crucial features of the mental images.)

Dewey and Pragmatism

Before bringing in John Dewey, the third name promised at the beginning, I must first mention Dewey’s precursor in American pragmatism, Charles Saunders Peirce, for Peirce was also a precursor to Polya. To deduction and induction, Peirce added a third logical operation, “abduction,” something rather close to Polya’s “intelligent guessing.”

The philosophy of mathematics as practiced in many articles and books is a thing unto itself, hardly connected either to living mathematics or to general philosophy. But how can it be claimed that the nature of mathematics is unrelated to the general question of human knowledge? There has to be a fit between your beliefs about mathematics and your beliefs about science and about the mind. I claim that Dewey’s pragmatism offers the right philosophical context for the philosophy of mathematical practice to fit into. I am thinking especially of Logic—the Theory of Inquiry. For Dewey, “inquiry” is conceived very broadly and inclusively. It is “the controlled or directed transformation of an indeterminate situation into one that is so determinate in its constituent distinctions and relations as to convert the elements of the original situation into a unified whole.” So broadly understood, inquiry is one of the primary attributes of our species. Only because of that trait have we survived, after we climbed down from the trees. I cannot help comparing Dewey’s definition of inquiry with Polya’s definition of problem solving. It seems
to me they are very much pointing in the same direction, taking us down the same track. With the conspicuous difference that, unlike Dewey, Polya is concise and memorable.

Dewey makes a radical departure from standard traditional philosophy (following on from his predecessors Peirce and William James, and his contemporary George Herbert Mead). He does not throw away the concept of truth, but he gives up the criterion of truthfulness, as the judge of useful or productive thinking. Immanuel Kant made clear once and for all that while we may know the truth, we cannot know for certain that we do know it. We must perforce make the best of both demonstrative and plausible reasoning. This seems rather close to “warranted assertibility,” as Dewey chooses to call it. But Polya or Poincaré are merely talking about mathematical thinking, Dewey is talking about human life itself.

What about deductive thinking? From Dewey’s perspective of “warranted assertibility,” deductive proof is not a unique, isolated mode of knowledge. A hunter tracking a deer in the North American woodland a thousand years ago concluded, “So it must have gone to the left.” Concrete deductive reasoning, the necessary basis of theoretical deductive reasoning. And it never brings certainty, simply because any particular deductive proof is a proof in practice, not in principle. Proof in practice is a human artifact, and so it can’t help leaving some room for possible question, even possible error. (And that remains true of machine proof, whether by analog, digital, or quantum computer. What changes is the magnitude of the remaining possible error and doubt, which can never vanish finally.) In this way, we take our leave, once and for all, of the Platonic ideal of knowledge—indubitable and unchanging—in favor, one might say, of an Aristotelian view, a scientific and empirical one. And while deductive proof becomes human and not divine or infallible, non-deductive plausible reasoning and intuition receive their due as a source of knowledge in mathematics, just as in every other part of human life. Dewey’s breadth of vision—seeing philosophy always in the context of experience, that is to say of humanity at large—brings a pleasant breath of fresh air into this stuffy room.

Nicholas Rescher (2001) writes,

The need for understanding, for ‘knowing one’s way about,’ is one of the most fundamental demands of the human condition....Once the ball is set rolling, it keeps on going under its own momentum—far beyond the limits of strictly practical necessity....The discomfort of unknowing is a natural aspect of human sensibility. To be ignorant of what goes on about us is almost physically painful for us...The requirement for information, for cognitive orientation within our environment, is as pressing a human need as for food itself. (Rescher 2001)

The need for understanding is often met by a story of some kind. In our scientific age, we expect a story built on a sophisticated experimental-theoretical methodology. In earlier times, no such methodology was available, and a story might be invented in terms of gods or spirits or ancestors. In inventing such explanations, whether in what we now call mythology or what we now call science, people have always been guided by a second fundamental drive or need. Rescher does not mention it, but Dewey does not leave it out. That is the need to impart form, beauty, appealing shape or symmetry to our creations, whether they be straw
baskets, clay pots, wooden spears and shields, or geometrical figures and algebraic calculations. In *Art as Experience* Dewey shows that the esthetic, the concern for pleasing form, for symmetry and balance, is also an inherent universal aspect of humanity. In mathematics this is no less a universal factor than the problem-solving drive. In “Mathematical Discovery” Poincaré takes great pains to emphasize the key role of esthetic preference in the development of mathematics. We prefer the attractive looking problems to work on, we strive for diagrams and graphs that are graceful and pleasing. Every mathematician who has talked about the nature of mathematics has portrayed it as above all an art form. So this is a second aspect of pragmatism that sheds light on mathematical practice.

Rescher’s careful development omits mathematical knowledge and activity. And Dewey himself doesn’t seem to have been deeply interested in the philosophy of mathematics, although there are interesting pages about mathematics in *Logic*, as well as in his earlier books *The Quest for Certainty* and *The Psychology of Number*. He may have been somewhat influenced by the prevalent view of philosophy of mathematics as an enclave of specialists, fenced off both from the rest of philosophy and from mathematics itself.

But if we take these pragmatist remarks of Rescher’s seriously and compare them to what mathematicians do, we find a remarkably good fit. Just as people living in the woodland just naturally want to know and find out about all the stuff they see growing—what makes it grow, what makes it die, what you can do with it to make a canoe or a tent—so people who get into the world of numbers, or the world of triangles and circles, just naturally want to know how it all fits together, and how it can be stretched and pulled this way or that. “Guess and test,” is the way George Polya put it. “Proofs and refutations” was the phrase used by another mathematically trained Hungarian philosopher, following up an investigation started by Polya. Whichever way you want to put it, it is nothing more or less than the exploration of the mathematical environment, which we create and expand as we explore it. We are manifesting in the conceptual realm one of the characteristic behaviors of *homo sapiens*.

Even though we lack claws or teeth to match beasts of prey, or fleetness to overtake the deer, or heavy fur or a thick shell, we long ago adapted to virtually every environment on Earth. We invented swimming, paddling and sailing, cooking and brewing and baking and preserving, and we expanded our social groups from families to clans to tribes to kingdoms to empires. All this by “inquiry,” or by problem-solving. Dewey shows that this inquiry is an innate specific drive or need of our species. It was manifested when, motivated by practical concerns, we invented counting and the drawing of triangles. That same drive, to find projects, puzzles, and directions for growth, to make distinctions and connections, and then again make new distinctions and new connections, has resulted in the Empire of mathematics we inhabit today.

**References**


Abstract: In three experiments, the relation between different modes of thought and the generation of “creative” and original ideas was investigated. Participants were asked to generate items according to a specific instruction (e.g., generate place names starting with an “A”). They either did so immediately after receiving the instruction, or after a few minutes of conscious thought, or after a few minutes of distraction during which “unconscious thought” was hypothesized to take place. Throughout the experiments, the items participants listed under “unconscious thought” conditions were more original. It was concluded that whereas conscious thought may be focused and convergent, unconscious thought may be more associative and divergent.

Acknowledgments

In this work I benefited from suggestions and criticisms by Carlo Cellucci, Richard Epstein, Russell Goodman, Cleve Moler, Peter Lax, Ulf Persson, Vera John-Steiner, and members of the study group on mathematical thinking in Santa Fe, New Mexico.
To Establish New Mathematics, We Use Our Mental Models And Build On Established Mathematics

To appear in From an heuristic point of view, in honor of Carlo Cellucci

Keywords: proof, mental models, semantics, mathematical practice, warranted assertibility, truth

Abstract Mathematician’s proof doesn’t start from a pre-ordained set of axioms. It starts from relevant pieces of established mathematics out of which some new mathematical result can be derived and incorporated into established mathematics. Our reasoning is not syntactic but semantic. We use our mental models of mathematical entities, which are culturally controlled to be mutually congruent within the research community. These socially controlled mental models provide the much-desired “semantics” of mathematical reasoning.

Introduction

At last, in the 21st century, the “maverick” topic of mathematical practice arrived as a legitimate theme of philosophical investigation. Carlo Cellucci attended to mathematical practice, and challenged outworn philosophical clichés. Paolo Mancosu edited a collection entitled The Philosophy of Mathematical Practice. An Association for the Philosophy of Mathematical Practice was organized, and published two issues of the journal Erkenntnis.

But philosophical writing on mathematical practice still struggles to get a grip. Before one philosophizes on mathematical practice, one might wish to find out, What do real mathematicians really do? This article reports on the mathematical practice of actual mathematicians. It focuses on proof—the “front side” of mathematics. But in the course of our report, we must also look at the back side—heuristics, or “the analytic method” (Hersh, 1988; Cellucci, 2008).

I will quote from Andrew Wiles’ proof of Fermat’s Last Theorem (FLT), but I start off with Vaughan Jones (of the “Jones polynomial” renowned in knot theory) and Bill Thurston (who cracked open four-dimensional topology, by grounding it in three-dimensional non-Euclidean hyperbolic geometry). At a conference in Sicily in 1995 on “Truth in Mathematics,” Jones told of the wonderful properties of the Fourier transform, and listed a few of the many fields in mathematics and physics where it is essential. He said, “To doubt the ’truth’ of the Fourier transform, however the word ’truth’ be interpreted, would be mathematical lunacy…. The mathematician is as certain of his faith in mathematics as he is in the fact that a ball will drop if held above the ground and released—more sure than that the sun will rise the next morning.” (Dales 203, 205) Jones backed up his claims with interesting examples from braid theory.
At that same conference the illustrious number theorist and algebraic geometer Yuri Manin (author of a beautiful textbook on logic) quoted the following confessions of Bill Thurston:

"When I started as a graduate student at Berkeley, I had trouble imagining how I could ‘prove’ a new and interesting mathematical theorem. I didn’t really understand what a ‘proof’ was. By going to seminars, reading papers, and talking to other graduate students, I gradually began to catch on. Within any field, there are certain theorems and certain techniques that are generally known and generally accepted. When you write a paper, you refer to them without proof. You look at other papers in the field, and you see what facts they quote without proof, and what they cite in their bibliography. You learn from other people some idea of their proofs. Then you’re free to quote the same theorem and cite the same citations.”

(Dales, 152.)

Some philosophers may find these testimonies strange, but they will strike mathematicians as commonplace. In order to explicate them, I will spell out the concept of “Mathematicians’ Proof”: proof as it is understood by mathematicians.

Wiles’ proof of FLT isn’t an axiomatic proof, it’s a “Mathematicians’ Proof”

In 1637, in the margin of a copy of Diophantus’ *Arithmetica*, Pierre de Fermat wrote in Latin: “It is impossible for a cube to be written as a sum of two cubes or a fourth power to be written as the sum of two fourth powers, or, in general, for any number which is a power greater than the second to be written as a sum of two like powers. I have a truly marvelous demonstration of this proposition which this margin is too narrow to contain.” This is “Fermat’s last theorem” (FLT). Fermat’s proof of it was never found, but it was verified and checked for all powers of $n$ up to 4,000,000. For over three and a half centuries, it was the most famous open problem in mathematics. Around 1964, a young Japanese mathematician, Goro Shimura, published a remarkable conjecture, which became known as the Taniyama-Shimura conjecture (TS): “Every elliptic curve defined over the rational number field is uniformized by modular functions.” This very bold and startling conjecture has no apparent relation to FLT. It asserts an intimate connection between two seemingly unrelated subjects: algebraic geometry, where elliptic curves are a major topic, and function theory, where the modular functions are central and classical. But then, in the 1970’s, the German number theorist Gerhard Frey found reason to surmise that from TS one could prove FLT! An outline of a possible proof of Frey’s surmise was offered in 1985 by the famous mathematician Jean-Paul Serre. And in August of the following year, Ken Ribet of Berkeley proved the lemmas in Serre’s proposal! Thus it was established: FLT would indeed be proved, if only TS were proved. That final step was made by a Princeton professor who had been obsessed with FLT since the age of 10. Andrew Wiles realized that to obtain FLT, the full Taniyama-Shimura conjecture is unnecessary, it is enough to prove TS just for “semistable” elliptic curves. And that is what Andrew Wiles did, with help from his student Richard Taylor. (See Mozzochi and Gowers) Wiles’ paper uses a great variety of sophisticated mathematical ideas. Nevertheless, an outsider can read the introduction, to glimpse what he did and how he did it.

(Carlo Cellucci (2008) raised an interesting objection. It wasn’t Wiles, he argued, it was Ribet who proved FLT, by deriving FLT from TS. Wiles merely
proved TS, not FLT itself. Ken Ribet himself certainly never would claim any such credit, for merely deriving FLT from an unproved conjecture. Yet, strangely enough, if the order of events had been different, if Wiles had first proved TS, and Ribet had then derived FLT from TS, it would indeed have been Ribet who had the glory of “proving FLT”! That is how mathematicians assign credit. The first mountaineer to stand atop Mount Everest gets the glory, even though everyone knows that his climb was merely the last in a long sequence of preparations. Sad to say, criticism from a philosopher will not change the way mathematicians assign credit.)

In Wiles’ proof, as in all mathematical research, two aspects are intricately intertwined: the heuristic or “problem-solving”, and the “rigorous” or deductive. The researcher trying to find a rigorous proof uses plausible or heuristic reasoning in order to find it, so the heuristic and rigorous aspects of mathematical research are inseparable. (George Polya’s deep and entertaining accounts of mathematical heuristics (1925, 1945, 1954, 1980) unfortunately have been ignored by philosophers, because they are presented as pedagogy, not philosophy. For more on Polya, see my article (2011) and Frank (2004)).

If you believe that a mathematical proof is supposed to start with some unproved “axioms”, you might be puzzled and frustrated by Wiles’ work. No axioms in sight! (The same is true of most mathematical research publication, all the way back to the Géométrie of René Descartes.) Some sentences from Wiles’ paper will convey a sense of how it reads.

“The key development in the proof is a new and surprising link between two strong but distinct traditions in number theory, the relationship between Galois representations and modular forms on the one hand and the interpretation of special values of L-functions on the other. The former tradition is of course more recent..... The second tradition goes back to the famous analytic class number formula of Dirichlet, but owes its modern revival to the conjecture of Birch and Swinnerton-Dyer. In practice, however, it is the ideas of Iwasawa in this field on which we attempt to draw, and which to a large extent we have to replace.....The turning point in this and indeed in the whole proof came in the spring of 1991. In searching for a clue from commutative algebra I had been particularly struck some years earlier by a paper of Kunz [62]..... It was only on reading Section 6 of [121] that I learned that it followed from Tate’s account of Grothendieck duality theory for complete intersections that these two invariants were equal for such rings. The impact of this result on the main problem was enormous..... Then in August, 1991 I learned of a new construction of Flach [35]..... Then, unexpectedly in May, 1993, on reading of a construction of twisted forms of modular curves in a paper of Mazur [71] I made a crucial and surprising breakthrough. ....Believing now that the proof was complete, I sketched the whole theory in three lectures in Cambridge, England. .....” (Wiles 1995)

One doesn’t have to be familiar with L-functions and so on to see that this proof depends on a vast acquaintance and deep understanding of “the literature”—the relevant parts of established mathematics.

Established mathematics

Established mathematics is the body of mathematics that is accepted as the basis for mathematicians’ proofs. It includes proved statements “in the literature,”
and also some simpler statements that are so well accepted that no literature reference is expected. The central core of established mathematics includes not only arithmetic, geometry, linear and polynomial algebra, and calculus, but also the elements of function theory, group theory, topology, measure theory, Banach and Hilbert spaces, and differential equations—the usual first two years of graduate study. And then to create new mathematics, one must also master major segments of the established knowledge in some special area.

Every mathematician has mastered a substantial portion of established mathematics, and has complete confidence in it. He/she could not be qualified, accepted or recognized as a mathematician without attaining such confidence and mastery. Confidence in established mathematics is for a mathematician as indispensable as confidence in the mechanics of a piano for a piano virtuoso, or confidence in the properties of baseballs and bats for a big league baseball player. If you’re worried about that, you aren’t even in the game.

Established mathematics is an intricately interconnected web of mutually supporting concepts, which are connected both by plausible and by deductive reasoning. Starting from numbers and elementary geometry, we have built a fantastically elaborated and ramified collection of concepts, algorithms, theories, axiom systems, examples, conjectures, and open problems. It provides most of the “models” or “applications” that are in daily use, in the marketplace, the bank, and the interplanetary rocket control center. Deductive proof, mutually supporting interconnections, and close interaction with social life (commerce, technology, education) all serve to warrant the assertions of established mathematics. Deductive proof is the principal and most important warrant. Publication of a research result means adding something new to the body of established mathematics.

How is the established core established? We mathematicians do remember once having seen the Fourier transform proved, by means of some more elementary parts of algebra and calculus, but, as Vaughan Jones proclaims, that’s not the most important part of why we believe in the Fourier transform. And where does our belief in elementary algebra and calculus come from? Not from axiomatic set theory (Zermelo and Fraenkel), nor from Dedekind and Peano’s axioms of the natural numbers. For centuries before Dedekind, Peano, Zermelo or Fraenkel were born, the practice of arithmetic, algebra and calculus had been firmly established. In actual mathematical practice, mathematicians start from a given, a basis, which we accept as firmly as we accept the reality of the physical and social worlds. As Jones said, to question the Fourier transform, or any other standard mathematical tool, would be “mathematical lunacy.” Established mathematics is “warranted” by common consent based on shared experience, and reinforced by its logical and intuitive connection to basic arithmetic.

The body of established mathematics is not a fixed or static set of statements. The new and recent part is in transition. A piece of mathematics first arises in the course of a mathematician’s work as new knowledge. It gains status as he/she communicates it to his closest community. If it has any interest to others, it spreads by word of mouth and electronic media. It is written up for publication, spread around the internet, and after scrutiny by referees, may be published, in print or on line. After that it may fall into obscurity, or it may became firmly established, generally known and used. What is firmly established varies both in time and in
space, because people in New York, Paris or Moscow may choose different tools and ideas from established mathematics.

What justifies established mathematics? Doesn’t it have to have a foundation? No, it doesn’t. Providing a foundation for established mathematics has fascinated many people, but it is not a necessity. Although arithmetic can be derived from set theory, arithmetic does not rely on set theory as a foundation. Arithmetic is a well-established ancient practice, closely linked to visual and tactile interpretations, and essential for government, commerce, and industry. Accepting it is a standard criterion of sanity. To a lesser degree, similar things can be said about ordinary plane and solid geometry and ordinary school algebra.

Established mathematics is established on the basis of history, social practice, and internal coherence. The more recent parts were established on the basis of rigorous proofs based on the older parts. It doesn’t need a foundation, it IS the foundation for what the mathematician is trying to do: to build on it.

What has been published remains subject to criticism or correction. The status of established mathematics is not absolute truth, but rather, warranted assertibility. In the higher reaches of this structure, (for example the nonlinear partial differential equations of evolving continuous media) the situation is rather different from that in elementary arithmetic, and becomes to some extent analogous to the situation in empirical science. In a mathematical model for physics or engineering, while of course it is hoped that mathematical reasoning will enlighten the physical problem, it also happens that physical reasoning enlightens a mathematical problem. At the advanced research level, where sometimes only a handful of experts have the ability and the motivation to check a proof or a derivation, mathematics does not have quite the same status of virtual certainty as in elementary arithmetic. We strive to understand strange structures for which our intuition fails. We check until we feel we have checked as well as possible, and have found no loopholes. A notorious example is the classification of finite simple groups. The leading organizer of that research community compared their achievement—a complete list of the finite simple groups—to flying over Antarctica, trying to make sure you haven’t missed any mountain tops (Gorenstein). It’s quite unlikely that you would have overlooked one. A kind of warranted assertibility.

**Mathematicians’ proof vs. axiomatic proof**

Let me display as sharply as possible the difference between this kind of proof, the kind produced by Andrew Wiles, which I am calling “mathematicians’ proof”, and the kind of proof, going back to Aristotle, called *axiomatic proof*. An axiomatic proof is supposed to transmit or transport “truth value” from the axioms to the conclusions. In the pre-modern understanding of Euclidean geometry, one “knew” that the axioms were “true” (perhaps because they were self-evident) and therefore one knew that the theorems were “true.” One can say that modern mathematics was born when non-Euclidean geometry was discovered—when the believed self-evidence of Euclid’s axioms evaporated. Nothing claims to be “self-evident” any more.

Nowadays it is occasionally claimed or hoped that the set-theoretic axioms of Zermelo and Fraenkel are a “basis” for all of standard mathematics (including the works, cited by Wiles, of Mazur, Tate, Kunz and so on). My personal favorite is
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number 5, the Replacement Schema. Here it is [Jech]:

\[
\forall u_1 \ldots \forall u_k \left( \forall x \exists! y \varphi (x, y, \hat{u}) \rightarrow \forall w \exists v \forall r (r \in v \equiv \exists s (s \in w \& \varphi(x, y, \hat{s}, r, \hat{u}))) \right)
\]

(“The image of a set under a definable map is itself a set.”)

Of course it isn’t faith in Zermelo-Fraenkel (or any alternative set-theoretic axioms) that causes mathematicians to accept Wiles’ proof. It is the status of established mathematics as a whole, and the status of the results he quotes, as accepted parts of established mathematics. This is characteristic of most contemporary mathematical proof—to start from established mathematics, which is taken as “given”, known, reliable. Any proposed axiomatic “foundations” cannot be as credible and reliable as the established mathematics they are supposed to support.

By starting from established mathematics, this “mathematicians’ proof” establishes new mathematics. Axiomatic proof, on the other hand, cannot establish anything, because axioms are not established, they are simply postulated. Cellucci argues convincingly that any attempt to establish them leads either to an infinite regress, or else to a more or less arbitrary, subjective decision to adopt them provisionally—by fiat. He writes,

“It is widely believed that the axiomatic method guarantees the truth of a mathematical assertion (Rota). This belief is unfounded because it depends on the assumption that proofs are deductive derivations of propositions from primitive premisses that are true in some sense of ‘true’. Now, as we will presently see, generally there is no rational way of knowing whether primitive premisses are true. Thus either primitive premisses are false, so the proof is invalid, or primitive premisses are true but there is no rational way of knowing that they are true, then we will be unable to see whether something is a proof, so we will be unable to distinguish proofs from non-proofs. In both cases, the claim that the axiomatic method guarantees the truth of a mathematical assertion is untenable.” (Cellucci 2008).

Mathematicians’ proof is semantic, not syntactic

In addition to the absence of axioms, and the total reliance on the body of established mathematics, there is another very important, striking feature of Wiles’ discourse to be noted by philosophers, and again, one that is found in nearly all mathematical publication. There is no syntactic argument, no syllogistics or modus ponens! In fact, it has often been remarked that mathematicians who are not logicians, know very little logic, and never mention logic in our work. That does not mean that we are illogical or irrational! It means (if I may use a technical term from logic) that our reasoning is “semantic” rather than “syntactic” (Rav). Wiles talks about all sorts of esoteric mathematical entities with complete confidence, he knows what he means, he knows the relevant “facts” or “properties.” His argument simply uses the facts and properties of mathematical entities. He can do that because the number theorists who will understand and evaluate his work have congruent or matching understandings of those objects or entities. Of course at times he does look into a book on his shelf, but certainly not at every line of his paper. He knows what a Galois representation is, he knows what a semistable elliptic curve is—not just because he has memorized a formal definition, he knows their properties and capabilities well enough to use them effectively in his proof. That is what I
mean when I say he has mental models of these objects—mental models which were acquired and developed in the course of his mathematical practice, which shaped them and molded them to be congruent or matching to the models possessed by other experts in his field of number theory.

For more explanation and justification of “mental models of mathematical entities”, see my [2011], p. 312 ff. These socially regulated mental models of mathematical objects are candidates for the “new semantics” sought in footnote 24 of Buldt et al. (“an important task for the philosophy of mathematics is, then, to work out the details of a new semantics for the platonese we still prefer to speak.”) [Warning! The term “mental model” is used here with a different meaning than in the works of cognitive scientist Philip N. Johnson-Laird.]

Mathematical reasoning, in the construction of a proof, is based on possession of shared mental models of mathematical entities, and on using one’s knowledge of these mental models in one’s possession. Indeed, something similar was already expressed by John Locke in 1690. “…if we consider, we shall find that [the knowledge we have of mathematical truths] is only of our own ideas. The mathematician considers the truth and properties belonging to a rectangle or circle only as they are an idea in his own mind.” (An Essay concerning human understanding). In 1739 David Hume enriched this viewpoint by contextualizing it socially: “There is no Algebraist nor Mathematician so expert in his science, as to place entire confidence in any truth immediately upon his discovery of it, or regard it as any thing, but a mere probability. Every time he runs over his proofs, his confidence encreases; but still more by the approbation of his friends; and is rais’d to its utmost perfection by the universal assent and applauses of the learned world.” (Treatise of Human Nature, p. 231)

Both in heuristics and in deductive proof, the mathematician works with his/her socially sanctioned mental model. Arguments or steps in a deductive proof are convincing when they are clearly “seen,” mentally perceived as unquestionable. A familiar example is the commutative law of multiplication in elementary arithmetic. To do “7 times 9” you imagine seven rows of soldiers, and in each row, nine soldiers. But “look at it from the side” instead of from the front, and you see nine rows, with seven soldiers in each row. So 7 times 9 is the same as 9 times 7. And this insight could just as well have been about 5 and 11 as about 7 and 9. So you clearly see, it doesn’t matter which two numbers you multiply, the result will always be the same if you reverse the order. This is a completely convincing, correct proof—not just a guess or a conjecture. Still, graduate students must also learn the tedious proof in Landau’s classical text, which follows from the Dedekind-Peano axioms by mathematical induction. The first proof is intuitive and explanatory, the second calculational and mechanical.

In some other branches of mathematics, the role of established mathematics is not so heavy. In combinatorics one can sometimes prove a counting formula for finite sets by simply presenting an ingenious way to combine those sets from smaller sets. Proof ideas are sometimes even presented without words—as diagrams that speak for themselves (Nelsen 1993 and 2000). That kind of proof is quite different from Wiles’ kind of proof. On the other hand, it is even more clearly not “axiomatic” or “syntactic.” One can call it “visual” or “diagrammatic”.

That raises the question, “What is a proof, really?” The simplest truthful answer was given long ago by the Ideal Mathematician: “A proof is just an argument.
that convinces a skeptical expert.” (Davis and Hersh, 1981). But what does it take to convince a skeptical expert? That is not so easy to say. At a minimum, the proof should explicitly avoid all the familiar, well-known fallacies, booby traps, and oversights that workers in the field are required to know about. A world-renowned probabilist said in my hearing, “You check the most delicate points in the paper, and if that part is OK, you figure the rest is probably correct.”

The Fregean notion of formal proof, using formal derivation in a formal language, is not relevant to proof as it is known in nearly all published math papers. However, an active group of computer programmers and mathematicians are formalizing mathematicians’ proofs of some interesting theorems (see Hales and Harrison). The Flyspeck Project of Thomas Hales proposes, over a period of years and with the assistance of a considerable group of specialists, to produce a complete formalization of Hales’ proof of Kepler’s conjecture on the packing of space by spheres. Such a project requires replacing any semantic steps in the proof by syntactic ones. If successfully completed, the credibility of Hales’ proof would be raised from, say, 99% to 99.99%. As Hales is the first to admit, and as any computer user well knows, 100% is not attainable.

A formal proof is either a formalized axiomatic proof or formalized from a “mathematicians’ proof.” If it is a formal axiomatic proof, the shortcomings of axiomatic proof, as a source of truth, apply just as well to a formalized version. If it is a formalization of a mathematicians’ proof, it is still based on some parts of established mathematics, but the semantic reasoning of the mathematician has been replaced by the syntactic reasoning of the logician. We would expect this formalized proof to be more reliable than the mathematician’s semantic proof. Such a formal proof is in fact a long, complicated computer program. Are long, complicated computer programs absolutely reliable, with certainty? No one in 2012 USA believes such a thing. Indeed, in his impressive, convincing article on formalizing his Kepler conjecture proof, Hales starts out by disarming criticism. He offers quite an extensive account of the unreliability of very long, complicated computer programs. They can be more reliable than human semantic proofs, they cannot be totally error-free.

Established mathematics is fallible

But then, mathematicians’ proof does not guarantee truth, whatever one might mean by “truth.” On pages 43–47 of my (1997) is a litany of famous and not-so-famous published mistakes. To this list can be added the erroneous prize-winning publication by Henri Poincaré on celestial mechanics, which forced Gosta Mittag-Leffler at great expense to retrieve and shred all the copies of the Acta Mathematica that had been mailed out, so that a completely new article by Poincaré could be published. (See Barrow-Green.)

Mistakes continue to be found and corrected, after publication. The gap or the error is repaired, corrected, fixed up, without bringing down the whole body of established mathematics. Hilbert’s Grundlagen der Geometrie is devoted to correcting, fixing up, the 2,000-year-old Elements of Euclid. Sometimes the word “correct” is used instead of “true”. After the Grundlagen had been published, some defects were noticed in it (and of course, fixed up). What counts as rigorous proof has evolved historically, and has often been controversial. Some wise philosopher (was it Wittgenstein?) has defined mathematics as “the subject where it is possible
to make mistakes.” One is often quite certain that something is “false” or incorrect. One is not often absolutely sure what is “true” or correct.

Questions of “truth” versus “fiction” are irrelevant to practice. Established mathematics is “assertible”—that is, available for use in mathematical proof. In fact, it is “warrantedly assertible.” (The unappealing expression “warranted assertibility” goes back to John Dewey’s pragmatism.) What is a “warrant”? A warrant is a justification to act on an assertion, a justification based on lived experience. In mathematics, two important warrants are deductive proof, and successful application—application both within mathematics itself, and in “the real world”. There are also weaker warrants—analogy and induction—which only grant plausibility, not “established” status. Analogy and induction serve to justify conjectures, to justify a search for deductive proof. Neither analogy nor induction nor deductive proof can establish the “truth” of a mathematical statement, for Truth in the sense of perfect certainty is unattainable.

Experience! That is really what is behind it all. Experience never guarantees truth. It can provide warranted assertibility. Deductive proof is the strongest warrant we know for the assertibility of some proposition. The strongest possible warrant, yes. Absolute certainty, no. Absolute certainty is what many yearn for in childhood, but learn to live without in adult life, including in mathematics.

Disregarding Vaughan Jones’ use of the word “true,” and contrary to the picture often presented in logic text books, deductive proof in mathematical research publication does not establish anything as “true.” Deductive proof connects some proposed “result” to the body of established mathematics. Once the proposed theorem is accepted or established, one is permitted to use it in other proofs. Jones did well to keep the word “truth” in scare quotes. Trying to explain what one means by “truth” in mathematics is a hopeless quicksand. Jones instead adduces the many irresistible warrants for the Fourier transform. One of those warrants is the essential “existence proof.” This proof makes the Fourier transform as reliable as basic arithmetic or algebra. If it was somehow found that the Zermelo-Fraenkel or Dedekind-Peano axioms are contradictory, the Fourier transform would not be abandoned, any more than the rest of established mathematics would be abandoned. They are established more firmly than the axioms that are presented as their “foundation.”

I must add that warrantedness comes in degrees. The strength of the warrant varies from virtually unassailable, for standard arithmetic, algebra, geometry, calculus, linear algebra and so on, down to very solid for much-studied well known results, both old and new, including the Wiles-Taylor FLT, and further down to reasonably reliable but needing some care, which would apply to more recent and less well-known publications. As more convincing arguments for a mathematical statement are discovered, it becomes more strongly warranted. A deductive proof makes it part of established mathematics, but that’s not always the end of the story. If the statement is widely studied, analyzed and used, if it becomes closely connected, both plausibly and rigorously to other established mathematics, then its warrant becomes stronger and stronger. It can even become, like the Fourier transform, so firmly embedded in established mathematics that it is inconceivable to exclude it.

Plausible (non-rigorous) reasoning is a warrant for making a conjecture, even for “establishing it” as a plausible, well-founded conjecture (like FLT, before Wiles.)
And then plausible reasoning (problem-solving) is likely to be essential in finding the rigorous proof. But plausible reasoning (analogy, induction) is not accepted as a substitute for rigorous proof. Rigorous proof is the method by which established mathematics becomes established. But of course, problem solving uses not only plausible reasoning, it also uses established mathematics, which was previously established by rigorous proof! The front of mathematics (rigorous proof) and the back (problem-solving by plausible reasoning) are not opposed or competing, they are intricately interconnected, they are inseparable. One attains rigorous proof by a problem-solving process. In the process of proving a conjecture, deficiencies, weaknesses or imprecisions are often revealed and corrected.

When people speak of mathematical certainty, they ordinarily mean very strongly warranted assertibility, not total, absolute certainty. Mathematics is human, and nothing human can be absolutely certain. Well-established core mathematics is warranted as strongly as anything we know. Warranted assertibility is all that can be attained, either in empirical science or in mathematics. Science and mathematics are different, because they use different kinds of warrants to confer assertibility.

**Published vs. private, rigorous vs. plausible**

Mathematical publication is not identical with mathematical knowledge. All that is published is not correct knowledge, mistakes sometimes do get published. And more importantly, all correct knowledge does not become published. Some things are not submitted for publication even though everybody knows they are true—in fact, BECAUSE everybody knows they are true. (Novelty is normally a prerequisite for publication in a research journal.) Some things are not submitted for publication even though everybody knows they are true, because they have never been proved. Much practical down-to-earth how-to-do-it knowledge in scientific computing falls under this heading (Hersh, 2011).

If you have made a publishable discovery, the process by which you found out your result probably won’t be included in your article. Discovering the interesting result was probably the outcome of a heuristic investigation, of the kind that Polya and Cellucci describe. But you will probably omit that story from your article, if only to save yourself extra trouble. If you choose to include it, you risk a rejection note from the editor: “We don’t have space for all the good papers we are receiving, even without irrelevant heuristics.” This policy is very unfortunate. The final polished deductive proof may conceal a key insight, that could have made the mathematical result more meaningful and accessible. Such insights may then be available only by personal contact between selected individuals. It’s important to advocate publication of the heuristic side of mathematical discovery, along with the deductive proof.

Deductive proof is intended to compel agreement. It serves to convince and to explain, sometimes one more than the other. It legitimates a result as “established”. By contrast, a plausible derivation does not establish the result, even if, as in the example of FLT, it has been verified in millions of individual cases. That’s where Polya’s respect for rigorous deduction comes in. But Polya is mistaken when he says that deductive proof renders a statement absolutely certain. Cellucci rightly denies this. Deductive proof is the standard for acceptance of one’s findings into the
TO ESTABLISH NEW MATHEMATICS

body of established mathematics. That is just saying their assertion is warranted. Not absolutely guaranteed to be free of error.

Sometimes a rigorous deductive proof is simply not available, at a time when a practical decision must be made. In the absence of deductive proof, there can still be practical certainty, which can justify decisions affecting billions of dollars, and even human life (Hersh 2011). Nevertheless, the distinction between a proved theorem and an unproved conjecture is the central, characteristic feature of mathematics, as practiced from Euclid to this day. To underestimate or ignore it is out of the question.

In textbook writing, the axiomatic method has advantages of economy and clarity—and the disadvantage of possibly obscuring goals and motivations. ZF set theory is a branch of mathematics, not a “foundation” for all the rest of mathematics. But questions of logical structure are intrinsically interesting, and there is some interest in the logical foundations of Wiles’ proof of FLT. As part of modern algebraic geometry, his work is connected to certain “very large” category-theoretic constructions called “Grothendieck universes” (see [McLarty]).

Established mathematics is not controversial

Since mathematicians’ proofs grow out of and are based on established mathematics, we may want to check, is established mathematics doubted or rejected by a significant number of dissenters? Is it in danger of overthrow? Established mathematics is a historically evolved construct. There have been several episodes that could be construed as major challenges to it. We will see that in every case, the challenge was made from within established mathematics, not against it. The episodes we will briefly consider are the constructivist-intuitionist critique of Kronecker, Brouwer and Bishop; the revival of infinitesimal methods by Abraham Robinson; the “theoretical mathematics” proposal of Quinn and Jaffe: and the introduction of computers into mathematical research in various ways by several contemporary mathematicians.

The sharpest criticism came from the constructivists Leopold Kronecker, Luitzen Brouwer and Errett Bishop. This controversy has a long history and a huge literature. Unlike Kronecker and Brouwer, Bishop actually aims to affirm the structure and content of established mathematics. His goal is to reconstruct it, as closely as possible, without the law of the excluded middle. His monumental effort was directed to either establish the contents of classical analysis constructively, or else to provide the closest possible constructive substitute. What is most remarkable is how well he succeeded in recreating or re-justifying it, with appropriate tweaks and precautions. This achievement of his is not a repudiation of established mathematics, but an attempt to strengthen it.

An utterly different re-conceptualizing of classical mathematics was made possible by Abraham Robinson’s introduction of non-standard analysis, legitimizing the once illegal infinitesimals. Non-standard analysis may appear to be a radical challenge to established mathematics, since it violates the well-established banishment of infinitesimal quantities. But Robinson by no means claimed that it could stand apart from and independent of established mathematics. On the contrary, he devoted the first chapters of his book to establishing it on standard foundations, both in logic and in set theory. (An example where foundational research made a major contribution to mainstream mathematics.) Of course, he did not claim
that his results were “true,” since no one claims that the basic statements of either logic or set theory are known with certainty to be true. All he did was the usual normal thing, to prove his new results by deriving them from established mathematics. The upshot was that new results and new methods were added to the body of established mathematics.

The proposal by Quinn and Jaffe was intended to provide some kind of license for mathematical publication without complete proof. They perceived a problem, that such results were appearing in the literature without being labeled as provisional or incomplete, but they didn’t want to prevent such publications, which they recognized as having a useful role to play. They merely wanted to label them as such. Their proposal stimulated a long discussion and controversy. It received a great deal of criticism and opposition, and very little support.

Finally, there has been some foundational discussion related to computers. We have already described the work of Thomas Hales and his collaborators on formalized proof. Gonthier reports that the entire proof of the four-color theorem has now been formalized or computerized. The original version, by Appel and Haken, combined sections of ordinary mathematicians’ proof with substantial sections of computer calculations. This original version of the proof prompted a much-discussed article by Thomas Tymoczko, who argued that by incorporating a computer calculation into their proof, the authors were changing the standard notion of mathematical proof. The following discussion dealt with two aspects of proof. Does it establish certainty? Does it provide understanding? As to the first, it is clear that computer proof is fallible, for several reasons. On the other hand, proof by human calculation is also fallible, and there is a powerful argument that for calculations that are very long, complex, and tedious, computers are more reliable than humans. On the second point, Paul Halmos was particularly vehement in rejecting the computer proof of the four-color theorem, because “we learn nothing from it.” To this, there are two answers. First of all, it may turn out that we can’t find any other proof, so we can either accept the theorem as proved, or reject it, on the basis of what we have, namely the computer proof. Of course, the computer proof must be checked by the best standards of computer verification. The mathematical community already has clearly accepted the four-color theorem as proved. In addition, it seems that some hand-made proofs also don’t provide much insight.

Doron Zeilberger has proposed in all seriousness that proofs be graded according to the degree of certainty that is claimed. This suggestion has not received any support that I am aware of. Finally, it is important to notice the new field of “experimental mathematics” as advocated by Jon Borwein and his collaborators, in several books and a journal of that name. Experimental mathematics amounts to systematic and persistent use of computers to make mathematical discoveries. It is avowed and understood that such discoveries are not accepted as established until a traditional deductive proof is given. The experimental mathematics of Borwein and Bailey is an elaboration or modernization of the heuristics of Polya or the analytic method of Cellucci. It simply brings the speed and memory of the computer in a sophisticated way to make heuristics much more powerful. Computers will be used more and more, both in heuristic, or problem solving, and in actual proving, using formalized reasoning. These important additions to the mathematician’s repertoire will only strengthen the two sides of mathematical work—heuristics and rigor.
We see that all of these critical and radical proposals completely accept the legitimacy of established mathematics! One may expect it to absorb and incorporate within itself any future challenges.

In practical decision making, both in empirical science and in daily life, all that we mean by “true” is “well justified” or “firmly established.” As John Dewey famously phrased it, “warrantedly assertible.” The credibility of established mathematics is based on experience—many people’s experience—and on its connection and application to practical life, including commerce, science and technology. The purpose of mathematical proof is to endow a new result with that strong credibility. This is just the mathematics version of John Dewey’s view of logic and knowledge.

Acknowledgments

Thanks to Carlo Cellucci, Martin Davis, David Edwards, Sol Feferman and Robert Thomas for helpful suggestions.

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How Mathematicians Convince Each Other or
“The Kingdom of Math is Within You”

Introduction and abstract

There is a problem about mathematical proof—actual mathematical proof, done by actual mathematicians. Typical samples of mathematicians’ proof hardly resemble formal proof.

Formal proof has been well described and analyzed by logicians. Mathematicians’ proofs are different. We rarely cite the rules of logic. (Many of us don’t know much about logic.) Our “proofs” don’t usually start from axioms. If they include rule-governed calculations, these calculations are embedded in noncalculative reasoning—a combination of verbal argument and citation of literature. Nevertheless, these “informal” proofs work—they compel agreement. That is what mathematicians mean by a proof—an argument that compels agreement.

Mathematicians and logicians both end by claiming that a certain statement has been “proved.” But the steps that reach this claim are very different.

Recognizing this discrepancy, Brendan Larvor has asked the following question: “What qualifies the usual informal mathematical arguments as ‘proof’?”

Before answering Larvor’s question, I begin by posing and answering a different question. Not “what qualifies them?”, but rather, “What makes informal proof work?” How does it compel agreement?

Mathematicians’ arguments are glaringly incomplete from the point of view of formal logic, yet they actually do compel agreement. This “working”, this compelling agreement, is stronger in mathematics than in any other human endeavor. Indeed, it is the defining quality of mathematics. “Mathematics is the science that draws necessary conclusions” (Benjamin Peirce).

How does it “work”? How does it compel assent, inconclusive as it is by the standards of formal logic?

Mathematician’s proof works for the same reason that ordinary empirical science works. Different people observing the stars see the same thing, because they are looking at the same thing. Different mathematicians observing a sporadic group or a stochastic process “see” the same thing, because they also are “looking” at the same thing. That is how, starting from established mathematics, we establish a new result, which then becomes part of established mathematics.

This claim is argued below, supported by a classic description of Isaac Newton’s thinking, and by half a dozen famous mathematicians, describing their experience of this internal reality.
A quote from Hardy

An article by G. H. Hardy exasperated his friend Ludwig Wittgenstein (according to Ray Monk, Wittgenstein’s biographer). Hardy’s article discussed Russell’s logicism, Hilbert’s formalism, and Brouwer’s intuitionism. Into this dry philosophizing, Hardy threw a strange passage, which some philosophers have condemned as sheer nonsense. That is a pity. Hardy was trying hard to explain what really goes on in mathematicians’ proof.

“I have myself always thought of a mathematician as in the first instance an observer, a man who gazes at a distant range of mountains and notes down his observations. His object is simply to distinguish clearly and notify to others as many different peaks as he can. There are some peaks which he can distinguish easily, while others are less clear. He sees A sharply, while of B he can obtain only transitory glimpses. At last he makes out a ridge which leads from A, and following it to its end he discovers that it culminates in B. B is now fixed in his vision, and from this point he can proceed to further discoveries. In other cases perhaps he can distinguish a ridge which vanishes in the distance, and conjectures that it leads to a peak in the clouds or below the horizon. But when he sees a peak he believes that it is there simply because he sees it. If he wishes someone else to see it, he points to it, either directly or through the chain of summits which led him to recognize it himself. When his pupil also sees it, the research, the argument, the proof is finished. The analogy is a rough one, but I am sure that it is not altogether misleading. If we were to push it to its extreme we should be led to a rather paradoxical conclusion; that there is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but point; that proofs are what Littlewood and I call gas, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate the imagination of pupils. This is plainly not the whole truth, but there is a good deal in it. The image gives us a genuine approximation to the processes of mathematical pedagogy on the one hand and of mathematical discovery on the other; it is only the very unsophisticated outsider who imagines that mathematicians make discoveries by turning the handle of some miraculous machine.”

Of course Hardy knew there was no literal mountain peak that he or his student was seeing! What did he mean? He meant that he perceived some fact or phenomenon about his internal mathematical world, and he could get another mathematician to perceive it, by guiding him to it through that mathematician’s own mental world.

He and his pupil each had his internal mathematical universe, where he could make observations. And these observations, by introspection, seem definite and indubitable, like our observations of the exterior world by our eyes and ears.

If an ornithologist discovers some unexpected entity in the body of a certain species of swan, his claim is checked by other ornithologists, who have access to samples of that species of swan. They look where he tells them to look, and either they do or don’t see what he says is there. Something precisely analogous takes place in mathematicians proof! When Hardy makes a discovery, he explains how other mathematicians can verify his claim, by following a certain sequence of steps, to arrive at “seeing it.” And those directions are “the proof”!

Mathematical concepts are real entities, not fictions. Platonism mistakenly locates these entities “out there,” in an external unspecified realm of non-human, non-physical reality. But they are right here, in our own individual minds, shared
also with many other individual minds. Their reality is both psychological and social, it is mental-cultural.

Mathematical concepts differ from non-mathematical concepts in having definite properties or attributes which obtain near unanimous agreement. Some of these properties are immediate, or, as we say, “by definition”. Others are not immediate, and require demonstration, which we call “proof”.

By “proof” mathematicians mean a procedure which suffices to ESTABLISH some “result” by near-unanimous consent. There are various kinds of proofs. There are visual proofs, where displaying a diagram or a graph is sufficient. There are directly conjunctive proofs, where the result is “seen” immediately, by combining two or more results from ESTABLISHED MATHEMATICS. And there are proofs of the kind Hardy describes. The mathematician leads the learner to observe and manipulate his/her own mental models, enabling her/him to “see”—to apprehend directly by observing his/her own mental model—the claimed attribute or property of the mental model in question. And therefore the learner apprehends the concept, the mathematical object, of which that model is a representative. We proceed to support these claims, by recent quotes from leading mathematicians, and by a look into the mind of Sir Isaac Newton.

(Note: This account of the way “mental models” function can be compared to Jody Azzouni’s notion of “inference packages”.)

What some mathematicians say they are doing

Start with brief but pungent remarks.

“In mathematics you have concrete objects before you and you interact with them, talk with them. And sometimes they answer you.” (Bettina Heintz, quoted by Michael Harris, in T. Gowers’ Princeton Companion to Mathematics, p. 974.)

“If you can steal ideas, then they are real. Every mathematician knows that ideas can be and often are stolen.” (Harris, p. 971)

“When mathematicians refer to “intuition” in the sense I have in mind, it is crucially public...it can be transmitted from teacher to student, or through a successful lecture, or developed collectively by running a seminar and writing a book on the proceedings.” (Harris, p. 974)

“When Alain Connes and I speak about the reality of mathematical objects, what we mean is that one or another mathematical object is as real to us as the singular nerve ganglion of some tiny creature is to the electrophysiologist who studies it,” (Marcel Paul Schutzenberger). “Exactly.” (Connes) (Triangle of Thought, p. 41)

“I confess I have great difficulty distinguishing my activity from that of an entomologist.” (Schutzenberger) “I agree completely.” (Connes) (op. cit., p. 37)

“Five-dimensional shapes are hard to visualize—but it doesn’t mean you can’t think about them. Thinking is really the same as seeing.” (Bill Thurston’s obituary.)

The reader who is not a mathematician may never have heard of Bill Thurston or Alain Connes. Let’s turn to Isaac Newton, described in the famous talk “Newton the Man” by Lord Keynes:

“Until the second phase of his life, he was a wrapt, consecrated solitary, pursuing his studies by intense introspection with a mental endurance perhaps never equaled... I believe that the clue to his mind is to be found in his unusual powers of continuous concentrated introspection... his peculiar gift, especially amongst his
contemporaries, was the power of holding continuously in his mind a purely mental
problem until he had seen straight through it. I fancy his preeminence is due to his
muscles of intuition being the strongest and most enduring with which a man has
ever been gifted. Anyone who has ever attempted pure scientific or philosophical
thought knows how one can hold a problem momentarily in one’s mind and apply
all one’s powers of concentration to piercing through it, and how it will dissolve and
escape and you find that what you are surveying is a blank. I believe that Newton
could hold a problem in his mind for hours and days and weeks until it surrendered
to him its secret. Then being a supreme mathematical technician he could dress
it up, how you will, for purposes of exposition, but it was his intuition which was
preeminently extraordinary—‘so happy in his conjectures’, said De Morgan, ‘as to
seem to know more than he could possibly have any means of proving’. The proofs,
for what they are worth, were, as I have said, dressed up afterward—they were not
the instrument of discovery.”

Now turn to Andrew Wiles, famous for proving Fermat’s Last Theorem, speak-
ing on a Nova science TV broadcast:

“Perhaps I can best describe my experience of doing mathematics in terms of a
journey through a dark unexplored mansion. You enter the first room of the man-
sion and it’s completely dark. You stumble around bumping into the furniture, but
gradually you learn where each piece of furniture is. Finally, after six months or so,
you find the light switch, you turn it on, and suddenly it’s all illuminated. You can
see exactly where you were. Then you move into the next room and spend another
six months in the dark. So each of these breakthroughs, while sometimes they’re
momentary, sometimes over a period of a day or two, they are the culmination
of—and couldn’t exist without—the many months of stumbling around in the dark
that precede them.” [Andrew Wiles, http://www.pbs.org/wgbh/nova/phy...]

Now I quote more extensively from David Ruelle and Alain Connes. To make
their reports more easily digestible to academic scholarship, I provide interlined
comments in italics to their texts—Ruelle’s and Connes’ testimonies are our textes
d’explication. Ruelle is a member of the Institut des Hautes Etudes Scientifiques,
a colleague of Alexandre Grothendieck, Jean Dieudonné, and Rene Thom. He has
made major contributions to dynamical systems and chaos theory.

“Mathematical research requires mental agility and the patience to pace around
an infinite and dreary logical labyrinth to examine and contemplate a connected
series of mathematical concepts and images until you find something that has not
been understood before you: a new point of view, a new proof, a new theorem.” p.
79 [my italics]

“Doing mathematics” is thus working on the construction of some mathematical
object and resembles other creative enterprises.” p. 88

“The set of tools available to a mathematician may be compared to the system
of highways available to a traveler; both provide the means to go efficiently from A
to B. But... The highway system reflects the geography of a country, which we also
know by other methods, so that building another road will not significantly change
our knowledge of the geography. The panoply of technical tools of mathematics
reflects the inside structure of mathematics and is basically all we know about this
inside structure, so that building a new theory may change the way we understand
the structural relations of different parts of mathematics.” p. 94
[In studying mathematical concepts we make use of appropriate tools, devices, and methods, which themselves are also mathematical concepts, and are studied as such. Improving or revising these tools changes the possibilities and relations of our network of mathematical concepts.]

“Putting together a sequence of mathematical ideas is like taking a walk in infinite dimensions, getting from one idea to the next. And the fact that the ideas have to fit together means that each stage in your walk presents you with a new variety of possibilities, among which you have to choose. You are in a labyrinth, an infinite-dimensional labyrinth. The ideas are human, and they belong to a human mathematical culture, but they are also very much constrained by the logical structure of the subject. The infinite labyrinth of mathematics has thus the dual character of human construction and logical necessity. And this endows the labyrinth with a strange beauty...only through long study do we come to taste fully the subtle and powerful aesthetic appeal of mathematical theories.” p. 96

[The “labyrinth” is simply a suggestive name for the complex multiply-connected network of concepts and methods available for consideration and contemplation by the mathematician.]

“Constructing a mathematical theory is the essence of mathematical work... Constructing a mathematical theory is thus guessing a web of ideas, and then progressively strengthening and modifying the web until it is logically unassailable. Before that point you don’t have a theory. In fact, it is usually not assured at the beginning that you will be able to complete your construction as originally planned (otherwise, the theory would be uninteresting.). Clearly, during your construction work, you should concentrate your effort on the more uncertain links in your arguments. This is where your theory is most likely to fail, and you save time by knowing this early on. The easy and safe steps are left for later and are often handled in the final write-up by a dismissive sentence “it is obvious that...” “it is well-known that...” (p. 114)

“Doing mathematics is often an individual and solitary enterprise. But mathematics as a whole is a collective achievement.” [The mathematician thinks about a problem or searches for a new idea. This means examining the concepts he possesses, using the tools he has available from established mathematics. These concepts and tools are shared with the rest of the mathematical community. There may be some that he himself has invented and which are presently known only to himself.]

“A mathematician lives in an intellectual landscape of definitions, methods, and results, and has greater or lesser knowledge of this landscape. With this knowledge, new mathematics is produced, and this invention changes more or less significantly the existing landscape of mathematics.” (p. 108)

[The term “landscape” is metaphorical, and can be replaced by “mathematical world.”]

“A mathematician who has finally understood a question may say that it was after all very simple. But this is usually an erroneous feeling. In fact, when our mathematician starts writing things down, their complexity unfolds and may end up looking formidable. A simple mathematical argument, like a simple English sentence, often makes sense only against a huge contextual background.” (p. 87, Ruelle.)
And finally, quotes from an article by Alain Connes on page 1006 of the monumental compendium created by Timothy Gowers, *The Princeton Companion to Mathematics*. Connes is outstanding even among Fields Medal winners. His non-commutative geometry is a vast continuation of John von Neumann’s work on operator theory.

“The scientific life of mathematicians can be pictured as an exploration of the geography of the “mathematical reality” which they unveil gradually in their own private mental home.”

The trained and educated mathematician possesses a huge collection of mathematical concepts of all sorts from established mathematics (definitions, theorems, diagrams and graphs, problems both solved and unsolved) and modes of reasoning about them (standard arguments and calculations, heuristics including analogy and induction). Purposeful thinking about a specific problem, or openly seeking something interesting and worth pursuing, develops a familiarity with the connections and relationships of these concepts, which is analogous to the geography of one’s neighborhood in some town or city.

“Once a mathematician truly gets to know in an original and ‘personal’ manner, some small part of the mathematical world, however esoteric it may look at first, the journey can properly start. It is of course vital not to break the ‘fil d’Ariadne’ (‘Ariadne’s thread’); that way one can constantly keep a fresh eye on whatever one encounters along the way, but one can also go back to the source if one ever begins to feel lost.”

In this mathematical exploration, the mathematician may be led into “strange territory” (a domain of mathematics with which he is not yet thoroughly familiar). Connes advises him in that case, to look back at the starting point where he was thoroughly “at home” (i.e., completely familiar and in mastery of the concepts and tools).

“It is also vital to keep moving. Otherwise, one risks confining oneself to a relatively small area of extreme technical specialization, thereby limiting one’s perception of the mathematical world and of its huge, even bewildering, diversity.”

This is advice which some mathematicians follow and some do not.

“The fundamental point in this respect is that even though many mathematicians have spent their lives exploring different parts of that world, with different perspectives, they all agree on its contours and interconnections.”

The internal images, representations or models possessed by different mathematicians, while private and idiosyncratic, are “congruent”; that is, they give the same answers to test questions about “interconnections.” They match each other in the ways necessary to permit mathematicians to talk to each other about these concepts with understanding and agreement.

“Whatever the origin of one’s journey, one day, if one walks far enough, one is bound to stumble on a well known town: for instance, elliptic functions, modular forms, or zeta functions.”

This is a testimony to Connes’ own experience, or a statement of faith in mathematical thinking in general. If one hasn’t stumbled on a well-known town yet, then one tells oneself that one simply hasn’t walked far enough yet.

It would be easy to make a translation between his account (traveling through a mathematical landscape) and Wiles’ account (exploring a darkened room).
Connes’ image of a landscape is similar to Alexandre Grothendieck’s description, in Récoltes et Semailles, of his feelings on leaving functional analysis for algebraic geometry: “I still remember this strong impression (completely subjective of course) as if I was leaving dry and gloomy steppes and finding myself suddenly in a sort of “promised land” of luxuriant richness, which spread out to infinity wherever one might wish to put out one’s hand to gather from it or delve about in it.”

What does all this add up to, Grothendieck, Thurston, Newton, Wiles, Connes, Schützenberger, Ruelle, Harris and Heintz?

With different metaphors and each in their own way, they tell us that mathematical work feels like direct contact, bumping into actual objects, which the mathematician manipulates, “fools around with,” plays with, turns over, tries to connect to other mental objects, until finally he/she “sees” what is going on.

To Andrew Wiles, it is like stumbling around an unfamiliar room and banging against the furniture, until at last a light goes on and he sees clearly where he is. (Then he is ready to enter the next dark room.) David Ruelle and Alain Connes talk of mathematics as a landscape, or as a labyrinth through which one travels and searches.

Such testimonies have not usually been taken seriously by philosophers of mathematics. But they are not lunatic ravings, nor are they senseless inexplicable poetry. They are important evidence, which needs to be interpreted.

These authors know what they are talking about. Their accounts are not fanciful fiction. They are autobiographical reports of actual experience. Anyone interested in how mathematics works would be mistaken to dismiss or ignore them.

Of course there is no “furniture” or “labyrinth” in any literal sense. What there is, is actual mathematical thinking! Wiles and Ruelle and Connes are encountering mental mathematical entities. These mental mathematical entities are experienced as actual objects, more or less clearly or obscurely perceived, that have their own properties, which the mathematician may struggle for a long time to ascertain.

This is psychology, or “psychologism,” long mocked and outlawed in the philosophy of mathematics. And it is “introspection,” long ago outlawed as unscientific in the world of “scientific psychology”. Nevertheless, there can be no substitute for such self-reports of actual practitioners. Yes, do also analyze their published research articles, to see what is present there. But what is not present there, that is the very essence of the matter.

There are two mistaken ways to respond to this internal reality. A naïve materialist minimizes or denies it. (“Thoughts and concepts are ephemeral, immaterial. Rocks or stars or neurons, what is detectable by scientific instruments, that is really real.”) On the other hand, a mathematical Platonist mythologizes it, spiritualizes it, makes it superhuman and transcendental (It is “out there.”)

The Materialist versus the Platonist: Changeux and Connes

The materialist met the Platonist in Conversations on Mind, Matter, and Mathematics between the neuroscientist Jean-Pierre Changeux and the mathematician Alain Connes. (A critique of the Conversations has been written by Jean Pettitot.)

The materialist viewpoint is clearly expressed by Changeux. (See also Behrens.) Changeux says, “Mathematical objects exist materially in your brain. You examine them inwardly by a conscious process in the physiological sense of the term. Because these objects have a material reality, it’s possible to study their properties. In
the head of a mathematician, mathematical objects are material objects—mental objects if you like—with properties that are analyzable by a reflexive process. It ought in principle to be possible to observe them from the outside looking in, using various methods of brain imaging.” (p. 12)

Can we, even in principle, observe, for example, the commutative law of multiplication, inside my brain? By electronic or chemical methods of observation, we observe electrochemical phenomena. An electrochemical phenomenon is not a law of arithmetic. The commutative law of multiplication simply is not of the same “category” as any physically observable electro-chemical process. Yet our mathematical thinking, like any kind of thinking, conscious or unconscious, is based in or rooted in our bodies (mainly our nervous systems.) Our thoughts, feelings, and inner consciousness, must somehow “be there”, in our nervous systems and our brains (although in ways we may never in detail understand).

So our mathematical concepts in some manner or representation are inseparable from brain components or activities—electro-chemical entities. To the mathematicians’ mental struggle to solve a problem, taking place “in his head”, there must be a corresponding brain process, an electro-chemical process, on the bio-physical level. (Nieder’s article, “The Neural Code for Number”, is an impressive example of work to reveal this connection.)

But impressive as is research such as Nieder’s, it is not directly relevant to the mathematician or the philosopher. When we are doing mathematics—creating, elaborating, criticizing, and connecting mathematical objects—we can do so only by dealing with them as having content, meaning something. Thinking of mathematical concepts as merely properties of neurons would make it impossible to do mathematics.

Alain Connes, Changeux’s opponent, is a forthright Platonist. “There exists, independently of the human mind, a raw and immutable mathematical reality, and on the other hand, we have access to it only by means of our brain at the price of a rare mixture of concentration and desire.” (p. 26) “My position cannot change...it’s humility finally that forces me to admit that the mathematical world exists independently of the manner in which we apprehend it.” (p. 38)

Connes certainly knows his own mental mathematical experience. It would be absurd to doubt his reports of his inner mental process. Many another mathematician has said much the same thing: “Our knowledge of mathematical reality is certain, it is unquestionable!”

But we have to distinguish two aspects of such claims. There are reports of actual experience when doing mathematical work, and there are interpretations of actual experience. What about the claim that the truths of arithmetic are eternal and unchanging? Such a claim cannot, on the face of it, be based on direct experience. It is a statement of belief, not a report of direct experience. What of the claim that these truths are independent of the knowledge or the existence of human beings? Such a claim is also, on the face of it, not something that one could know from direct experience.

Connes’ subjective experience of doing mathematics, which is experienced in common by many mathematicians, and described by several of us, is not a delusion, nor a superstition, nor a myth, any more than the inner, personal, subjective experience of people in general.
He experiences mathematical concepts \textit{directly in working with them} as a researcher. He cannot be swayed from his conviction that they are real, and independent of his own awareness of them. That opinion of his is perfectly correct. These concepts are embedded in the vast intertwined structure of living mathematics, which is a social structure shared by the mathematical community as a whole. It exists independently of the awareness of any individual mathematician.

But for Connes, it is necessary to describe this objectivity in the traditional language of Platonism. “Mathematics is absolute, universal, and therefore independent of any cultural influence.” (p. 50)

It is “out there,” he says, neither in space nor in time, eternal, unchanging, immaterial, extra-human. This claim is an interpretation of his experience, not an experience or an observation. It is neither verifiable nor refutable. Anyone is free to believe it or not. And it is incompatible with ordinary scientific discourse, which long ago rejected the dualism of separate incomparable “Substances” called “Spirit” and “Matter”.

When he insists that there are facts of the matter, which are what they are, regardless of our desires, Connes is correct, and entirely justified. These facts are properties or aspects of our shared mental models. Connes’ attraction to Platonism, which is shared by many other mathematicians, expresses our conviction that we are dealing with something real, something that goes beyond our own individual awareness. But there is no necessity to place it beyond time, space, and human consciousness.

Connes and other mathematicians observe our inner lives as we work mathematically, and then reflect and recall about such work. In order to talk about it, we resort to metaphors—a “darkened room”, a “labyrinth”, a “landscape”. Of course there is no darkened room inside Andrew Wiles’ head, no labyrinth inside David Ruelle’s head! However, our inner world of mathematical knowledge and thought \textit{is} an objective reality. We explore it and try to understand it. We navigate it, not arbitrarily, or as we might hope or prefer, but according to its own possibilities. When we invent or create new mathematical entities, they too become parts of this inner world of objective reality, with their own properties, some that are clear and apparent, and others that are hidden and inscrutable.

So who is right, Changeux or Connes? Are mathematical concepts the thoughts of Alain Connes, or are they electro-chemical traces in Connes’ brain? Both Connes and Changeux are right. Changeux is right about the neurons and the brain, and Connes is right about what he actually does when he does mathematics.

They are not contradictory! They are complementary. Changeux and Connes are talking past each other. The facts that Connes is emphasizing are not inconsistent with the facts emphasized by Changeux. They report two different ways of observing the same thing, from two radically different points of view.

The disagreement is in Changeux’s implied “nothing but.” Mathematical concepts are properties of the brain, but they are not \textit{nothing but} properties of the brain. Connes parries Changeux’s reductionism, saying “It might also be an illusion—like believing that if we only knew more about the chemistry of ink and paper we would have a better understanding of the works of Shakespeare.” (page 14) The seeming contradiction is created by the reductionist impulse—the impulse to say “either-or”, rather than “both-and.”
To see how two such different pictures can fail to be contradictory, it is enlightening to compare the Changeux-Connes conversation to an ordinary conversation between a physician and a patient.

The patient reports “light-headedness,” “fatigue,” “confusion.”

The physician considers: “Auto-immune reaction? Infection? Head injury?”

No one would ask, “Who is right, the physician or the patient?” Everyone understands that both aspects are real, and complementary! Both the patient’s inner subjective report, and the physician’s external, physical explanation.

How does a physician know what a patient means by “light-headed” or “confused” or “fatigued”? He/she knows, because he/she also can experience such sensations, because he/she also possess a human body and brain. The patient is in the physician’s “lifeworld”, to steal a phrase from Husserl or Habermas. A common biological and cultural lifeworld.

And how do mathematicians understand each other, about their inner thoughts, guesses and insights? Because they share a “lifeworld.” They are all human beings, first of all. And on that basis, their common training and education brings them to their common understanding.

Here is another metaphor. Think of a huge cathedral, like Notre Dame de Paris. One observer is locked outside of it. He sees turrets and gargoyles, but he has no access to the interior. A second observer is locked inside. She sees glorious statuary and gold ornaments, but she has no access to the exterior. They communicate “on line”. But they cannot understand each other. Their two descriptions seem to be utterly contradictory. But both are correct! What’s lacking is the insight, that their two descriptions are complementary.

Consider two complementary views of a spherical surface like an eggshell. From the inside, it is concave. From the outside, it is convex. Which is correct? Both, of course!

Mathematical entities (objects, concepts, theorems, algorithms, problems, conjectures, analogies, and so on) are things that we think about. So they are available for us to think about. They are mental entities. And also, at the same time, in some form or realization, they are physically present in our brains. So they have definite properties of two kinds—both mental, as shared mathematical activities, and physical, in our brains.

The puzzle about mathematical proof is dissolved if we realize that mathematical proof is about a kind of mental object accessible to mental inspection. By thinking about numbers (or functions or operators), we can sometimes come to understand, to see, that a certain statement about numbers is correct, is “actually the case”. Then we tell others how they too can come to see that fact about numbers.

We aren’t able to show this by the methods of neuroscience. But it’s not surprising that mental models—which are also brain activities—can be directly observed mentally—which is to say, by the brain itself. That “observing,” “seeing”, is what mathematicians mean by a “proof”. That’s what Hardy, Ruelle, Connes and Wiles are trying to tell us.

I use the expression “mental model” for the internal entity in the mind of anyone, including a mathematician, any entity, object, or process that one may think about, concentrate on, study by inner thought. A mathematical concept is a collection of mental models that are “mutually congruent,” fit together. The concept “triangle”, for example, is a shared, public, inter-subjective entity. Each of us who
“understands” the word “triangle” has his/her own internal entity, available for contemplation or mental manipulation. That inner, private mental entity corresponding to the shared concept is what I mean by our “mental model.” Under the pressure of a strong desire or need to solve a specific problem, we assemble a mental model which the mind-brain can manipulate or analyze.

(The term “model” seems to be the most appropriate, even though it risks misunderstanding. The notion of “mathematical modeling” in applied mathematics is not what I mean. The psychologist P. N. Johnson-Laird has used the term in a specialized way, not what I mean.)

The mathematicians’ inner life, even though reported by metaphor, is a fact, not a fiction. The inner life of the mathematician is just as real as yours, dear reader! The philosophy of mathematical practice cannot answer some of its leading questions without recognizing and dealing with this reality—the subjectivity or inner life of the mathematician.

While mathematical thinking, like other thinking, is in an important sense private, even perhaps incommunicable, in another sense it is public and open. Mathematicians share a culture, a system of knowledge, a tradition, a set of methodologies, and a special kind of subject matter that permits them a unique and very precious consensus, even unanimity. In other kinds of theories, in “the humanities”, theorists may freely continue to disagree for centuries. But in mathematics, with rare and brief exceptions, once a new mathematical result is proved—once a “proof” has been accepted by the appropriate referees, editors, and experts—then it becomes part of established mathematics. Then every mathematician is free to use it as a tool or a building block in creating or discovering new mathematics.

This testimony is subjective, or introspective, but that does not mean it is private or impenetrable. On the contrary, the essential characteristic of mathematical thinking is that it is shared by the mathematical community. Our training, our education, and our work experience make our personal mental models congruent, fitting in with each other. The social life of mathematics makes these personal, internal mathematical objects “mutually congruent,” or matching.

It is helpful to borrow some terminology from mathematics itself. “Equivalence class” is a fundamental elementary mathematical notion. It denotes a set of things that are equivalent to each other in some definite sense. The members of an equivalence class are individuals who share certain properties with other members of the class. Such an equivalence class itself becomes a well-defined entity, with its own definite properties.

For instance, the triangles of all shapes and sizes, that mathematicians talk about, are each members of the equivalence class, or concept, “triangle.”

The numeral “3” is an equivalence class. All the various symbols for “3” that are actually written on paper or blackboards are “representatives” of this equivalence class. They are not identical, but they all serve to represent the “class”, which is the numeral “3” (and which, on this page that you are now looking at, is represented by whatever representatives my computer program generates).

In our present context, dealing with mathematical cognition—thinking, proving, and problem-solving—we talk about the various individual mathematicians’ mental models of some mathematical concept. All these individual mental models of the concept are equivalent—that is, mutually interpretable, communicable, with agreement and understanding. Together, they form a collection of mental models.
which are equivalent to each other in that sense, of being mutually interpretable, communicable, with agreement and understanding. We can think of them as a kind of equivalence class, like a mathematical equivalence class.

In effect, this equivalence class itself is the "mathematical concept."

My interior mathematical objects are personal and subjective, but the "equivalence class" of mutually congruent models is inter-subjective, it is cultural.

My personal representatives of the concepts are internally available for manipulation and experimentation. In that sense, my knowledge of mathematical entities comes from direct perception.

The "proof" is a procedure, an argument, a series of claims, that every qualified expert understands and accepts. It is the possibility and necessity of proof that defines mathematics. (To the extent that physics or linguistics or genetics have proofs, we speak of *mathematical* physics, linguistics or genetics.)

But who are these "qualified experts"? Anyone who understands the concepts involved in the proof! That is to say, anyone who has acquired or constructed the necessary mental models. In some cases (like Wiles' proof of Fermat's Last Theorem) only a small group of leading specialists are thus qualified. In other cases, which use only high-school level mathematics, you, the reader, are qualified. (This paper concludes with my example of a mathematicians' proof accessible to most people.)

The mental-social reality of mathematics is different from other socially shared beliefs, in crucial ways. It has persisted for centuries, and grown to a vast extent. Established mathematics is universally accepted and adopted, in every part of the world. This impressive fact calls for an explanation. It is plausible that mathematical thinking is a general aspect or property of the human brain—not an innate ability everyone is born with, but a capacity for development that every sane adult possesses, to one degree or another—some only slightly, others very highly.

Bodily and visual sensing of distance and direction, from which *geometry* arises, are shared by us with every mammal that is ever either a predator or a prey. And the discovery of "subitizing" in weeks-old infants seems to establish an inborn basis for *counting*. (Not such a novel idea, really. *Old Kant was right* when he said that mathematical ability is based on something innate in our mind-brain. His mistake, to fetishize Euclidean geometry, is only a detail. Even Plato was right, when rightly interpreted, in claiming that geometric reasoning is remembered from before we were born. He put it up in Heaven. We put it rather in evolution, in genetics. (Yehuda Rav has written incisively about the evolutionary basis of mathematical thinking.)

There remains a gigantic scientific challenge: to refute or to verify these claims, by detailed, specific knowledge of the brain and nervous system. Some beginnings of such knowledge have accumulated. (See Dehaene-Brannon.) We might wish for specific, refined knowledge, such as, "Exactly what electro-chemical circuits in the brain of Isaac Newton or Terry Tao made their discoveries possible?" Such knowledge may forever remain out of reach.

The mathematician has direct access to her own representatives of the concepts that she shares with the mathematical community, *established mathematics*. I call these representatives her "mental models," to bring out the compatibility of this statement with contemporary neuroscience. The neural description of thought and the psychological or introspective description of thought are complementary
descriptions of the same thing. By reminding you that these mathematical concepts have a material basis, I hope to convince you that they are real, objectively existing entities, even though they are accessible to us only in our thoughts.

**What, then, is a mathematicians’ proof?**

Mathematicians’ proofs compel agreement and acceptance by leading the reader along a path in his/her mental mathematical universe, to where he/she “sees” the claimed result—or more conventionally stated, “sees” that it is “true” or “correct.”

And when must such a proposed proof be rejected? Since the proof-claims that are accepted and the ones that are rejected both are deficient as logic, what makes a proof-claim unacceptable? Simply, that it fails. Meaning simply that the reader is not led to “see” the proposed result.

But does “proving” a mathematical claim, in the sense of mathematicians’ proof, guarantee that it is “true”? There is no need to step into the notorious quagmire of trying to define “mathematical truth”. The history of mathematics shows that mathematics undergoes a continuing process of self-correction and clarification. Is Euclid’s parallel postulate “true”? In one sense yes, in another sense no. Euclidean geometry is a well-established theory, a collection of well-established theorems, that mathematicians are able to use, to create and establish new mathematics.

“Anecdotal description is all very well,” someone might object, “but what about Right and Wrong? Don’t we need logic, to tell us what we ought to do?”

The mathematician using established mathematics is comparable to a normal human being using vision and hearing. While our eyes and ears may deceive us, we must trust our eyes and ears (using due precautions, of course). Refusing to use them would be insane. By using our eyes and ears, we stay alive; if I refused to use them, I would die.

If I want to do mathematics, I have to acquire and use the concepts in established mathematics, the concepts shared by the mathematical community. That is what doing mathematics means. Acquiring those concepts means acquiring my own mental representatives of them, my own mental models.

To the question, “What qualifies the usual informal mathematical arguments as ‘proof’? I can now answer, in two parts. First of all, the usual informal mathematical arguments are accepted as “mathematicians’ proof” if they convince readers who are qualified—who possess the appropriate mental models. This “convincing” is based on observing—directly verifying—that their own mental models have the properties claimed in the “proof.” This is the way new results are added to established mathematics.

Secondly, the question then arises of “qualifying” established mathematics itself. Some have tried to do this, by providing it with a foundation. But such work, interesting and fruitful though it may be, leaves us asking, what “qualifies” the proposed “foundation”? With or without “foundations,” the world’s businesses, budgets, machinery and technology operate on the basis of established mathematics. As many have already said, this in the end is what “qualifies” it. Mathematics is part of life, which does not have to be qualified.

When we do “applied mathematics,” we relate mathematical entities to physical ones. But even then, it would usually be wrong and misleading to think of the mathematical entity as being in the first place a representation of a physical
one. This fact is obvious for 4- (or 5- or 6-) dimensional geometry, since ordinary
classical space is limited to 3 or fewer dimensions. The \( n \)-dimensional hypersphere
is a mathematical entity in the minds of individual mathematicians. All math-
ematicians acquainted with the \( n \)-sphere will give the same answers to test questions
about it. True, the \( n \)-sphere can be defined axiomatically, but that possibility is
simply one part of the information contained in the mathematical model. Math-
ematicians’ command of the \( n \)-sphere is based largely on intimate acquaintance
with the special cases \( n = 1 \) and 2, together with a refined sense of how parts of
that knowledge can or cannot be carried over to higher cases. In physics, there
are “phase spaces”, which we can think of in terms of four- or more-dimensional
geometry. But the study of four- or higher-dimensional space does not depend on
such interpretations.

To complete a mathematicians’ proof, one usually must also include calcu-
lations. Any special field of mathematics has its associated calculations, which
are often done by machine, using programs such as Matlab or Mathematica. A
mathematical discovery or proof often involves an insight that a certain fact can
be discovered or verified by a certain calculation. Then one does the calculation,
to see if the insight is borne out or rejected. But to interpret the calculations in
mathematicians’ proof as meaning that such proofs are essentially symbol-pushing
would be an elementary and fatal mistake. Indeed, one of the gravest signs that an
aspiring mathematics student is out of his depth, and may be in the wrong class-
room, is being caught thinking or talking formally: concentrating on the symbols,
rather than interpreting the symbolism as representing concepts. Mathematical
thinking is conceptual thinking. The symbols can be transformed or replaced while
the meaning remains the same.

**Relation between formal proof and mathematicians’ proof**

The heritage of Leibniz, Frege and Russell persists in the notion that mathe-
natical proof is, can be, or should be reduced or reducible to syntactics (first-order
logic). Impressive formalizations of significant parts of mathematics have actually
been achieved, showing that it is actually possible. (See Hales and Gonthier.) To
the extent that such formalizations are mathematical projects, they must be carried
out by the “usual informal” kind of mathematical reasoning.

Logic and mathematics are two distinct strands of rational thinking. Formal
logic is sometimes thought of as a model of how mathematicians think, and on other
occasions as a guide to how they ought to think. Simultaneously, mathematicians
recognize mathematical logic as “another branch of mathematics, like geometry
or number theory”, with practitioners thinking in the “usual, informal” mode of
mathematicians. The two distinct strands are doubly intertwined. Indeed, their
mutual fertilization is possible precisely because they are distinct.

When it is mentioned that mathematicians’ “proof” is not formal proof, the
discrepancy is sometimes explained by saying that mathematician’s proof is an ab-
breviation of a formal proof. “For convenience and readability, mathematicians find
it advisable to leave out logically necessary steps in their proofs.” So a mathemati-
cian’s proof is then supposed to be a token or a promise of a full logical Proof.
It has even sometimes been said that a mathematician must be prepared to fill in
the gaps—to turn his/her mathematicians’ proof into a formal proof, if someone
requires him/her to do so.
But the mathematician, in most cases, has neither the knowledge, the ability, the interest, nor the willingness, to do anything of the kind. And our logically defective proofs actually do what they are meant to do: convince our fellow mathematicians!

“Well then!” the logician may rightfully ask, “Let the mathematician tell me what he or she means by proof?”

A reasonable request. Since it isn’t Proof as Proof is known in logic, how does it serve the purpose of proof—namely, to compel agreement?

“The working mathematician can be likened to an explorer who sets out to discover the world. One discovers basic facts from experience. We run up against a reality that is every bit as incontestable as physical reality.” (Connes, Conversations, p. 12)

What is this “reality”? It is our internal concepts, our mental models, which are real objects, with real properties, and which are congruent to each other, which fit together and match. That’s the sense in which it is “the same thing” that we are looking at. (The notion of the mathematician’s “mental model” was amplified in my article “Mathematical Intuition”.)

The term “mental model” is meant to suggest substantial existence or autonomy. The common term “representation” is misleading, because it suggests that some other more substantial or even physical entity is being “represented”.

The formal proof is an important “model” of mathematical proof. This sense of the word “model” is different from that in the rest of the present article! As in other parts of applied mathematics, here “model” means a mathematical structure that has an interpretation in terms of a certain “real-world” phenomenon. In this instance, using formal proof as a “model” for mathematicians’ proof, the formal proof is the mathematical structure, and the “real-world” phenomenon is mathematics itself.

The zero'th principle of mathematical modeling is, DO NOT confuse or identify the model with the process being modeled. Fluid dynamics is not water. We study water waves by means of fluid dynamics, but that is no substitute for diving and swimming!

One may use formal proof as a “model” of mathematicians’ proof, but mathematicians’ proof is not formal proof. Our starting point is established mathematics, not some postulated axioms, and our reasoning is “semantic”, based on the properties of mathematical entities, rather than “syntactic”, based on properties of formal sentences. This testimony may be called “anecdotal,” but it is not fictitious, it is lived experience. It is real evidence.

The formal logic model of mathematics is a great success. The model of mathematics as a formal system has made possible many amazing insights—going far beyond its monumental beginnings (Godel’s incompleteness theorem and Turing’s unsolvability theorem). But the very reason for taking the trouble to model mathematics is the interest and importance of mathematics itself. One may be interested in mathematics itself, as well as in its models, even though such an interest does not produce deep theorems or powerful technologies.

Many mathematicians testify that mathematical knowledge is strictly analogous to the empirical scientist’s knowledge of his/her objects of study. Connes and Schutzenberger plainly say that they see no difference between themselves, studying mathematical concepts, and entomologists, studying insects.
Cognitive neuroscience has a different way of describing and studying the objects of thought, locating them in the nervous system and the brain. Our subjective or introspective testimony is not competing with or contradicting the testimony of neuroscience. It is complementary to it. These two different kinds of descriptions are complementary ways of reporting on our “internal mathematical objects”, or “mental mathematical models”.

**Aristotle, Kant, and Locke**

Mathematician’s proof is based on a kind of direct seeing—internal “seeing”, of course, rather than external. This is not a novel or unfamiliar opinion. Wedberg cites both Aristotle and Kant:

“Aristotle emphatically asserts that the geometrical figures have only a potential existence before they are brought to actuality through the geometer’s thinking...the geometer’s thinking is an actuality...it is by making constructions that people come to know them.” [Metaphysics 1051 a 21–33. Wedberg page 88]

“As soon as the appropriate construction has come into existence, the construction itself is directly seen and the proposition to be proved is immediately understood to be true.”

[Not usually that the proposition to be proved is immediately understood to be true, but rather, that each step in the argument is immediately seen to be true, from the preceding step. But still, it is SEEN to be true, not syntactically deduced to be true.]

“Aristotle seemingly anticipates some of Kant’s most characteristic views, viz. (a) that the geometer carries out constructions in an intuitively given space, and that (b) that in establishing geometrical theorems the geometer makes essential use both of logical deduction from axioms and direct inspection of his construction in the intuitively given space.” [p 89]

John Locke is another respected philosopher who had similar views; see the quotations in my *What is mathematics, really?*

These references to Aristotle, Kant and Locke show that what I am saying is not novel or unprecedented among philosophers.

**Is this mere Platonism?**

The evidence and argument in this article is making two points.

First: mathematical concepts are neither fictional nor transcendental, they are real mental entities, with definite properties of which we can have reliable knowledge.

Second: mathematicians’ proof (“informal proof,” as some would have it) works, it is compelling, because it uses direct observations of mental entities accessible to the mathematician.

Is this mere Platonism? With Platonism it shares the assertion, that mathematicians directly perceive properties of mathematical objects, but WITHOUT giving those objects any superhuman, transcendental or eternal existence “out there.” They are “down here”, in the shared, or collective, or public consciousness of thinkers, of us humans.

Recognizing and accepting that mathematician’s concepts are real entities (as thoughts in the minds of individual mathematicians, and as the shared equivalence
class of such models)—will help clarify an old puzzle about standard and non-standard models. The puzzle arises when a formal axiomatic system has several mathematically distinct interpretations. The formal system can’t tell which interpretation is the “standard” or “intended” one. But if we step away from the formal axioms, we can say that the standard or intended interpretation is just the one that is actually present in the thinking of mathematicians, whether or not one is able to characterize it axiomatically.

In arithmetic, a nonstandard interpretation of the Dedekind-Peano axioms was discovered by the Norwegian logician Thoralf Skolem. In set theory, Paul Cohen created non-standard interpretations of the Zermelo-Frankel axioms. Cohen viewed accepting or rejecting the Continuum Hypothesis (CH) as a matter of choice or preference. For the present time, the standard interpretation is to leave CH undecided. On the other hand, Hugh Woodin is working to decide the correct choice, based on its set-theoretic consequences.

**Heron’s area theorem**

I will take as an example a simple modern derivation of a classical formula for the area of a triangle, as a function of the lengths of the three sides. It is called Heron’s formula, but may actually be due to Archimedes. Although it is simple and useful, it’s not in Euclid, and it isn’t taught in standard high-school geometry. The traditional proof relies on similar-triangles constructions that are long and tricky. Dunham [William Dunham, *Journey through Genius*, Penguin Books, New York, 1990] gives a very nice presentation. He calls the formula “surprising” and “un-intuitive”, because it involves a square root operation and an unfamiliar variable $s$, the semi-perimeter (half the sum of the lengths of the sides). I will present an easy derivation of Heron’s formula, using simple high-school algebra, in order to illustrate the points I have been making about mathematical thinking and mathematicians’ proof.

(This presentation is similar to the one in “On the interdisciplinary study of mathematical practice, with a real live case study”, Chapter 13 of “Perspectives on Mathematical Practices”, Ed. B. V. Kerkhove et al., Springer, 2007, which is the next article in this book. The same idea had previously been published by R. C. Alperin as a classroom note. [“Heron’s area formula,” *The College Mathematics Journal* 18 (1987) 137–138].)

As presented in standard references, Heron’s formula gives the area of a triangle with sides of length $a$, $b$ and $c$ as the square root of

$$s(s - a)(s - b)(s - c).$$

$s$ is the “semi-perimeter”, $(a + b + c)/2$. In the proof attributed to Heron, $s$ is the radius of a certain circle essential for the proof.

When I read the proof in Dunham’s book, it seemed to me unreasonable to work so hard and be so tricky in order to prove something so simple.

We learn in 10th grade that triangles with equal corresponding sides are congruent to each other, so of course they have the same area. Therefore, the area is a function of the three side-lengths $a$, $b$, and $c$. Why should the formula involve the irrelevant quantity, $s = (a + b + c)/2$?

This objection to the standard Heron formula is esthetic. Yet it will be understood and accepted by any mathematician.
So we replace \( s \) by \( (a + b + c)/2 \) and simplify. There now appears under the square root sign a new product of four factors:

\[
((a + b + c)/2)[(-a + b + c)/2][(a - b + c)/2][(a + b - c)/2].
\]

This expression is more natural and appealing. The three variables \( a, b, \) and \( c \) are all treated alike. That’s as it should be, since the area doesn’t depend on how we label the three side lengths. (This remark, introducing symmetry reasoning, while completely elementary, goes beyond Euclid and introduces a modern viewpoint.)

In this formula, the three factors with minus signs are not what we would expect. They’re like bumping against a piece of furniture. Where do they come from?

Let’s visualize all sorts of triangles, acute and oblique, swimming around, changing size and shape as they swim around. Some of them degenerate when a vertex approaches and collapses onto the opposite side.

Wait! Look at that! The area will be zero! When sides \( b \) and \( c \) collapse onto side \( a \), and \( a = b + c \), then the triangle degenerates to a line segment, and the area is zero!

Insight! A link between algebra and geometry! The area must be zero, if any side length equals the sum of the other two! Now pull in a little algebra from 11th grade high school—the Factor Theorem. If a polynomial in a variable \( a \) equals 0 when \( a = b + c \), then the first degree expression \( (-a + b + c) \) must be a factor of the polynomial. And by symmetry, the same must also be true for \( (a - b + c) \) and \( (a + b - c) \).

That’s it! Almost everything is explained. If the area is a polynomial in the side length \( a \) (for example), then the expressions \( (-a + b + c) \), \( (a - b + c) \), and \( (a + b - c) \) must be factors of the area formula, because when \( a \) equals \( (b + c) \) or \( (b - c) \) or \( (-b + c) \), the area will be zero.

This little insight is like Wiles’ light going on, making the furniture visible.

But wait a minute—that cannot be! We’re back in the dark, with the furniture banging against our legs. The area of a triangle scales quadratically with length. (If you double all the sides of a triangle, for example, the area is multiplied by four.) The expression we were just imagining is a product of three first-degree factors, it has third degree, it would get multiplied by 8, not by 4. So it is wrong!

(We have actually proved a little proposition, not deserving to be called a theorem: “To any polynomial in three variables \( a, b, c \) satisfying the triangle inequality (the sum of any two side lengths is greater than the third side length) there is a triangle with side lengths \( a, b, c \) whose area does NOT equal the value of that polynomial.”)

But how do we finish the derivation of Heron’s formula?

It has to contain three distinct first-degree factors, but area cannot do so, because it is homogeneous quadratic. Then why not try the next best thing, the SQUARE of the area? That will be a homogeneous fourth-degree or “quartic” expression. We can get such a thing by just multiplying the three linear expressions we already have by one more first-degree expression.

That’s just a guess, but it’s the simplest one.

The first-degree expression has to be symmetric in \( a, b \) and \( c \), so it can only be \( (a + b + c) \), or maybe \( (2a + 2b + 2c) \), or \( (3a + 3b + 3c) \).
It is now clear that while the formula is NOT a polynomial, it COULD be the square root of a quartic, of the form
\[ k \sqrt{[(a + b + c)(-a + b + c)(a - b + c)(a + b - c)]} \]
where \( k \) is some numerical constant. To determine the constant \( k \), choose \( a = b = 1 \), and \( c = \sqrt{2} \). This is a right triangle with area \( \frac{1}{2} \). Our formula becomes
\[ \frac{1}{2} = k \sqrt{[(2 + \sqrt{2})(\sqrt{2})(\sqrt{2})(2 - \sqrt{2})]} \]
After simplifying we get \( \frac{1}{2} = k \sqrt{4} \), so \( k = \frac{1}{4} \).

This is Heron's formula. We have proved that IF the area of an arbitrary triangle is the square root of a quartic function of the side lengths, then it is given by Heron's formula.

We have not yet proved Heron's formula completely, for we used an extra hypothesis. But once the formula is in our hands, a routine exercise finishes up, by Cartesian coordinates, or the law of cosines. The interesting part was deriving the formula. How did we accomplish that?

First we looked for the simplest formula, a polynomial in \( a \), \( b \), and \( c \). The key insight was that the geometric condition for degeneracy—a vertex collapsing onto its opposite side—implies an algebraic condition—a certain first-degree expression must appear as a factor in the area formula. By symmetry, the formula would then have to contain at least three first-degree factors, and so would be of degree at least three. But we know that area is a quadratic scale function. Therefore we see that the formula CANNOT be a polynomial, and we guess the next simplest possibility—square root of a quartic. Using symmetry again, we see that only one quartic is possible, except for a constant factor, which we determine by choosing a convenient special triangle.

We used an insight connecting algebra to geometry, and then a geometry theorem, an algebra theorem, some plausible reasoning, and symmetry.

Most of this would be absent from anyone's list of axioms for elementary geometry. But all of it is available to an educated mathematician. Our derivation is convincing to anyone who understands the Factor Theorem, and the scaling of area, and reasoning by symmetry. This exercise is a little example of how mathematicians “prove”. We do it by citing properties of the entities in question. Shared knowledge of those properties compels final agreement.

It would be easy to pad out this derivation formally, and then rewrite it syntactically. But to what purpose?

Instead, the mathematically natural next step is to generalize to three dimensions.

Just as three non-collinear points in the plane define a triangle, four non-coplanar points in space define a tetrahedron, or triangular pyramid. What is the analog for the tetrahedron of Heron’s formula for the area of a triangle?

A natural guess is a formula for the volume, as a function of the lengths of the six edges. It “should be” the square root of a sixth-degree symmetric polynomial in six variables.

This natural conjecture FAILS! An account of how and why is given in op. cit.

The lesson is: a mathematical object is what it is, not what I might want it to be or imagine it to be! It may take a few bruises to the “knees and elbows” (speaking metaphorically) to recognize relevant properties or facts about this mental object.
Conclusions

This little excursion into plane geometry and high-school algebra is meant as an example of the way mathematicians come to conclusions and convince each other of their results.

The recognition that mental mathematical models or internal mathematical entities actually exist, are real things about which factual statements can be made, is the key to understanding mathematicians’ proof. Mathematicians’ proof depends on mathematicians possessing, recognizing and communicating congruent samples of certain equivalence classes of mental models. These equivalence classes of mental models are ordinarily referred to as “mathematical concepts”. They correspond to brain processes which are congruent, or equivalent.

The claim that I am making is speculative. It asserts that certain things are going on in the brain or the nervous system that we are not able to directly observe or verify. The claim is based on the testimony of mathematicians about what they experience, and on the plain fact: mathematicians do make discoveries about mathematical entities by just thinking about them, and these discoveries are ordinarily verified by other mathematicians by thinking about them (and also doing some calculations).

Acknowledgments

Brendan Larvor, Vera John-Steiner, Edward Dunne, David Edwards, Colin McLarty, Martin Davis, Ulf Persson, Chandler Davis, Joe Auslander and Carlo Cellucci all provided essential sound advice and moral support.

Appendix

Following a reference in Timothy Gowers’ blog, I came upon a wonderful entry in the online website called Quora. http://www.quora.com/Mathematics/What-is-it-like-to-have-an-understanding-of-very-advanced-mathematics

On Quora, people post interesting questions, and wait for someone to answer them. Someone asked Quora, “What is it like to have an understanding of very advanced mathematics? I’m interested to hear what very talented mathematicians and physicists have to say about ‘what it’s like’ to have an internalized sense of very advanced mathematical concepts, just what it really feels like, to be a professional mathematician.”

That question is one that I myself have been trying to answer, for years. The response by Anonymous is by far the best thing on this question that I have ever seen. I could not squeeze these excerpts into the text of this article, so here they are, as an Appendix.

Answer by “Anonymous”:

“As you get more mathematically advanced, the examples you consider easy are actually complex insights built up from many easier examples.”

“Once you know these threads between different parts of the universe, you can use them like wormholes to extricate yourself from a place where you would otherwise be stuck.”

“The accomplishment a mathematician seeks is finding a new dictionary or wormhole between different parts of the conceptual universe.”
“You can answer many seemingly difficult questions quickly. But you are not very impressed by what can look like magic, because you know the trick. The trick is that your brain can quickly decide if a question is answerable by one of a few powerful general purpose ‘machines’ (e.g., continuity arguments, the correspondences between geometric and algebraic objects, linear algebra, ways to reduce the infinite to the finite through various forms of compactness) combined with specific facts you have learned about your area. The number of fundamental ideas and techniques that people use to solve problems is, perhaps surprisingly, pretty small.”

“You are often confident that something is true long before you have an airtight proof for it (this happens especially often in geometry). The main reason is that you have a large catalogue of connections between concepts, and you can quickly intuit that if X were to be false, that would create tensions with other things you know to be true, so you are inclined to believe X is probably true to maintain the harmony of the conceptual space. It’s not so much that you can imagine the situation perfectly, but you can quickly imagine many other things that are logically connected to it.”

“You are comfortable with feeling like you have no deep understanding of the problem you are studying. Indeed, when you do have a deep understanding, you have solved the problem and it is time to do something else. This makes the total time you spend in life reveling in your mastery of something quite brief. One of the main skills of research scientists of any type is knowing how to work comfortably and productively in a state of confusion.”

“Your intuitive thinking about a problem is productive and usefully structured, wasting little time on being aimlessly puzzled. For example, when answering a question about a high-dimensional space (e.g., whether a certain kind of rotation of a five-dimensional object has a “fixed point” which does not move during the rotation), you do not spend much time straining to visualize those things that do not have obvious analogues in two and three dimensions. (Violating this principle is a huge source of frustration for beginning maths students who don’t know that they shouldn’t be straining to visualize things for which they don’t seem to have the visualizing machinery.)”

“When trying to understand a new thing, you automatically focus on very simple examples that are easy to think about, and then you leverage intuition about the examples into more impressive insights. For example, you might imagine two- and three-dimensional rotations that are analogous to the one you really care about, and think about whether they clearly do or don’t have the desired property. Then you think about what was important to the examples and try to distill those ideas into symbols. Often, you see that the key idea in the symbolic manipulations doesn’t depend on anything about two or three dimensions, and you know how to answer your hard question.”

“As you get more mathematically advanced, the examples you consider easy are actually complex insights built up from many easier examples; the “simple case” you think about now took you two years to become comfortable with. But at any given stage, you do not strain to obtain a magical illumination about something intractable; you work to reduce it to the things that feel friendly.”

“To me, the biggest misconception that non-mathematicians have about how mathematicians think is that there is some mysterious mental faculty that is used to crack a problem all at once. In reality, one can ever think only a few moves...
ahead, trying out possible attacks from one’s arsenal on simple examples relating
to the problem, trying to establish partial results, or looking to make analogies
with other ideas one understands. This is the same way that one solves problems
in one’s first real maths courses in university and in competitions. What happens
as you get more advanced is simply that the arsenal grows larger, the thinking
gets somewhat faster due to practice, and you have more examples to try, perhaps
making better guesses about what is likely to yield progress. Sometimes, during
this process, a sudden insight comes, but it would not be possible without the
painstaking groundwork.”

“You go up in abstraction, ‘higher and higher’. The main object of study
yesterday becomes just an example or a tiny part of what you are considering
today. For example, in calculus classes you think about functions or curves. In
functional analysis or algebraic geometry, you think of spaces whose points are
functions or curves—that is, you ‘zoom out’ so that every function is just a point in
a space, surrounded by many other “nearby” functions. Using this kind of zooming
out technique, you can say very complex things in short sentences—things that, if
unpacked and said at the zoomed-in level, would take up pages. Abstracting and
compressing in this way allows you to consider extremely complicated issues while
using your limited memory and processing power.”

“Learning the domain-specific elements of a different field can still be hard—for
instance, physical intuition and economic intuition seem to rely on tricks of the brain
that are not learned through mathematical training alone. But the quantitative and
logical techniques you sharpen as a mathematician allow you to take many shortcuts
that make learning other fields easier, as long as you are willing to be humble and
modify those mathematical habits that are not useful in the new field.”

“You move easily between multiple seemingly very different ways of representing
a problem. For example, most problems and concepts have more algebraic represen-
tations (closer in spirit to an algorithm) and more geometric ones (closer in spirit
to a picture). You go back and forth between them naturally, using whichever one
is more helpful at the moment.”

“Indeed, some of the most powerful ideas in mathematics (e.g., duality, Ga-
en.wikipedia.org/wiki/Alg...]) provide “dictionaries” for moving between
“worlds” in ways that, ex ante, are very surprising. For example, Galois theory
allows us to use our understanding of symmetries of shapes (e.g., rigid motions of
an octagon) to understand why you can solve any fourth-degree polynomial equa-
tion in closed form, but not any fifth-degree polynomial equation. Once you know
these threads between different parts of the universe, you can use them like worm-
holes to extricate yourself from a place where you would otherwise be stuck.”

“Understanding something abstract or proving that something is true becomes
a task a lot like building something. You think: “First I will lay this foundation,
then I will build this framework using these familiar pieces, but leave the walls to fill
in later, then I will test the beams...” All these steps have mathematical analogues,
and structuring things in a modular way allows you to spend several days thinking
about something you do not understand without feeling lost or frustrated. (I should
say, “without feeling unbearably lost and frustrated; some amount of these feelings
is inevitable, but the key is to reduce them to a tolerable degree.”)"
“In listening to a seminar or while reading a paper, you don’t get stuck as much as you used to in youth because you are good at modularizing a conceptual space, taking certain calculations or arguments you don’t understand as “black boxes”, and considering their implications anyway. You can sometimes make statements you know are true and have good intuition for, without understanding all the details. You can often detect where the delicate or interesting part of something is based on only a very high-level explanation. You are good at generating your own definitions and your own questions in thinking about some new kind of abstraction.”

“On the other hand, you are very comfortable with intentional imprecision or “hand-waving” in areas you know, because you know how to fill in the details.”

“[After learning to think rigorously, comes the] ‘post-rigorous’ stage, in which one has grown comfortable with all the rigorous foundations of one’s chosen field, and is now ready to revisit and refine one’s pre-rigorous intuition on the subject, but this time with the intuition solidly buttressed by rigorous theory. (For instance, in this stage one would be able to quickly and accurately perform computations in vector calculus by using analogies with scalar calculus, or informal and semi-rigorous use of infinitesimals, big-O notation, and so forth, and be able to convert all such calculations into a rigorous argument whenever required.) The emphasis is now on applications, intuition, and the ‘big picture’. This stage usually occupies the late graduate years and beyond.”

“In particular, an idea that took hours to understand correctly the first time (“for any arbitrarily small epsilon I can find a small delta so that this statement is true”) becomes such a basic element of your later thinking that you don’t give it conscious thought.”

(From “QUORA”, Thursday, Dec 15, 2011. QUO)

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On the interdisciplinary study of mathematical practice, with a real live case study

(Published as Chapter 13 of Perspectives on Mathematical Practices, Ed. B. V. Kerkhove et al., Springer, 2007)

Abstract: The study of mathematical practice is not the private property of philosophers. Many other disciplines, especially including mathematicians, are engaged in it. As a case study, an elementary derivation of Heron's area formula is presented, and then analyzed methodologically and ontologically.

Keywords: Mathematical practice, Heron's formula, tetrahedron, mathematical existence

Of course, various special sorts of math studies have been going on for a long time. Philosophical, since Pythagoras/Plato. Historical, since Proclus. Anthropological, sociological and psychological, somewhat more recently. Now starting to see neurophysiological studies of math. Sometimes the different specialties have been fiercely autonomous. Gottlob Frege (to whom all contemporary philosophy of math is said to be but a footnote) banned psychologizing and historicizing from the philosophy of mathematics. On the other hand, André Weil, the great number theorist, entered the history of math with a dictum: no two disciplines have less in common than the history of mathematics and the philosophy of mathematics.

The conference from which this paper originates was called by philosophers. Two mathematicians were on the program, and there were mathematics educators, and a sociologist, and some historians. I hope this interdisciplinary approach will be continued and extended. Imre Lakatos (paraphrasing Immanuel Kant) said, as I remember, “Without history, the philosophy of mathematics is sterile; without philosophy, the history of mathematics is aimless.” Yet I hear quiet intimations that the participation of mathematicians themselves is—shall I say—unexpected? This is a lamentable misapprehension! We mathematicians have a special, direct, intimate acquaintance with mathematical practice. Some of us have written about our practice.

Everyone knows George Polya's famous books on mathematical heuristics—a salient aspect of mathematical practice. The articles and books of Raymond Wilder are not obscure. He was a leading mathematician who applied the viewpoint of anthropology to his own field. There are the famous writings of Henri Poincaré on the psychology of mathematical discovery. And the famous book on that subject by his student Jacques Hadamard. The memoirs of Gian-Carlo Rota, Alfred Rényi, John Littlewood, G. H. Hardy, Norbert Wiener, Paul Halmos, Stanislaus Ulam, Mark Kac, Paul Lévy, Laurent Schwartz, are rich with comments and revelations on mathematical practice, as are the two volumes of interviews, “Mathematical People” and “More Mathematical People.” It is true is that mathematicians' talk about mathematical practice is not in the logicist style inherited from Russell, Carnap and Quine, nor is it ornamented with statistically massaged “quantitative
data” in the prevalent style of Anglophone psychology or sociology. But surely such shortcomings can be tolerated.

Enough querulous comments. I now turn to a more constructive tone. What would be the goal of a study of mathematical practice? What questions would one hope to answer, or at least clarify?

Here are some obvious questions that are subject to standard research techniques: Who are these mathematicians, sociologically, psychologically, anthropologically? How are their activities organized—institutionally, politically, economically? What are their attitudes and emotions about mathematical activity? Etc.

These are questions one asks about any sect or clique. But the point of studying mathematical practice is to understand its uniqueness. Its special character that makes it what it is. The invisibility, intangibility, of its objects of study. The unique consensus or “certainty” of its “results.” And its “unreasonable” effectiveness in understanding and mastering Nature. These are the big philosophical questions. It’s not clear how studying mathematical practice can answer them.

When Carnap or Quine philosophized about mathematics, they philosophized about what analytic philosophy postulated mathematics to be. Not about what mathematicians actually do. Why then do some philosophers now think that mathematical practice is philosophically interesting? Lakatos exposed this scandal. Philip Kitcher made an impressive effort to give a philosophical description of mathematical practice. Perhaps the impulse for philosophers to study mathematical practice now is their response to the exposure of the irrelevance of their predecessors.

As a mathematician invited to this project, I can contribute a case study from my own mathematical practice, presented in as philosophical a style as I can summon up. (The statement of Heron’s formula is the same as in the previous article, but the subsequent methodological discussion is different and more elaborate. It aimed for a phenomenological attitude.)

Here is Heron’s formula:

If $a$, $b$, $c$ are the lengths of the sides, and $s$, (the “semi-perimeter”) $= (a+b+c)/2$, then the area is $\sqrt[]{s(s-a)(s-b)(s-c)}$.

(For example, take the right triangle with sides 3,4,5. By the elementary formula “area = 1/2 base times altitude,” it has area $1/2 \times 3 \times 4 = 6$. The semi-perimeter is 6. Heron gives $\sqrt[]{(6 \times 1 \times 3 \times 2)}$ which of course checks.)

The excellent book “Journey Through Genius” by William Dunham takes seven pages for the proof. Dunham comments, “This is a very peculiar result, which, at first glance, looks like nothing if not a misprint. The presence of the square root and semiperimeter seems odd, and the formula has no intuitive appeal whatever. The proof that Heron furnished is at once extremely circuitous, extremely surprising, and extremely ingenious.”

I will go through my thoughts and afterthoughts, in 40 very short steps. Then I will look back, and look for some general conclusions.

1. The first step was reading the classical proof in Dunham’s fine book and being bothered that while Heron’s formula is so simple, its classical proof is so complicated. Such a simple formula “must” have a simple, natural proof.

2. The next step is to formulate this “feeling” as a problem: Find a simple, natural proof of Heron’s area formula. This is a short step, but it is essential. Only this second step leads to action.

3. Stare at the formula. Look for something to do with it or to it.
4. Notice that the letter “s”, the semi-perimeter, can be eliminated. (In Heron’s proof, the semi-perimeter plays a starring role.)

5. A purely “mechanical” step: substitute the definition of “s” into Heron’s formula, and combine terms. You get a “new” formula:

$$\text{Area} = \sqrt{\left(\frac{a+b+c}{2}\right)\left(\frac{-a+b+c}{2}\right)\left(\frac{a-b+c}{2}\right)\left(\frac{a+b-c}{2}\right)}$$

6. This formula makes even clearer the symmetry between the three letters a,b,c.

7. Under the square root sign we have a polynomial. In fact, a symmetric polynomial in three variables a,b,c. This simple remark is the key insight! It makes the crucial connection with Algebra!

8. As soon as a polynomial is noticed, I think of finding its roots—finding the values of a,b and c that make it equal zero. Why? Because solving polynomials—finding their roots—is what one usually has to do to a polynomial. This polynomial is already factored! I see immediately that it equals zero if and only if either $(a+b+c)$ or $(-a+b+c)$ or $(a-b+c)$ or $(a+b-c)$ equals zero.

9. But what does that mean geometrically? What would that say about the triangle?

10. Since the side lengths a,b,c all are positive, $a+b+c$ cannot equal zero.

11. If $-a+b+c=0$, then $a=b+c$.

12. That says that two side lengths are equal to the third. The triangle is “degenerate.” Side “a” lies on top of sides “b” and “c”. The area is zero.

13. Of course! If the triangle is degenerate, the area MUST equal zero! The formula for the area must vanish to zero when $a = b + c$.

14. But that means that $-a+b+c$ must be a factor in the area formula. (This is the “Factor Theorem”, from the 11th grade.)

15. There is nothing special about “a” among the three letters. Therefore, along with $(-a+b+c)$, $(a-b+c)$ and $(a+b-c)$ must also be factors. This interchangeability of the letters is called “symmetry.”

16. With at least three distinct factors, the area as a function of the side lengths would have to have degree at least three.

17. But area scales like a quadratic, a second power of lengths. I could get a quadratic function by taking the two-thirds power of a cubic. But that seems ugly and unnatural. It is much more “natural” to look for one more first-degree (“linear”) factor, and then take the square root of the resulting fourth-degree (quartic) expression.

18. What could the fourth linear factor be? It would have to preserve symmetry in the three letters $a,b,c$. So it could only be a linear combination of them with equal coefficients. I may as well take those coefficients to be 1.

19. So the vanishing of the degenerate cases and symmetry give me all the factors of the area function, assuming the area is the square root of a quartic polynomial. I still have to determine an arbitrary constant factor of multiplication.

20. To find that constant, I can use any triangle of known area. If $a=3, b=4, c=5$, area = 6, I find that

$$\sqrt{(a+b+c)(-a+b+c)(a-b+c)(a+b-c)}$$

is $\sqrt{12 \times 6 \times 4 \times 2}$ which is 24. So I must multiply by $1/4$ to get the area, 6.
21. I have derived an area formula:

\[ \text{Area} = \frac{1}{4} \sqrt{[(a + b + c)(-a + b + c)(a - b + c)(a + b - c)]} \]

which is the same as Heron. (In Heron, each factor under the square root sign is divided by 2, which gives a denominator of 16 under the square root sign. If you take the denominator outside the square root sign, you must take the square root of 1/16, and get 1/4.)

22. Am I done? I have obtained a quick derivation of Heron's formula, using: the symmetry of the three letters \( a, b, c \), the requirement that the area equal zero when the triangle is degenerate, the quadratic dependence of area on length, the area for the special triangle with sides 3, 4, 5, AND the assumption that the area is the square root of a quartic. But what right do I have to assume THAT?

23. The area formula must contain at least three independent linear factors, so it can't just be a quadratic polynomial. Could it be the cube root of a sextic, or the fourth root of an octic?

24. To eliminate these possibilities, I need to verify this formula—prove it. My derivation, which leads me to find a formula, is not a proof that the formula works correctly in all cases.

25. In preparation for a proof, I multiply out the four linear factors. The product will be of fourth degree, symmetric in the three variables \( a, b, c \). Furthermore, replacing \( a, b, \) or \( c \) by its negative leaves the product unchanged, so the product is an “even” function. It can contain only even powers, no first or third powers.

26. Consequently, the resulting multiplied-out quartic can only contain 4th powers, products of two squares, and possibly an additive constant.

27. Because the area is zero if any of the sides is zero, the additive constant must equal zero. So the multiplied-out form, by symmetry, must be a linear combination of two expressions: the sum of three fourth powers, and the sum of three products of two squares.

28. To find the two unknown coefficients, I check with two specific triangles, and obtain the formula

\[ \text{area} = \frac{1}{4} \sqrt{2\{(a^2)(b^2) + (b^2)(c^2) + (a^2)(c^2)\} - \{a^4 + b^4 + c^4\}} \]

Of course this is the same thing I would obtain by multiplying out from the factored form.

29. How can I verify this formula—prove it is valid for all triangles?

30. I know a standard trick—introduce x-y coordinates.

31. For maximum simplicity of algebra, I put the origin at a vertex of the triangle, with the x-axis on one side of the triangle, and the point \( x = 1, y = 0 \) at a second vertex. Then I choose the unit of length on the y-axis equal to the corresponding altitude. The third vertex is then at \((x, 1)\) for some value of “\(x\)”.

32. The base of the triangle has length 1, and the altitude has length 1, so the area in these coordinates is 1/2, independent of “\(x\)”. The side lengths squared are now \(a^2 = 1, b^2 = x^2 + 1,\) and \(c^2 = (x - 1)^2 + 1\).

33. Plugging these expressions into my multiplied-out area formula, the variable “\(x\)” cancels out, the algebra simplifies to the correct answer, 1/2, and Heron’s formula is verified.

34. FINISHED! I have derived and then proved Heron’s formula, without any ingenious geometrical constructions, and without trigonometry!! (In fact, from this independent proof of Heron, I can get an independent derivation of the law
of cosines. This is a generalization of the Pythagorean theorem, which Dunham laboriously derives from Heron.)

35. What next? I won’t stop now, after only one victory. What new problem is now inviting me?

36. Is there a Heron’s formula for the non-Euclidean triangle? Is there a Heron’s formula for the Euclidean quadrilateral? Is there a Heron’s formula in three dimensions, for the volume of a tetrahedron?

37. I checked with three friends (an editor of the *College Mathematics Journal*, a colleague who is a well-known expert on problem-solving, and my respected mentor Peter Lax.) None of them had seen my proof before. All encouraged publication. Professor Lax sent me a strangely complicated yet somehow elegant formula for the volume of a tetrahedron.

38. *Focus*, a publication of the Mathematical Association of America, accepted my article [November, 2002, Volume 22, Number 8, page 22].

39. The editor of *Focus* [January, 2003, Volume 23, Number 1, page 15] received letters from readers pointing out that my derivation of Heron’s formula had already appeared in 1987, in a note in the *College Mathematics Journal*. Moreover, my formula for the volume of a certain tetrahedron was wrong, the denominator should be 12, not 6. Moreover, a formula for the volume of a tetrahedron as the square root of a sixth degree polynomial in the lengths of the sides is already in the literature, in George Polya’s *Mathematics and Plausible Reasoning*. (While writing this article, I found a reference [Dorrie, p. 285] stating that Euler had published this tetrahedron formula in 1753.)

40. I presented this story in Brussels at the meeting, “Perspectives on Mathematical Practices.” The night before my lecture, I checked Lax’s formula for the volume of a tetrahedron. It is wrong! (It turned out that he had forgotten a factor of 1/6.) Polya gives Euler’s tetrahedron formula without a derivation or proof. Lax’s derivation of it generalizes to higher dimensions. But it is unknown whether there is a factorization, even in the three-dimensional case given by Polya. There is a Heron-type formula for the area of a cyclic quadrilateral (one inscribed in a circle.) This is “Brahmagupta’s formula.” Maybe it wouldn’t be too difficult to prove Brahmagupta by some adaptation of our proof of Heron.

This detailed account of a small piece of research in elementary mathematics is offered as a case study, accessible with minimal mathematical preparation. Of course it does not claim to be typical or representative. Other stories of mathematical investigation could be told. This one is not out of the ordinary. It may suggest some hypotheses of a methodological or even ontological kind.

So, “What is going on here?”

First of all, the original motivation is “esthetic.” The classical proof seemed “too complicated” for such a simple result. There “must be” a simple proof.

Secondly, the known result is the starting point. I already know Heron’s formula is true. But I want a simpler explanation, I want to understand it more directly.

Thirdly, the work is done against my whole background of mathematical know-how. Basic facts about triangles, areas, and polynomials are assumed and used. Standard arguments and manipulations are stored in my head, ready to be called on.
Fourthly, the key observation is this: three of the factors under the square root in Heron’s formula are seen to be necessary. This insight makes the formula natural and understandable. The mystery evaporates.

Fifthly, it is a combination of two qualitative properties of area (symmetry and quadratic scaling) and a single quantitative one (vanishing of area in the degenerate cases) that suggests the form of the area function.

Sixthly, to find the final form of the formula, it is necessary to work out one special case numerically.

Seventhly, although the completed derivation is still not a proof, once the formula has been derived, the “rigorous” or complete proof is almost routine. Some insight was needed to discover the formula. Once it is known, the proof is not much more than an afterthought. Once the relationship between side lengths and area is understood, the details are easy.

These remarks suggest some properties of the part of mathematical practice that is called “research”: the discovery of new results. An interesting new proof of an old result counts as a kind of new result. Research can start by noticing something “funny”—an unexpected or unexplained analogy. A peculiar complication that intuitively seems unnecessary. One’s sense of fitness, of what feels right, can tell one where to look for something interesting.

One may not know in advance which tools, what background knowledge or know-how may come in handy as the investigation proceeds. The researcher expects to use his whole background of mathematical knowledge. Indeed, he may sometimes use analogies from outside of mathematics (perhaps from mechanics) and then his knowledge of mechanics may advance his investigation. He may need to go beyond his available skills, by going to the library or onto the Web or by calling up a friend.

The key event in the investigation may be making a fruitful connection between two different theorems or theories, two different ideas which click together. In this case, the Factor Theorem from elementary algebra, and the vanishing of the area of a degenerate triangle.

What is interesting to me may have been interesting to someone else. Once the question is asked, the answer is “there,” waiting to be found by anyone who looks hard enough. In other words, the concept we are investigating already contains the answer hidden away. It is “pregnant” with the solution. So it is normal for the same discovery to be made several times.

The investigation may lead to a formula or “theorem” (precise statement) which is convincing on the basis of the derivation. Yet there may remain some question of the precise conditions under which it is true. There may be unnecessary hypotheses or assumptions used in the derivation. (In our case, that the area is the square root of a quartic; in the final proof, this is part of the conclusion.) Consequently, there may be a need for an a posteriori proof of the result. Without such a proof, it may be called a mere conjecture. Yet the derivation which led to the conjecture may leave no doubt that it is true, at least in certain important cases. The proof sometimes may be routine, compared to the derivation.

This last fact has important consequences for exposition and teaching. To present the proof without the derivation may then be a piece of mystification. The derivation would really be the heart of the matter, even though, from a pedantically “logical” point of view, the “rigorous” proof makes the original derivation superfluous. The slogan, “Mathematics is nothing without proof” becomes false.
when it degenerates to, “Mathematics is nothing but proof.” Our example shows that sometimes proof is a less interesting and important part of the work. Finally, can we ascend from methodology to ontology? In other words, now that we have seen what is actually being done, does that tell us anything about the significance, the meaning, the import of all this conversation?

The most important thing that is forced on us is that there are facts of the matter. The area of a triangle may be defined in one way or another, but all the definitions have to match. Whatever definition you use, the area is one half the base times the altitude. (Any of the three bases, times the corresponding altitude.) And of course, Heron.

What then are we to make of the contemporary argument, whether mathematical objects are “real” or “fictional”? This is a perfectly useless question, unless some clarity is achieved on the meanings of “real” and “fictional”. One could say, “Something is real if there are verifiably true statements about it. If we can have true knowledge of it, we can say it is real.” If that is what we mean by “real”, the area of a Euclidean triangle is some sort of real entity. If by “fictional” we mean arbitrary, something which is invented at the pleasure of the inventor (Mark Twain could have called Huck Finn by any other name, and changed his story any way he liked, subject only to his own fictional goals and tastes) then the area of a triangle is not a fiction. On the other hand, if by “real” you mean only an object that is made of atoms and molecules, then area is not real, and neither is “triangle” or “polynomial” or anything else we talked about in this whole tragedy. So what’s needed, really, is a critical consideration of what we want to mean by “real”, or, equivalently, what we want to mean by “exist”.

This much can be said. Mathematics really exists. It is going on, it is taking place, it has been around a long time and is here to stay. If your vocabulary insists that it is not real, and since in any ordinary meaning of the word it is not “fictional”, then you must find some other kind of ontology, neither “real” in your sense nor fictional in any sense, to place it in. My own answer to this conundrum is presented in my book.

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Wings, not foundations!

Abstract The phrase “foundations of mathematics” is deconstructed. The notion of “phenomenological foundation” is proposed. A recent mathematical experience is offered as an example.

1. What foundations?

Foundations of mathematics? What are they? What could be, what should be the meaning of that expression?

If we may start with the utmost naivete, we can say that a foundation is something that holds up a building. Especially a big building, a skyscraper. Without a foundation, the building would fall down. Everyone understands that.

Of course mathematics is not a building. It is big, but it is not a big building. So the phrase “foundations of mathematics” is a metaphor, a figure of speech. The idea is that mathematics is like a big building, in some important respect. That similarity between mathematics and a big building would be what would make the phrase “foundations of mathematics” sensible.

But other metaphors are equally apt, or more so. Mathematics has often been compared to a tree. A tree has roots, not a foundation. Roots grow while the branches grow. Old roots could die as new roots sprout. Not like a solid, permanent foundation, which is supposed to be unchanging, once and for all. Others have even compared mathematics to a flying eagle, supported not by a solid foundation but by the motion of wings. Mathematics survives and prospers and grows by the successful activity and work of its ongoing creators, whose thoughts are its “wings.”

Perhaps the comparison to a big skyscraper came from the notion that mathematics might be tottering, might be in danger of collapse unless a foundation was inserted underneath it. Gottlob Frege may have had some such feeling, when he wrote, on receiving Bertrand Russell’s postcard announcing the famous Paradox, that he was in the position of a builder whose building crumbled just as he applied the finishing touches. But if anything crumbled, of course, it was Frege’s theory, not Mathematics!

Others feared that other set theoretic confusions, even contradictions, endangered Mathematics. Their fears proved groundless. A century passed, and mathematics is growing in all directions—solving old problems, making newer and stronger connections with every field of science. All without a Foundation! The efforts to give it a firm Foundation, by Brouwer, Hilbert, Bishop and their epigones, are not claimed by anyone to have achieved their goal. Hardly anyone even claims any more, that it is either possible or necessary, to save mathematics from tottering by providing it with a firm Foundation.
And yet people still talk about Foundations! What are they talking about?

Probably about axiomatics. That subject became prominent following the publication of David Hilbert’s Foundations of Geometry. It had already been much studied in Italy by Giuseppe Peano, Mario Pieri and others. Oswald Veblen in the U.S. was a respected practitioner. In the preface to Projective Geometry, he wrote: “Even the limited space devoted in this volume to the foundations may seem a drawback from the pedagogical point of view to some mathematicians. To this we can only reply that, in our opinion, an adequate knowledge of geometry cannot be obtained without attention to the foundations. We believe, moreover, that the abstract treatment is peculiarly desirable in projective geometry, because it is through the latter that the higher geometric disciplines are most readily coordinated.”

To escape the paradoxes of intuitive set theory, Ernst Zermelo proposed axioms for set theory. These were completed and refined by others. Later on, an axiomatic coordination of all mathematics was attempted by the Bourbaki group. Whether your axioms present one subject, like projective geometry, or many, like the Bourbaki, the axiomatic arrangement of known subject matter is a technical mathematical project. The meaning and validity of projective geometry does not depend on its axiomatic presentation. Its meaning and validity depends on its factual content, and on its connection to other mathematical subjects, to physics and science.

I once had the misfortune to become involved with a computer list-serve called FOM—an obvious acronym. To my eyes, they were working on axiomatic set theory. A project equally worthy as “axiomatic foundations of geometry.” Their goal, as well as I could grasp it, seemed to be, to find good new axioms about sets, in hope that they’d lead to an interesting or useful new theory. To me, being what some philosophers choose to call a “working mathematician” (or even, “an ordinary mathematician”) such work is a respectable, if slightly esoteric, branch of mathematics, or mathematical logic. The odd thing was this: when anyone mentioned an alternative “foundation” for mathematics (such as category theory, for example), the “moderator” of this “FOM” responded, not like a mathematician, but more in the manner of a real estate salesman hearing the name of an unwelcome competitor.

The logician Richard Grandy, when asked why the field of axiomatic set theory calls itself “Foundations”, answered as follows: This field is descended from earlier studies by Russell, Frege, Hilbert, etc., which really were motivated by a concern for foundations in a philosophical sense—somehow to establish the safety or security or certainty of mathematics, by anchoring it to something solid—something unquestionable, whether logic, concrete symbols, or the Intuition of the Creating Mathematician. As time passed, the search for such a foundation had only very limited success, and the need for or possibility of such security came to be less apparent to the community of mathematicians. But even after the original philosophical impetus faded away, the technical or mathematical work continued to call itself by the same name, much as he, Richard Grandy, might call himself Polish because, in fact, he does have one Polish grandfather.

Certainly no one claims today that “Set theory is a rock on which mathematics rests solidly and securely.” Most everybody knows that the continuum hypothesis of set theory is undecided and, by standard set theory, undecidable. Every mathematician knows, or ought to know, another result of Godel and Cohen: that
the axiom of choice can be assumed or denied, without introducing any contradictions not already present without it. With that axiom of choice we get famous indigestible things to swallow, like Zermelo’s well-ordering theorem, and the notorious Banach-Tarski theorem. In fact, it is generally acknowledged that set theory is more worrisome and insecure than number theory—for which it is supposed to provide a “foundation.” So, if everybody knows this, why do they still talk of set theory as the foundation?

In fact, set theory today does provide something important: a universal language, for rigorized or formalized mathematics. I say “today”, because mathematics is a historically developing subject. Set theory did not provide a common language for mathematics in the 17th, 18th, 19th or earlier centuries. I withhold judgment whether it will do so through the 21st century and any further centuries in which mankind may survive to practice mathematics. At any rate, in the 20th century set theory came to provide a universal language in which it was possible to formulate all rigorous mathematics.

Granting that claim, it by no means follows that such a language must be unique. There could be others. Indeed, Lawvere and some of his followers actually make a rival claim for category theory. I suspect that there have been logicians, of whom I know not, who have proposed still other candidates for such a language.

In a practical sense, such proposals are in vain. Someone already fluent in a natural language switches languages only under compelling necessity. The same is true for a mathematical language. Set theory is here. It is established. It is vain for inventors to advocate substitutes or radical improvements, as it was vain for Peano and others to advocate Esperanto or Interlingua. Of course, people interested in language from a theoretical point of view may well be interested in inventing, comparing and evaluating new languages, theoretically.

So in the common talk of “ordinary mathematicians”, the expression “foundations of mathematics” has degenerated to mean “universal language for mathematics”. This does not seem to be problematic. It does not seem to be the most pressing issue for philosophers to worry about. Yet set theory’s role as a language is taken by analytic philosophers like W.V.O. Quine to mean, philosophy of mathematics need consider nothing but axiomatic set theory! This is simply the application to mathematics of the basic mistake of “analytic philosophy”—that philosophy should be just about language.

“Foundations” in the sense of “axiomatic set theory” is simply an inherited misnomer, like “complex variables” as the standard name for “the theory of analytic functions of a complex variable”, or “real variables” as the standard name for “elementary point-set topology mixed with measure theory and introductory functional analysis.” These other inherited misnomers generally are harmless. People learn to interpret them correctly. But the “Foundations of Mathematics” misnomer is harmful, when it becomes confused with “philosophy of mathematics”. Since foundations of mathematics as a philosophical (rather than a technical mathematical) problem seems to be rather outdated and inactive, of little interest to most mathematicians, so also philosophy of mathematics may then be mistakenly thought to be of little interest to mathematicians and people seriously interested in mathematics. This confusion should be abolished forthwith and post haste. This will help make room for genuine philosophical investigations of real live problems about the nature of mathematics and mathematical practice.
Solomon Feferman, in my opinion the leading thinker on foundational questions in logic, acknowledges the apathy or even distaste among “working mathematicians” for “foundations” [2]. He responded by listing foundational endeavors, or foundational activities, investigations about axiomatic set theory which might well interest working mathematicians. If set theory is a common language for mathematics, rather than a “foundation” in any metaphorical or ontological sense, its properties and limitations are still relevant to mathematicians in general, not only to “foundational” specialists.

2. Lived experience as “foundation”

If we do want to talk about “foundations of mathematics”, we have to recognize that there can be many different kinds of foundations. It has become automatic to interpret “foundations” as “logical” or “axiomatic” foundations. This notion of foundations is based on seeing mathematics as a collection of statements or formulas, a library of inscriptions. The foundation then is the inscription from which all the other inscriptions can be derived. But it’s equally valid to regard mathematics as a historical process, part of the intellectual and cultural history of humanity. Then one could ask for historical foundations. It is equally legitimate to see mathematics as embedded in society, as a part of the socio-economic-political life of our times. Then one could ask for the socio-economic-political foundations of mathematics. And certainly one can think of mathematics as an activity of the individual mind/brain, a function of the nervous system. Then one could seek the psychological/neurological foundations of mathematics. In fact, all three kinds of activity are going on today.

I have become interested in still a different kind of foundation, what is sometimes called “phenomenological foundation.” Here “phenomenological” refers to the ideas of Edmund Husserl and Karl Heidegger. The famous combinatorialist Gian-Carlo Rota was a deep thinker and expositor of Husserl and Heidegger [7]. In [6] there are four chapters describing aspects of mathematics from the viewpoint of phenomenology.

Husserl [5] was concerned about the loss of contact with lived experience in the prevalent reductionist materialistic way of thinking about the world. Beginning with Galileo’s denial of reality to any sensation not subject to measurement or numeration, and Descartes’ radical separation between Mind and Matter, science made great advances by focussing on the mathematically measurable aspects of reality. By the 20th century, this denial and separation led to a common assumption that what’s real is just—atoms and molecules. Our perceptions of the world, our actual moment-to-moment interaction with the world, came to be disregarded, or thought of as a secondary after-effect to the reality—interactions of atoms and molecules.

Husserl worked hard to bring us back, to see that our perceptions, our moment-to-moment interaction with the world, is our basic reality, from which our scientific theories are ultimately derived, and on which they ultimately depend. His project was a phenomenological description of reality, faithfully reporting without preconceptions how we perceive and what we perceive and what it is, “to perceive.”

Maurice Merleau-Ponty [6] followed Husserl in this project, and went further, showing that in our perception of the world, of colors and shapes and motions of objects and creatures, there is a mutual interaction. The perceived “object” is
actively presenting itself, almost forcing itself on us, as we actively reach out with all our senses to grasp it.

What do Husserl and Merleau-Ponty have to do with mathematics? I think it is possible to take, as the “foundation of mathematics”, the lived experience of the active mathematician—which, indeed, so many have already described, in poetic or metaphorical language. That experience is an interaction between perceiver and perceived. Indeed, that interaction should be regarded as coming first, making possible the distinction between the perceiver and the perceived (the mind of the mathematician on one hand, the mathematical object or entity on the other hand). The act or experience of mathematical perception, like that of visual or auditory perception, is an interaction between two interpenetrating partners—the perceiving mathematician and the perceived mathematical object or entity.

Many people—Hardy, Cayley, Sylvester among others—have described mathematical work as actually “seeing” a landscape, a perceived world, making direct analogies to perception of physical objects. In an attempt at a phenomenological description of a mathematical episode, I used the simple example of Heron’s formula presented in the last two articles, where an obscure, mysterious formula was turned into a clear, understandable geometric fact.

(I learned later on that similar thoughts have been expressed earlier by others.)

In this little bit of mathematical work, the properties of algebraic expressions and the properties of triangles are already “there”, forcing themselves on my attention. Yet algebraic expressions and Euclidean triangles are not “real world” objects. They are merely “ideal objects”—shared cultural practices. But when I try to make them fit together, I must grapple with them. They are resistant, almost intractable, they insist on having their own way. I succeed only if what I want them to do fits their natures, if it’s in accord with how they work.

I found this out to my chagrin when I tried to carry Heron’s formula up to three dimensions. The solid analogue of a triangle is a tetrahedron (a triangular pyramid). Given a triangular base, choose a point above it. Then connect that point, with a new edge, to each vertex of the base triangle. There’s your tetrahedron! The volume in three dimensions is analogous to the area in two, so there “should” be a formula for the volume in terms of the edges, analogous to the formula for the area of a triangle in terms of the edges. There is an elementary formula for the volume of a tetrahedron: “one third the area of the base times the altitude.” (The “altitude” is the vertical height of the new vertex above the base.) This is the three-dimensional analogue of the familiar elementary formula for the area of a triangle—one half the base times the altitude. Now, just as Heron gives the area of a triangle as a symmetric quadratic function of the three sides, $a, b, c$, so there should be an analogous formula for the volume of the pyramid: a symmetric cubic function of the six sides, $a, b, c, d, e, f$.

I wasted many hours hunting for the right symmetric function of six variables. (I expected it to be, by analogy, the square root of a sixth degree polynomial, since volume depends on the scale of length as the third power (cube).)

I failed! The symbols $a, b, c, d, e, f$ could not be tortured to fit my preconception of what they should do. The symbols and the tetrahedron are obstinate, recalcitrant. Rather than forcing them to my will, I had to bow to “reality.” Finally, I saw that my goal was impossible. Once I thought to look for it, it was easy to come up with an example of two different tetrahedra, whose side lengths are the same,
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but whose volumes are different (See details below.) The volume of a tetrahedron is determined not only by the lengths of the six sides, but also by how the sides are connected! There is indeed a formula for the volume in terms of the six edges. But its symmetry properties are more complex, more subtle than what I had in mind. I had a chance to find it only after I adjusted my expectations to accord with reality—mathematical reality.

I submit this simple story as a prototype of mathematical creation and discovery—of mathematical research. Of course, it is a caricature as well as a prototype. “Serious” research involves generality and abstraction far beyond plane geometry and elementary algebra. Nevertheless, it is prototypical in the essential feature—the salience of the mathematical entities themselves. The mathematician struggles to understand and control them, but they have their own stubborn integrity, which must be respected, in order for the discovery/creation venture to succeed. The researcher struggles to penetrate to the inner nature of some mathematical situation he only partly understands. He has to interact with it, struggle with it, come to understand more deeply what he can do with it. The analogy to grappling with physical objects—like a sculptor learning to understand the clay, the metal or the stone—is inescapable.

[To construct two tetrahedra with different volumes but the same set of edges, let three edges all have lengths 1, and three others all have lengths square root of 2. For the first tetrahedron, construct the base triangle of the shorter lengths, 1, 1, 1, and use the three longer sides as the slant edges rising to the vertex. With a little trigonometry, its volume can be calculated: square root of 5 divided by 12. For the second tetrahedron, use the longer three sides for the base triangle, and the shorter three sides for the slant edges. It is a rectangular tetrahedron with volume 1/6.]

Bibliography

There is an old conundrum, many times resurrected: why do mathematics and physics fit together so surprisingly well? There is a famous article by Eugene Wigner, or at least an article with a famous title: “The Unreasonable Effectiveness of Mathematics in Natural Sciences.” After all, pure mathematics, as we all know, is created by fanatics sitting at their desks or scribbling on their blackboards. These wild men go where they please, led only by some notion of ‘beauty’, ‘elegance’, or ‘depth’, which nobody can really explain. Wigner wrote, “It is difficult to avoid the impression that a miracle confronts us here, quite comparable in its striking nature to the miracle that the human can string a thousand arguments together without getting itself into contradictions, or to the two miracles of the existence of laws of nature and of the human mind’s capacity to divine them.”

In such examples as Lobachevsky’s non-Euclidean geometry, or Cayley’s matrix theory, or Galois’ and Jordan’s group theory, or the algebraic topology of the mid-twentieth century, pure mathematics seemed to have left far behind any physical interpretation or utility. And yet, in the cases mentioned here, and many others, physicists later found in these ‘useless’ mathematical abstractions just the tools they needed.

Freeman Dyson writes, in his Foreword to Monastyrsky’s *Riemann, Topology, and Physics*, of “one of the central themes of science, the mysterious power of mathematical concepts to prepare the ground for physical discoveries which could not have been foreseen or even imagined by the mathematicians who gave the concepts birth.”

On page 135 of that book, there is a quote from C. Yang, co-author of the Yang-Mills equation of nuclear physics, speaking in 1979 at a symposium dedicated to the famous geometer, S. Chern.

“Around 1968 I realised that gauge fields, non-Abelian as well as Abelian ones, can be formulated in terms of nonintegrable phase factors, i.e., path-dependent group elements. I asked my colleague Jim Simons about the mathematical meaning of these nonintegrable phase factors, and he told me they are related to connections with fibre bundles. But I did not then appreciate that the fibre bundle was a deep mathematical concept. In 1975 I invited Jim Simons to give to the theoretical physicists at Stony Brook a series of lectures on differential forms and fibre bundles. I am grateful to him that he accepted the invitation and I was among the beneficiaries. Through these lectures T. T. Wu and I finally understood the concept of nontrivial bundles and the Chern-Weil theorem, and realized how beautiful and general the theorem is. We were thrilled to appreciate that the nontrivial bundle was exactly the concept with which to remove, in monopole theory, the string difficulty which had been bothersome for over forty years [that is, singular threads emanating from a Dirac monopole].
“When I met Chern, I told him that I finally understood the beauty of the theory of fibre bundles and the elegant Chern-Weil theorem. I was struck that gauge fields, in particular, connections on fibre bundles, were studied by mathematicians without any appeal to physical realities. I added that it is mysterious and incomprehensible how you mathematicians would think this up out of nothing. To this Chern immediately objected. “No, no, this concept is not invented—it is natural and real.”

Why does this happen?

Is there some arcane psychological principle by which the most original and creative mathematicians find interesting or attractive just those directions in which Nature herself wants to go? Such an answer might be merely explaining one mystery by means of a deeper mystery.

On the other hand, perhaps the “miracle” is an illusion. Perhaps for every bit of abstract purity that finds physical application, there are a dozen others that find no such application, but instead eventually die, disappear and are forgotten. This second explanation could even be checked out, by a doctoral candidate in the history of mathematics. I have not checked it myself. My gut feeling is that it is false. It seems somehow that most of the mainstream research in pure mathematics does eventually connect up with physical applications.

Here is a third explanation, a more philosophical one that relies on the very nature of mathematics and physics. Mathematics evolved from two sources, the study of numbers and the study of shape, or more briefly, from arithmetic and visual geometry. These two sources arose by abstraction or observation from the physical world. Since its origin is physical reality, mathematics can never escape from its inner identity with physical reality. Every so often, this inner identity pops out spectacularly when, for example, the geometry of fiber bundles is identified as the mathematics of the gauge field theory of elementary particle physics. This third explanation has a satisfying feeling of philosophical depth. It recalls Leibnitz’s “windowless monads”, the body and soul, which at the dawn of time God set forever in tune with each other. But this explanation, too, is not quite convincing. For it implies that all mathematical growth is predetermined, inevitable. Alas, we know that is not so. Not all mathematics enters the world with that stamp of inevitability. There is also “bad” mathematics, that is, pointless, ugly, or trivial. This sad fact forces us to admit that in the evolution of mathematics there is an element of human choice, or taste if you prefer. Thereby we return to the mystery we started with. What enables certain humans to choose better than they have any way of knowing?

A good rule in mathematical heuristics is to look at the extreme cases – when a small parameter becomes zero, or a large parameter becomes infinite. Here, we are studying the way that discoveries in “pure” mathematics sometimes turn out to have important, unexpected uses in science (especially physics). I would like to use the same heuristic – “look at the extreme cases”. But in our present discussion, what does that mean, “extreme case”? Of course, we could give this expression many different meanings. I propose to mean “extremely simple”. To start with, let’s take counting, that is to say, the natural numbers.

These numbers were, of course, a discovery in mathematics. It was a discovery that became very important, indeed essential, in many parts of physics and other sciences. For instance, one counts the clicks of a Geiger counter. One counts the
number of white cells under a microscope. Yet the original discovery or invention of counting was not intended for use in science; indeed, there was no “science” at that early date of human culture.

So let us take this possibly childish example, and ask the same question we might ask about a fancier, more modern example. What explains this luck or accident, that a discovery in “pure mathematics” turns out to be good for physics?

Whether we count and find the planets seven, or whether we study the \( n \)-body problem, where \( n \) is some positive integer, we certainly do need and use counting - the natural numbers - in physics and every other science.

This remark seems trivial. Such is to be expected in the extreme cases. We do not usually think of arithmetic as a special method or theory, like tensors, or groups, or calculus. Arithmetic is the all-pervasive rock bottom essence of mathematics. Of course it is essential in science; it is essential in everything. There is no way to deny the obvious fact that arithmetic was invented without any special regard for science, including physics; and that it turned out (unexpectedly) to be needed by every physicist.

We are therefore led again to our central question, “How could this happen”? How could a mathematical invention turn out, unintentionally, after the fact, to be part of physics? In this instance, however, of the counting numbers, our question seems rather lame. It is not really surprising or unexpected that the natural numbers are essential in physics or in any other science or non-science. Indeed, it seems self-evident that they are essential everywhere. Even though in their development or invention, one could not have foreseen all their important uses.

So to speak, when one can count sheep or cattle or clam shells, one can also count (eventually) clicks of a Geiger counter or white cells under a microscope. Counting is counting. So in our first simple example, there really is no question, ‘How could this happen?’ Its very simplicity makes it seem obvious how ‘counting in general’ would become, automatically and effortlessly, ‘counting in science’.

Now let’s take the next step. The next simplest thing after counting is circles. Certainly it will be agreed that the circle is sometimes useful. The Greeks praised it as ‘the heavenly curve’. According to Otto Neugebauer, “Philosophical minds considered the departure from strictly uniform circular motion the most serious objection against the Ptolemaic system and invented extremely complicated combinations of circular motions in order to rescue the axiom of the primeval simplicity of a spherical universe” (The Exact Sciences in Antiquity). I. B. Cohen wrote, “The natural motion of a body composed of ether is circular, so that the observed circular motion of the heavenly bodies is their natural motion, according to their nature, just as motion upward or downward in a straight line is like natural motion for a terrestrial object” (The Birth of a New Physics).

And here is a more detailed account of the circle in Greek astronomy: ‘Aristotle’s system, which was based upon earlier works by Eudoxus of Cnidus and Callippus, consisted of 55 concentric celestial spheres which rotated around the earth’s axis running through the center of the universe. In the mathematical system of Callippus, on which Aristotle directly founded his cosmology of concentric spheres, the planet Saturn, for example, was assigned a total of four spheres, to account for its motion ‘one for the daily motion, one for the proper motion along the zodiac or ecliptic, and two for its observed retrograde motions along the zodiac’ (E. Grant, Physical Science in the Middle Ages).
In recent centuries, a few other plane curves have become familiar. But the
circle still holds a special place. It is the ‘simplest’, the starting point in the study
of more general curves. Circular motion has special interest in dynamics. The
usual way to specify a neighborhood of a given point is by a circle with that point
as center.

So we see that the knowledge of circles which we inherited from the Greeks
(with a few additions) is useful in many activities today, including physics and the
sciences. I suppose this is one reason why 10th grade students are required to study
Euclidean geometry.

Again, we return to the same question. How can we explain this “miracle”?
Few people today would claim that circles exist in nature. We know that any
seemingly circular motion turns out on closer inspection to be only approximately
circular.

Not only that. We know that the notion of a circle is not absolute. If we define
distance otherwise, we get other curves. To the Euclidean circle we must add non-
Euclidean ‘circles’. If the Euclidean circle retains a central position, it does so
because we choose—for reasons of simplicity, economy, convenience, tradition—to
give it that position.

We see, then, two different ways in which a mathematical notion can enter into
science. We can put it there, as Ptolemy put circles into the planetary motion. Or
we can find it there, as we find discreteness in some aspect or other of every natural
phenomenon.

Let’s take one last example, a step up the ladder from the circle. I mean the
conic sections, especially the ellipse. These curves were studied by Apollonius of
Perga (262-200 B.C.) as the “sections” (or “cross sections” as we would say) of a
right circular cone. If you cut the cone with a cutting plane parallel to an element
of the cone, you get a parabola. If you tilt the cutting plane toward the direction
of the axis, you get a hyperbola. If you tilt it the other way, against the direction
of the axis, you get an ellipse.

This is “pure mathematics”, in the sense that it has no contact with science or
technology. Today we might find it somewhat impure, since it is based on a visual
model, not on a set of axioms.

The interesting thing is that nearly 2,000 years later, Kepler announced that
the planetary orbits are ellipses. (There also may be hyperbolic orbits, if you look
at the comets.)

Is this a miracle? How did it happen that the very curves Kepler needed to
describe the solar system were the ones invented by Apollonius some 1800 or 1900
years earlier?

Again, we have to make the same remarks we did about circles. Ellipses are
only approximations to the real orbits. Engineers using earth satellites nowadays
need a much more accurate description of the orbit than an ellipse. True, Newton
proved that “the orbit” is exactly an ellipse. And today we reprove it in our
calculus classes. In order to do that, we assume that the earth is a point mass
(or equivalently, a homogeneous sphere). But you know and I know (and Newton
knew) that it is not.

Kepler brought in Apollonius’s ellipse because it was a good approximation
to his astronomical data. Newton brought in Apollonius’s ellipse because it was
the orbit predicted by his gravitational theory (assuming the planets are point
masses, and that the interactive attraction of the planets is “negligible”). Newton used Kepler’s (and Apollonius’s) ellipses in order to justify his gravitational theory. But what if Apollonius had never lived? Or what if his eight books had been burned by some fanatic a thousand years before? Would Newton have been able to complete his work?

We can imagine three different scenarios: (1) Kepler and Newton might have been defeated, unable to progress; (2) they might have gone ahead by creating conic sections anew, on their own; (3) they might have found some different way to study the dynamics of the planets, doing it without ellipses.

Scenario three is almost inconceivable. Anyone who has looked at the Newtonian theory will see that the elliptic trajectory is unavoidable. Without Apollonius, one might not know that this curve could be obtained by cutting a cone. But that fact is quite unnecessary for the planetary theory. And surely somebody would have noticed the connection with cones (probably Newton himself).

Scenario one, that Newton would have been stuck if not for Apollonius, is quite inconceivable. He, like other mathematical physicists since his time, would have used what was available and created what he needed to create. While Apollonius’ forestalling Kepler and Newton is remarkable and impressive, from the viewpoint of Newton’s mechanics, it is inessential. In the sequence of events that led to the Newtonian theory, what mattered were the accumulation of observations by Brahe, the analysis of data by Kepler, and the development by Barrow and others of the “infinitesimal calculus”. The theory of the conic sections, to the extent that he needed it, could have been created by Newton himself. In other words, scenario two is the only believable one.

If a mathematical notion finds repeated use, in many branches of science, then such repeated use may testify to the universality, the ubiquitousness, of a certain physical property - as discreteness, in our first example. On the other hand, the use of such a mathematics may only be witness to our preference for a certain picture or model of the world, or to a mental tradition which we find comfortable and familiar. And also, perhaps, to the amiability or generosity of nature, which allows us to describe her in the manner we choose, without being “too far” from the truth.

What then of the real examples - matrices, groups, tensors, fiber bundles, connections. Maybe they mirror or describe physical reality – “by accident”, so to speak, since the physical application could not have been foreseen by the inventors.

On the other hand, maybe they are used as a matter of mere convenience - we understand it because we invented it, and it works “well enough”.

Maybe we are not even able to choose between these two alternatives. To do so would require knowledge of how nature “really” is, and all we can ever have are data and measurements, theorems and hypotheses in which we put more or less credence.

And it may also be deceptive to pose the two alternatives - true to nature, like the integers, or an imposed model, like the circle. Any useful theory must be both. Understandable - i.e., part of our known mathematics, either initially or ultimately - and also “reasonably” true to the facts, the data. So both aspects - man-made and also faithful to reality - must be present.

These self-critical remarks do not make any simplification in our problem.
The problem is, to state it for the last time, how is it that mathematical inventions made with no regard for scientific application turn out so often to be useful in science?

We have two alternative explanations, suggested by our two primitive examples, counting and circles. Example one, counting, leads to explanation one: That certain fundamental features of nature are found in many different parts of physics or science; that a mathematical structure which faithfully captures such a fundamental feature of nature will necessarily turn out to be applicable in science.

According to explanation two (of which the circle was our simple example), there are several, even many different ways to describe or “model” mathematically any particular physical phenomenon. The choice of a mathematical model may be based more on tradition, taste, habit, or convenience, than on any necessity imposed by the physical world. The continuing use of such a model (circles, for example) is not compelled by the prevalence of circles in nature, but only by the preference for circles on the part of human beings, scientists in particular.

What conclusion can we make from all this? I offer one. It seems to me that there is not likely to be any universal explanation of all the surprising fits between mathematics and physics. It seems clear that there are at least two possible explanations; in each instance, we must decide which explanation is most convincing. Such an answer, I am afraid, will not satisfy our insistent hankering for a single simple explanation. Perhaps we will have to do without one.

References

Mathematical Practice as a Scientific Problem

Abstract Mathematics is a living organism, a living part of human culture, society and history. Mathematical entities actually exist, they are cultural items. The nature of mathematics requires empirical as well as philosophical investigation. In this connection, we clarify the sense in which mathematical truths can be called “timeless.”

Atiyah’s pleasant surprise

Commenting on a recent anthology [HE2005], Michael Atiyah wrote: “I was pleasantly surprised to find that this book does not treat mathematics as desiccated formal logic, but as a living organism, immediately recognizable to any working mathematician.”

What does it mean to say that mathematics is “a living organism”? It grows, it evolves, it interacts with its environment. It has purpose and intention. It’s created and sustained by and for living human beings—with all the complexity which that fact implies.

Atiyah’s comment of course conveys no disrespect for logic as a branch of mathematics. The “desiccation” refers to the philosophical reduction of all mathematics to “formal logic” (including formal set theory.) That view of mathematics was plainly stated by W.V.O. Quine: “Researches in the foundations of mathematics have made it clear that all of mathematics in the above sense [i.e., all of both pure and applicable mathematics] can be got down to logic and set theory.” [Q 1966].

Forty years later, that opinion still is found in academic philosophy of mathematics. But when we mathematicians wonder about the meaning and nature of our work, we tend to look at our actual experience in creating and discovering the facts of geometry, algebra, topology, or analysis. Our report is then recognizable as mathematics by other mathematicians.

The young English philosopher David Corfield has been urging his colleagues to get interested in mathematics as it is lived and practiced. He writes, “By far the larger part of activity in what goes by the name ‘philosophy of mathematics’ is dead to what mathematicians think and have thought, aside from an unbalanced interest in the ‘foundational’ ideas of the 1890-1930 time...We should be looking to inspire a new generation of philosophers to sign up to the major project of understanding how mathematics works.” [CO]

Does “existence” matter?

Much current conversation among philosophers of mathematics is about “Platonism versus fictionalism”. Do mathematical “things” (objects, entities, items, whatever label you like) really “exist” (whatever that means)? Or are they just “fictions” (whatever that means)?
The trouble is that mathematical items don’t fall into either of René Descartes’
two categories of existence—physical (material, ponderable, tempero-spatial) or men-
tal (subjective, private.) The number 2, for example, is neither a physical object
nor a private thought in the philosopher’s head.

Perhaps out of impatience with the Platonist-fictionalist back-and-forth, some
writers have decided that existence is a matter of no concern ([CE], [Da], [Ma],
[Ro1996]). (After all, how we calculate and prove isn’t affected by philosophical
existence.) And yet, mathematical existence is one of our frequent concerns! (As
in, existence of the sporadic “Monsters” of finite group theory, or existence of a
classical solution of the mixed initial-boundary value problem for the Navier-Stokes
equation.)

I am only a “working mathematician”, not a philosopher, so I say something
“exists” if it affects us, if we need to take it into account in our actions. (This
violates the honorable tradition, older than Plato, in which the ephemera of daily
life are mere illusion, and do not “exist” for philosophy.)

If we take daily life and experience as real, the notion of existence can’t be
restricted only to physical existence—what can be weighed, measured, detected in
the laboratory or the observatory–nor to mental existence–the private consciousness
of the individual philosopher. “What exists” has to include the other things that
daily life is made up of that no one can ignore–the calendar, the schedule, the price
list, the pay roll.

Laws

Customs

Families

Nations

Wars

Special sales for Christmas

Et cetera, et cetera.

All that important stuff is not weighed and measured, nor is it located inside
the philosopher’s mind. You can call it “public”, or “inter-subjective”, or ”cultural-
historic-social”. I call it “social” for short. Of course, the social is grounded in the
physical and mental, in complex, fascinating ways. We must study and try to
understand all that. But first of all, given the slightest degree or measure of sanity,
it exists! I am grateful to Julian Cole [COLE] for taking this argument seriously
enough to give it some respectable philosophical backing.

Once existence is understood to include all this stuff we recognize and deal with
every day, then the big puzzle about mathematical existence fades away. Mathe-
matics exists, neither as a kind of physical entity, nor as a private mental experience,
but socially, historically, culturally, inter-subjectively, publicly. The classical article
[WH] by the famous anthropologist Leslie White should have established this simple
fact once and for all [COLE]. The project we should be engaged in is, from this sim-
ple observation, to develop the important properties and qualities of mathematical
entities, objects, processes.

Here is a possible objection. “Sure, the speed limit and the price of gasoline
do affect me, but how do the facts of mathematics directly affect anyone?” One
standard answer is, “Mathematics is tied, directly and indirectly, to physics, which
is embodied in the objects and processes you use every day in modern consumer
society.” A second answer is, “Your checks will bounce if you ignore the laws of
arithmetic.” A third answer is: “Mathematicians, once they enter the world of mathematics, find that they cannot do whatever they please. They must accommodate to mathematical reality.” (Thus it was that Andrew Wiles was stuck in his attic every day for seven years.)

The trouble with Platonism is not so much that it’s wrong. The trouble is that it’s an easy answer that gets in the way of looking for scientific answers. When Newton and Leibniz believed that mathematical truths are thoughts in the mind of God, they did not need to trouble further about the nature of mathematical truth. A kind of Platonic faith still is natural to us today, in the moments when we’re hot in pursuit of our research problem. But when we step back and look at ourselves and each other, we can recognize another fascinating problem: to understand mathematics as a special aspect of human thought and culture.

For a multi-disciplined study of mathematical practice

Once it is accepted that socio-cultural-historical entities are real objects with objective properties, the Platonist-fictionalist (formerly logicist-formalist) confusion dies away. Focus turns away from the time-honored philosophical disputation on the nature of mathematics, and toward an empirical or scientific question, toward a real phenomenon to study, to try ultimately to understand. The situation is analogous to what happened when anthropology, psychology or linguistics was recognized as an autonomous discipline, separating off from the “What is Man?” “What is Mind?” “What is Language?” wonderments. Mathematics—both mathematical practice and mathematical concepts—can be studied with every available method, as a special form of cultural life.

Of course the scientific study of special forms of cultural life is nothing new. Take economics, for instance. It started independent life as a priori rules of behavior of a hypothetized “economic man.” But later we saw the rise of “behavioral” or “empirical” economics. A similar story can be told about linguistics. The young field of “sociolinguistics” is empirical; it may some day bring mathematical linguistics down to the ground of actual speech data, of behavior of language speakers.

If economics is the study of economic behavior and linguistics is the study of language behavior, then we may be ready to start a systematic study of mathematical behavior. Our first step, naturally, is to ask:

What do we mean by “mathematical behavior”?

Certainly it includes thinking, wondering, dreaming, learning about mathematics. Certainly it includes problem solving at all levels, from pre-kindergarten up through postdocs and Fields Prize winners. Teaching mathematics, at all levels, is also mathematical behavior. (If it isn’t, then we’d call it bad teaching.) Ordinary commercial calculations too. Routine plugging numbers into formulas by engineers and technicians is another form of mathematical behavior. And geometrical reasoning, and probabilistic reasoning, and combinatorial reasoning, and any formal logical reasoning. In fact, we must expect that other, hitherto unthought-of kinds of mathematical behavior will yet arise. If that should happen, how would we identify such hitherto unseen behavior as mathematical?

One answer was given in 1981, in _The Mathematical Experience_: “While mathematics is a humanistic study with respect to its subject matter, it is like the sciences in its objectivity. Those results about the physical world that are reproducible—that come out the same way every time anyone asks—are called natural sciences. In the
realm of ideas, of mental objects, those ideas whose properties are reproducible, that come out the same way every time anyone asks, are called mathematical objects, and the study of mental objects with reproducible properties is called mathematics.” I was gratified when David Mumford quoted this approvingly. “I love this definition because it doesn’t try to limit mathematics to what has been called mathematics in the past but really attempts to say why certain communications are classified as math, others as science, others as art, others as gossip. Thus reproducible properties of the physical world are science whereas reproducible mental objects are math.” [MU]. Reasoning about mental objects (concepts, ideas) that compels assent (on the part of everyone who understands the concepts involved) is “mathematical”. This is what is meant by “mathematical certainty”. It does not imply infallibility! (On the contrary, history shows that the concepts about which we reason with such conviction have sometimes surprised us on closer acquaintance, and forced us to re-examine and improve our reasoning.)

History shows how new modes of reasoning come to be recognized as mathematical. Two famous examples are set theory and probability. Infinite sets were not part of mathematics before Georg Cantor explicitly based them on the notion of one-to-one correspondence. On that basis, he was able to make compelling arguments, and set theory (with some resistance) became a mathematical subject. An older example is gambling or betting. Fermat and Pascal demonstrated “rigorous” (irrefutable, compelling) conclusions about some games of chance. Therefore their work was mathematical, even though it was outside the limits of mathematics as previously understood. The subsequent work of Bernoulli, De Moivre, Laplace and Chebychev was certainly mathematics, for the same reason. Ultimately Kolmogorov axiomatized probability in the context of abstract measure theory. In doing so he was axiomatizing an already existing, ancient branch of mathematics.

Donald MacKenzie [MA] uses the term “rigorous proof” in contrast to “formal proof,” to mean “all those arguments that are accepted by mathematicians (or other relevant specialists) as constituting mathematical proofs, but that are not formal proofs.” I could amend him, and say “rigorous proof” is any argument that compels assent from everyone who understands the concepts involved. Then my definition of mathematics above could be shortened: any set of ideas is mathematical, to the extent that it is subject to rigorous proof.

Lakoff and Nunez have shown that mathematical proof often can be understood as based on “embodied metaphors.” Of course, that explanation of proof cannot be formalized. In fact, mathematical proof is so varied that it cannot be pinned down in a single precise, universal description! We can simply say that any irresistible argument about a socio-culturally shared concept, one that forces conviction, is a piece of mathematics. Any discourse that carries such conviction is mathematical discourse, a piece of mathematics.

Wouldn’t this include chess problems? Yes, of course. “A chess problem is simply an exercise in pure mathematics...Chess problems are the hymn-tunes of mathematics.” [H].

The basic problem

The basic problem, then, is: How is it possible for people to create reasoning which is indisputable?
Empirical studies of mathematics are already taking place, in considerable variety. The wonderful book *The Number Sense*, by Stanislas Dehaene, reports on neurological, linguistic and educational studies, all pointing to a bodily (neurological or biological) foundation for arithmetic. (See also [HMC].)

*Where Mathematics Comes From* by Lakoff and Nunez and *The Math Gene* by Devlin are path-breaking attempts to connect mathematical thinking with the language ability. Anthropologists have long studied mathematical understanding and language in many different cultures. Recently under the label of “ethnomathematics,” they have been joined by mathematicians Ubiratan D’Ambrosio and Marcia Ascher. Among sociologists, the writings of David Bloor in Edinburgh stirred up considerable resistance and controversy. The sociologists Andrew Pickering and Donald MacKenzie and the cognitive scientist Rafael Nunez contributed to my recent collection, *18 Unconventional Essays on the Nature of Mathematics*. The long-standing work of developmental and educational psychologists, especially Jean Piaget and Lev Vygotsky and their followers, has yielded a great deal of knowledge on how children learn mathematics. The oldest specialty in “mathematics studies” is of course the history of mathematics. Traditional history concentrated on describing mathematical findings and results as embedded or embalmed in print, but today many historians are broadening their focus, to see mathematics as the product of individuals in communities—the professional community of fellow-mathematicians, and also the larger political-economic-ideological community, whose support makes them possible. The path-breaking work of Raymond Wilder 25 years ago should be re-read [WI].

It would be redundant to try to summarize the work of these different authors. My purpose is to call attention to a major intellectual challenge, which will take decades to unfold: to use the methodologies of history, sociology, anthropology, psychology, cognitive and neuroscience, and no doubt still others yet to be invented, to develop a coherent, empirically-based, overall understanding of the nature of mathematical practice and knowledge, as a major part of our larger understanding of what it is to be human.

This is not merely saying “We need a sociology of mathematics.” Rather, it is saying that mathematics is a near-universal, almost all-pervasive aspect of humanity, of being human, and should be studied from all points of views, by all available methods—psychological, neurological, linguistic, sociological, anthropological, historical, philosophical. Not as distinct, disconnected academic departments, but as related ways of focusing on the same mysterious phenomenon.

Here are some questions to which we do not have adequate answers:

The dichotomy between discrete and continuous, between arithmetic and geometry, between logical and visual, is a fundamental pervading theme throughout mathematics. It was manifested to the Pythagoreans, when they discovered that no fraction can measure the diagonal of the unit square. It manifested again and again, in the acceptance and rejection of infinitesimals, from Archimedes, Newton, Leibniz, all the way to Abraham Robinson. Again it surfaced in André Weil’s conjectures linking the number of solutions of Diophantine equations to the cohomology of certain differentiable manifolds.

If we trace back to our animal origins, it is clear that both human and prehuman hunters and gatherers had to be capable of thinking about direction and distance, and that this kind of thinking is intimately connected with seeing, with
the visual function of the brain. It is equally clear that language is primarily au-
ditory, and that words and language, first spoken, later written, are the soil from
which spring logic and counting–number and arithmetic. Are there two different
brain activities or potentialities, one for visual-geometric and another for logical-
arithmetical thinking, associated with the two different brain centers–visual and
auditory? Is there then a neurological basis for these two opposed worlds of math-
ematical thought–at once incompatible and inseparable?

Another fundamental dichotomy is between “existence as construction” and
“existence as logical possibility.” How are we able to think and draw conclusions
about things that we do not know how to find? This has played out as the dispute
of intuitionism and constructivism (Brouwer, Bishop) against mainstream math-
easics, whether formalist or Platonist (Hilbert, Godel.)

Unconscious mathematizing is a huge mystery. Mathematical ideas can come
into consciousness by surprise, as if by a gift from nowhere–evidently from some
subconscious process. This is attested by many anecdotes; the most famous is
Poincaré’s discovery of the “theta-fuchsian” functions. A mystery calling for expla-
nation.

The disconnect between verbal and mathematical abilities is another unex-
plained common observation. So is the prominent but mysterious correlation be-
tween ability in mathematics and ability to perform music.

After mathematics is recognized as a scientific problem, philosophical problems
will remain. Just as some philosophers of mind ignore the work of psychologists, and
some philosophers of language ignore the findings of linguists, some philosophers of
mathematics will ignore empirical investigators, as outsiders at the tea party.

**Timely or timeless?**

One perennial puzzle about mathematics is its timeliness or timelessness. People
say mathematical truths are timeless, even eternal—“Always were true, always
will be.” The squares on the sides of a right triangle add up to the square on
the hypotenuse–presumably they “did so” before the notion of right triangle or
hypotenuse had crossed anyone’s head. This way of thinking seems to force one
to allow the “existence” of all right triangles with their hypotenuses, even before
the famous Big Bang that gave birth to our Cosmos. And of course, the same
thinking applies to all complex determinants, say, of order 29,146,298,979, or to all
Grothendieck toposes. They all existed, “somewhere, somehow.”

I would like to suggest a different way of looking at these matters. Once a
new, well-founded mathematical question is asked, the answer usually is already
determined, but still unknown. In that sense, the answer now exists. But the
answer didn’t exist in advance of the question. Once people conceived of right
triangles and of the area of a square, the question about the squares on the sides
of the triangle was meaningful, and we would say that the Pythagorean theorem
was true, even before it was stated. But before there were triangles and sums of
squares, there was no object to which the Pythagorean theorem could refer, so it
is senseless to say it was true already at that time.

Similarly, theorems on faithful functors or Abelian sheaves were neither true
nor false before the mathematical concepts they describe had been conceived or
formulated. They simply didn’t refer, they had no content. Once those objects came
into being, as mathematical objects, that is as thoughts and objects of discourse,
then we could say that certain theorems were already true—waiting to be discovered, so to speak.

In this respect mathematical facts are different from physical ones. The Earth rotated on its axis before there were people to care about night and day. But mathematical concepts or entities or objects, whichever you prefer to call them, are called into being by our questions. Only then can answers to such questions be true or false.

It is tempting for mathematicians and philosophers of mathematics to look for mathematical precision in thinking about mathematics. But it will not do to expect a definite yes-or-no, either-or answer to such questions as exactly when some mathematical fact became true.

Mathematics is not a fixed, static, eternal piece of abstract hardware. It is an evolving, growing, developing world of ideas, problems, algorithms, conjectures, proofs, analogies—a cultural world, a world existing first of all in the thoughts of people, and in the traces in their brains that correspond to these thoughts, and only subsequently in the records on paper and on microchips where they have recorded their thoughts.

A new mathematical concept may arise in the course of a mathematical conversation, or in the course of an individual’s thinking or writing about a mathematical topic. In its first state of coming into being, it is transient, evanescent, subject to quick disappearance and oblivion. It may be held onto, communicated to others, developed, it may become a topic of conversation among several people. If it is written down and preserved somehow, or if it becomes a widespread topic of conversation over a considerable period of time, we would recognize it as a stable part of the cultural world of mathematics, it would be recognized as a new topic or concept in mathematics. But there would be no single moment when it was created or invented.

If it is the predetermined solution of a definitely stated problem, there is a sense in which it existed latently, *in potentio*, from the time that problem was stated. And if that problem was a natural consequence, predictable as part of an already existing theory, we could say that the object in question had a potential existence, even before the explicit statement of the problem to which it is a solution. This is not a particularly mysterious situation. In exactly the same sense, one could say that a leaf on an oak tree was potentially present in the DNA of the acorn from which the tree grew. The creation of novelty in mathematics, as in life, is precisely the passage from potentiality to actuality.

The intermediate region between the potential and the actual is the area of active growth at any particular time. It is the social counterpart of what the psychologist Lev Vygotsky called “the zone of proximal development.” Colloquially, we talk about new ideas being “in the air.” We call on this notion to account for a very common event: multiple near-simultaneous inventions or discoveries.

On the other hand, when we are doing mathematics rather than reflecting on ourselves as doers of mathematics, we set aside, disregard the temporality of a mathematical object (or “item” or “entity,” if you like). For the purpose of doing mathematics, its temporality is irrelevant, so we set it aside, put it out of consideration. It is in that sense, and for that reason, that mathematics can be said to be timeless. We make it so, because its temporality is not to our purpose most of the time.
An analogy can be made to watching a movie. When we watch a movie in a movie theater, we are looking at a flat screen on which are projected patterns of light and dark. In order to watch it as a movie (that’s our purpose in being there) we intentionally put aside our awareness that all we are seeing is patterns of light and dark on a flat screen. That “putting aside” is what enables us to “see” the story, the movie.

Putting aside the temporality and concrete historicity of mathematical objects is necessary for us to enter the mathematical dream world and live there, make discoveries and creations there. On the other hand, when we are not doing mathematics, but talking about what mathematics is, we can look at it from the outside and see that it is a temporal part of human culture. (Just as movie-goers have no trouble, before or after the movie, in seeing the blank screen where the images will be projected.)

This may be what some people mean by calling mathematics “a fiction”. Call it “a fiction” if you want—but a “necessary fiction”, not an arbitrary fiction. A “fiction” with laws of its own that must be obeyed, if it is to be entered and lived in successfully. Because it does have its own laws, which compel us to obey, I call it a reality.

**Conclusion**

The existence of mathematical items or entities does matter. Their seeming timelessness is an artifact of our practice, a necessary way of framing them so that we can “enter into” their world. The study of the nature of mathematics and mathematical practice is too big for philosophy alone, or even for half a dozen separate, isolated academic specialties. It is a central problem in the ongoing study of Man by Mankind.

**Educational implications**

The humanistic viewpoint on the nature of mathematics has important implications for education. Mathematics educators easily accept it, they often seem to take it for granted. A forthcoming article [UH] develops some of the interactions between the philosophical and the educational issues. A better understanding of the nature of mathematical activity, based on empirical science as well as on educational practice and experience, would have a tremendously beneficial effect on mathematics education. So there is a pragmatic motivation, alongside the intellectual motivation, for such a program.

**References**

MATHEMATICAL PRACTICE AS A SCIENTIFIC PROBLEM

[COLE] J. Cole, in the present volume; also, “Practice-Dependent Realism and Mathematics,” Dissertation, Ohio State University, 2005
[UH] K. Umland and R. Hersh, “Mathematical Discourse: the link from pre-mathematical to fully mathematical thinking,” to appear in a special issue of Educational Philosophy and Theory.
Proving is Convincing and Explaining

Abstract. In mathematical research, the purpose of proof is to convince. The test of whether something is a proof is whether it convinces qualified judges. In the classroom, on the other hand, the purpose of proof is to explain. Enlightened use of proofs in the mathematics classroom aims to stimulate the students’ understanding, not to meet abstract standards of “rigor” or “honesty.”

I. What is proof?

This is one question we mathematics teachers and students would normally never think of asking. We’ve seen proofs, we’ve done proofs. Proof is what we’ve been watching and doing for 10 or 20 years!

In the Mathematical Experience (Davis and Hersh, 1981, pp. 39-40) this almost unthinkable question is asked of the Ideal Mathematician (the I.M. - the most mathematician-like mathematician) by a student of philosophy: “What is a mathematical proof?”

The I.M. responds with examples – the Fundamental Theorem of This, the Fundamental Theorem of That. But the philosophy student wants a general definition, not just some examples. The I.M. tells the philosophy student about proof as it’s portrayed in formal logic: permutation of logical symbols according to certain formal rules. But the philosophy student has seen proofs in mathematics classes, and none of them fit this description. The Ideal Mathematician crumbles. He confides that formal logic is rarely employed in proving theorems, that the real truth of the matter is that a proof is just a convincing argument, as judged by competent judges.

The philosophy student is appalled by this betrayal of logical standards. The I.M. puts an end to the conversation with these final words: “Everybody knows what a proof is. Read, study, and you’ll catch on. Unless you don’t.”

The Mathematical Experience has been in print for ten years. No philosopher or mathematician has yet taken up the Ideal Mathematician’s challenge: “If not me, then who?” (Meaning: Who has a better right than I to decide what is a proof?)

II. Proof among professional mathematicians

In mathematical practice, in the real life of living mathematicians, proof is convincing argument, as judged by qualified judges. How does this notion of proof differ from proof in the sense of formal logic? Firstly, formal proof can exist only within a formalized theory. Formal proof has to be expressed in a formal vocabulary, founded on a set of formal axioms, reasoned about by formal rules of inference. But

the passage from an informal, intuitive theory to a formalized theory inevitably entails some loss of meaning or change of meaning. The informal by its nature has connotations and alternative interpretations that are not in the formalized theory. Consequently, any result that is proved formally may be challenged: “How faithful are this statement and proof to the informal concepts we are actually interested in?”

Secondly, for many mathematical investigations, full formalization and complete formal proof, even if possible in principle, may be impossible in practice. They may require time, patience, and interest beyond the capacity of any human mathematician. Indeed, they can exceed the capacity of any available or foreseeable computing system.

The attempt to verify formal proofs by computer introduces new sources of error: random errors caused by fluctuations of the physical characteristics of the machine, and inevitable human errors in design and production of software and hardware (logic and programming). For most non-trivial mathematics, the hope of formal proof remains only hypothetical. When, as in the four-color theorem of Appel and Haken (see below), machine computation does execute part of the proof, some mathematicians reject the proof because the details of the machine computation are hidden (inevitably). More than whether a conjecture is correct, mathematicians want to know why it is correct. We want to understand the proof, not just be told it exists. (See Paul Halmos’ complaint below about the Appel-Haken theorem).

Some discussions of proof talk about mathematics only as it’s presented in journals and textbooks. There, proof functions as the last judgment, the final word before a problem is put to bed. But the essential mathematical activity is finding the proof, not checking after the fact that it is indeed a proof. At the stage of creation, proofs are often presented in front of a blackboard, hopefully and tentatively. The detection of an error or omission is welcomed as a step toward improving the proof. Lakatos’ (1976) book is a fascinating presentation of this aspect of proof. At the time of her conversation with the Ideal Mathematician our philosophy student had never witnessed proof at this level. If she had, she might not have been so shocked by the I.M.’s confession. (“Proof is an argument that convinces the experts.”)

All real-life proofs are to some degree informal. A piece of formal argument – a calculation – is meaningful only as part of an informal proof, to complete or to verify some informal reasoning. The formal-logic picture of proof is a fascinating topic for study in logic. It is not a truthful picture of real-life mathematical proof.

Some writers say that informal proofs are convincing if in principle they can be turned into formal proofs. To accept that a formal proof could be written, without seeing it written, is an act of faith. Is such a formalization possible for all the vast collections of accepted theorems in today’s mathematics? Perhaps it is possible. But firm belief in that possibility is an act of faith. (See Davis and Hersh, 1981, pp. 57–73).

III. Three meanings of “proof”

The root meaning of the English word “prove” is:
(A) Test, try out, determine the true state of affairs. (As in Aberdeen Proving Ground, galley proof, “the proof of the pudding,” etc.). It comes from the Latin probare, and is cognate to “probe”, “probation”, “probable”, “probity”.

In mathematics, “proof” has two meanings, one in common practise, the other specialized in mathematical logic and in philosophy of mathematics. The first mathematical meaning, the “working” meaning, is:

(B) An argument that convinces qualified judges.

The second mathematical meaning, the “logic” one, is:

(C) A sequence of transformations of formal sentences, carried out according to the rules of the predicate calculus.

What is the relation between these three meanings, one colloquial and two mathematical?

The logical definition C is intended to be faithful to the everyday meaning B, but more precise. But there has never been and can never be a strict demonstration that definition C really is identical or even similar to what mathematicians do when they prove. This is a universal limitation on any mathematical model of real-world phenomena. As a matter of principle it is impossible to give a mathematical proof that a mathematical model is faithful to reality. All one can do is test the model against experience. Certainly no one has proved that the logic model of proof is incorrect. If a counterexample is ever found, logicians will adapt logic to accommodate it. On the other hand, no one has ever attempted to show that mathematicians’ daily practise is faithfully described by the predicate calculus (first-order logic). To the casual observer, they do not seem similar, as the philosophy student said to the Ideal Mathematician.

Some have said that logic has nothing to do with discovering mathematics, only with verifying it. But no one has shown that the actual practise of verification by mathematicians is faithfully described by the predicate calculus.

Some might say that there is no need for such a demonstration, for if a mathematician doesn’t follow the rules of the predicate calculus, then his/her reasoning is incorrect, and needn’t be considered at all. I take the opposite standpoint: what mathematicians at large sanction and accept is correct. Their work is the touchstone of logic, not vice versa. However that may be, what is really done in day-to-day mathematics has little to do with formal logic.

And what about the root meaning of prove, “to test”? How does that connect to the two mathematical meanings? When a mathematician submits his work to the critical eyes of his colleagues, it is being tested or “proved.” With few exceptions, mathematicians have only one way to test or “prove” their work – invite everybody who is interested to have a shot at it. So the day-to-day mathematical meaning of “proof” agrees with the colloquial meaning. The proof of the pudding is in the eating; the proof of the theorem, in the refereeing.

IV. Variation in proof standards

One well-known difference between pure and applied mathematicians is in how they use proof. It has even been said that some pure mathematicians care more about the rigor of their proofs than about the value of their results. For the applied mathematician, it is certainly the other way around. In applied mathematics, it does happen that a paper is published without complete proofs. Compelling
heuristic evidence, usefulness of the result, plausibility or partial proof of the main results, all can help justify publication.

In pure mathematics, on the other hand, proof is the *sine qua non*. Complete proof, of course.

But even in pure mathematics, funny things have been happening. One example is “chaos,” a thriving field which is pursued mainly by pure mathematicians. The subject of chaos is part of the subject of dynamical systems. Chaos-theorists do prove theorems, but they also use machine computation to discover properties of dynamical systems. Facts discovered in this way may be accepted, even when they are beyond rigorous analysis - that is to say, unproved. Gleick’s (1987) book on chaos was a best-seller.

Finite simple groups is another field where standards of proof are a matter of discussion. A major recent accomplishment has been the complete classification of these groups. Daniel Gorenstein wrote (Davis and Hersh, 1981, pp. 388-389): “The ultimate theorem which will assert the classification of simple groups, when it is attained, will run to well over 5,000 journal pages! ... It seems beyond human capacity to present a closely reasoned several hundred page argument with absolute accuracy... How can one guarantee that the “sieve” has not let slip a configuration which leads to yet another simple group? Unfortunately, there are no guarantees - one must live with this reality.”

A different departure from traditional standards of proof is the work of Miller (1976), Rabin (1976), Schwartz (1980), and Davis (1977). They found a way to say of an integer \( n \) whose primality or compositeness is unknown, “On the basis of available information, the probability that \( n \) is prime is \( p \).” And if \( n \) is really prime, they can make \( p \) arbitrarily close to 1. Of course, the primality of a given \( n \) is not a random variable; \( n \) is either prime or composite. But if \( n \) is very large, determining its primality by a deterministic method can be so laborious that random errors are likely to occur in the computation. Rabin showed that if \( n \) is large enough, the probability of error in the deterministic calculation is greater than the probability \( p \) in his much faster probabilistic method.

V. The four-color theorem

Readers may recall reading about Appel and Haken’s proof of the four-color conjecture a few years ago. They used a computer to carry out parts of the computation beyond the capacity of humans. Not everyone was overjoyed. Paul Halmos (1990) said: “I do not find it easy to say what we learned from all that. We are still far from having a good proof of the Four-Color Theorem. I hope as an article of faith that the computer missed the right concept and the right approach. 100 years from now the map theorem will be, I think, an exercise in a first-year graduate course, provable in a couple of pages by means of the appropriate concepts, which will be completely familiar by then. The present proof relies in effect on an Oracle, and I say down with Oracles! They are not mathematics.” Why did Halmos call the computer an Oracle? Probably because we cannot know in detail all the steps in its calculations. Indeed, we do not even understand in full detail the physical processes by which computers work. Consequently, believing a computer is an act of faith, like believing a fortune teller - albeit a successful, well-reputed fortune teller.
Halmos did not say whether we should regard the four-color theorem as true. He seems to dislike the Appel-Haken proof for two reasons: because it uses an "Oracle," and because, he thinks, we cannot learn anything from it. Halmos’ criteria of judgment are not limited to the criteria of formal logic: completeness, correctness, accuracy. His criteria are also aesthetic and epistemological. This is normal in real-life mathematics, as distinct from formalized mathematics.

Mathematicians call some proofs elegant and beautiful, others awkward or ugly. Their criteria vary by field of mathematics, and of course by individual taste. Some criteria for elegance are: obtaining maximal results with minimal tools; being simultaneously surprising and inevitable; combining concepts that previously seemed unrelated. Hardy (1967) picked familiar theorems on the irrationality of $\sqrt{2}$ and the infinitude of primes as paradigms of beauty. Halmos’ remarks suggest that conceptual proofs are more beautiful than computational ones. Many mathematicians would agree. But to some, collaboration between mathematician and machine is not ugly but beautiful.

Mathematicians prefer a beautiful proof, even if it contains a serious gap, over a dull, boring, correct one. We seem to feel that if an idea is truly beautiful, it will somehow attain valid mathematical expression. Mathematicians will even change the meaning of a concept in order to obtain a more beautiful theory. Projective geometry is an example. In Euclidean geometry, as we all know, “Two points determine a line, and two lines determine a point, unless the lines are parallel.” In projective geometry we introduce ideal points at infinity – one point for each family of parallel lines. The axiom becomes: “Two points determine a line, and two lines determine a point.” Clearly this axiom is the right one. The Euclidean axiom by comparison is unesthetic.

In addition to Gorenstein and Appel and Haken, H.P.F. Swinnerton-Dyer is quoted in Davis and Hersh (1981, pp. 386–387) on the issue of computer reliability. Unlike Halmos and Appel and Haken, Swinnerton-Dyer acknowledges the uncomfortable position of the mathematician who must accept both the fallibility and also the indispensability of his computer.

What becomes of the traditional notion of proof in this situation? Computer proofs differ in two opposite ways from the old, handmade kind. They are freer from the old human blunders. But they are liable to new kinds of unreliability, which can be significant in large-scale computations.

The use of computers in mathematics will not go away. There will continue to be different varieties of proof, different levels of rigor. Not only machine proof versus machine-free proof, but also proof by a network of dozens of mathematicians (à la Gorenstein) versus proofs by one or two; probabilistic proofs (à la Davis-Miller-Rabin-Schwartz); and, more than likely, still other kinds as yet unknown. How will mathematicians adjust to such nonuniformity in the meaning of proof? We could think of proofs as having variable quality. Instead of saying “proved,” we could say “proved by hand” or “proved by machine.” Maybe we could even give an estimate of the reliability of the machine calculation used in a proof (Swart, 1980).

There are precedents for this situation. In the years between the two World Wars, some mathematicians who mistrusted the axiom of choice made it a practice to state explicitly where they had used it. In 1972 Errett Bishop said that the disagreement between constructivists and classicists would be resolved if the
classicists would only state explicitly when they used the law of the excluded middle (L.E.M.). (Constructivists reject the L.E.M. with respect to infinite sets.) Of course these issues did not involve computing machines. But they did involve deep disagreement about standards of rigorous proof.

If experience with the axiom of choice and the L.E.M. is indicative, then Swart’s proposal is not promising. Nobody worries any more about the axiom of choice, and few worry about the L.E.M. Perhaps mathematicians are not much interested in careful distinctions about quality or certainty of proof. Perhaps before long few will care if proofs are hand-made or computer-made.

The issue of machine error is not just a matter of electrical engineering. It is the difference between computation in principle (infallible) and computation in reality (fallible). A simple calculation shows that if the probability of error in each step is greater or equal to $\epsilon$, then the probability of error in sufficiently many steps is greater than $1 - \epsilon$. In brief, if a formal proof is long enough, it is sure to contain errors.

We may have other grounds for believing the conclusion of such a proof, apart from the claimed certainty of its step-by-step reasoning. Belief can come from examples and special cases, from analogy with other results, from an expected symmetry or an unexpected elegance, even from an inexplicable feeling of rightness. All these illogical logics may tell us “it is true!” When we have something we hope is a proof, such nonrigorous reasons may make us sure of the conclusion even while we know the “proof” contains uncorrected errors. Such intuitions are an invaluable guide. They are fallible, of course. But a “rigorous proof” is also fallible. Intuition is fallible in principle; rigor is fallible only in practise.

Beyond the near-certainty of error in sufficiently long calculations, we face the unavoidable fact that calculation is finite, mathematics infinite. There are limits to the size of the biggest possible computer, to how tightly its components can be packed, how fast its signals can travel, how long it can run (the lifetime of the human race would be one limit, or, if you prefer, the lifetime of the Universe). Put all these limits together, and you have a bound on how much computing anyone will ever do. Now, if we cannot know anything in mathematics except by a formal proof (which is a particular kind of computation), then we have established a bound on how much mathematics we can ever know. The physical bound on computation implies a bound on the number and length of theorems that can ever be rigorously proved. The only way to exceed this bound would be to find quicker but less certain ways of obtaining mathematical knowledge. (See Knuth, 1976, for more on this topic.)

Meyer (1974, p. 481) cites a theorem he proved jointly with L. J. Stockmeyer: “If we choose sentences of length 616 in the decidability theory of WSIS (weak monadic second-order theory of the successor function on the nonnegative integers) and code these sentences into $6 \times 616 = 3696$ binary digits, then any logical network with 3696 input which decides truth of these sentences contains at least $10^{123}$ operations.” (Note that WSIS is much weaker than ordinary arithmetic. Note also that the conclusion applies a fortiori to sentences longer than 616 digits.)

“We remind the reader,” writes Meyer, “that the radius of a proton is approximately $10^{-13}$ cm, and the radius of the known universe is approximately $10^{28}$ cm. Thus for sentences of length 616, a network whose atomic operations were performed by transistors the size of a proton connected by infinitely thin wires would..."
densely fill the entire universe.” No decision procedure for sentences of length 616 in WSIS can be physically realized. Yet WSIS is “decidable”: a decision procedure “exists” for sentences of any finite length, if we only have the time and space to execute it.

What can we conclude from these bits of mathematical news? Our inherited notion of “rigorous proof” is not carved in marble. People will modify that notion, will allow machine computation, numerical evidence, probabilistic algorithms, if they find it advantageous to do so. Then we are misleading our pupils if in the classroom we treat “rigorous proof” as a shibboleth.

VI. Proof in our classrooms

There is an important resonance between the thinking of researchers and the thinking of teachers. Communication between them is too often mediated by textbook writers or other go-betweens. Intermediaries can introduce misconceptions which interfere with mathematics teaching. A realistic understanding of proof in mathematics would help mathematics teaching. (See Hanna, 1983, 1990.)

The role of proof in the classroom is different from its role in research. In research its role is to convince. In the classroom, convincing is no problem. Students are all too easily convinced. Two special cases will do it. Every class finds it has to omit some proofs, either for lack of time, because they are too difficult for students at that level, or just because some proofs are tedious and unenlightening. In a first course in abstract algebra, proof of the fundamental theorem of algebra is often omitted, not only from the lectures, but also from the text. Nevertheless, the students believe the unproved theorems.

What a proof should do for the student is provide insight into why the theorem is true. I am not speaking here of proofs in the sense of formal logic. As the philosophy student said to the Ideal Mathematician, she had never seen such a thing in a math class, and neither has anyone else (except, of course, in a logic class!). In classrooms informal or semi-formal proofs are presented in a natural language. They usually include calculations, which are formal subproofs within the overall informal proof.

Some instructors perhaps would say that since it’s a math course, of course you have to prove things. “If you don’t prove anything, it just isn’t math.” This makes a kind of sense. If you believe, as many do, that proof is math and math is proof, then in a math course you’re duty bound to prove something. The more you prove, the more honest and rigorous you feel that your course is.

In fact, the effect on students of mere exposure to proof is often more emotional than intellectual. If the instructor gives no better reason for proof than, “That’s math!”, how will the student find out why we do this? The student knows she/he saw a “proof”, but not why or wherefore, except: “That’s math!”

There are two opposing views on the role of proof in teaching. One view is: “Without complete, correct proof, there is no mathematics.” I call this “Absolutist,” despite that word’s unfortunate associations (absolute monarchy, absolute zero, and so on.) Absolutism sees mathematics as a system of absolute truths. If mathematics is a system of absolute truths – independent of human construction or knowledge – an immaterial, indestructible aspect of Eternity – then mathematical proofs are external and eternal. They are for us to admire, hopefully to understand, but not to play with, not to break apart. Ideally, the Absolutist teacher tells the
student nothing except what he will prove (or assign to the student to prove). The proofs he chooses will be either the most general, or the shortest. He will not be concerned about how explanatory the proof is, because explanation is not the purpose of proof. The purpose is certification: admission into the catalog of primarily absolute truths.

The opposite view, what has been called Humanist, is this: “Proof is complete explanation. It should be given when complete explanation is more appropriate than incomplete explanation or no explanation.” To the Humanist, mathematics is ours, our tool and plaything, to use and enjoy as we see fit. Proofs are not obligatory rituals. They are, like the rest of mathematics, ours, to do with as we see fit. The Humanist mathematics teacher uses the most enlightening proofs, not necessarily the most general or the shortest.

This attitude can disturb people who define a mathematician as “someone who proves theorems.” “Without proof, there is no mathematics.” From that point of view, a mathematics where proof is not an absolute is heresy.

Last semester I taught “Introduction to Abstract Algebra,” using Hungerford’s Abstract Algebra; An Introduction (1990). This book is written in theorem-proof style. The proofs, of course, are normal mathematical proofs, written in English, with no reference to formal logic. It has plenty of down-to-earth examples, and some informal discussion.

My goal was to impart an understanding of rings and groups: first by some important examples, then as general concepts. The key facts about groups and rings were stated formally in the text. I did not always prove them formally in class. “It’s in the book,” I sometimes told the class, “read it.” Results which were crucial, surprising or difficult were proved in class, in detail, with plenty of discussion. So were those whose proofs were interesting, even apart from the interest of the results.

We did not pretend it was forbidden to use a theorem before we had proved it. If we wanted to use a theorem, we did so, even if we had not yet proved it for ourselves.

I assigned a lot of homework. Some was, “Calculate!” Some was, “Prove!” A high point came when students gave three different proofs of the proposition that for any integer $n$, $n^5$ is congruent to $n$ mod 30.

What was the purpose of proof for this class? Not to prepare the student for graduate work in algebra, much less for a research career in algebra. Few of the students in that class will take another course in algebra.

In my opinion, the main purpose of an upper division course like this one (more so for a graduate course) is to introduce new concepts to the student, and to explain them. There are different kinds of explanation. A proof is a complete explanation. Sometimes a partial explanation suffices. Sometimes we skip the proof, if a lemma or theorem seems clear enough on its own. (Hanna, 1990, has related but non-identical views).

In explaining “residue classes modulo $n$,” for example, one doesn’t prove both the commutative law of addition and the commutative law of multiplication. When one proof has been seen, the second “goes without saying.” It would be tedious and unnecessary to prove both associative laws. On the other hand, the existence of reciprocals for prime $n$ must be proved, because only the proof makes comprehensible why it’s true.
In brief, the purpose of proof is understanding. The choice of whether to present a proof “as is”, to elaborate it, or to abbreviate it, depends on which is likeliest to increase the student’s understanding of concepts, methods, applications.

There is a difficulty in this policy. It depends on the notion of “understanding,” which is neither precise nor likely to be made precise. Do we really understand what it means “to understand”? No. Can we teach so as to foster “understanding”? Yes. Because we can recognize understanding, even though we can’t say precisely what it is.

At any rate, the educational value of proof is the value of complete explanation. The teacher decides whether any explanation is called for. If so, would an incomplete explanation be appropriate? Or is a complete explanation (a proof) appropriate or necessary? There may be different opinions about particular theorems, but we cannot accept a situation where some mathematics graduates say, “I never had to do proofs.”

In a stimulating article, Uri Leron (1983) adapted an idea from computing – “structured proofs.” A structured proof is analogous to a structured program. Instead of beginning by proving little lemmas whose significance appears only at the last step, a structured proof begins by breaking the proof-task into chunks. Then each chunk is broken into sub-chunks. Then the little lemmas come in, when it is clear why they are needed in the proof as a whole. I think this idea is very promising.

In the undergraduate classroom my motto is: “Proof as a tool for the teacher and class.” Not as a shackle to restrain them. In teaching aspiring mathematicians, it is: “Proof as a tool of research.” Not as a shackle on the mathematician’s imagination.

VII. Coda

The attentive reader may be left with a question. If proof is two different things, why do we call it by one name? If we use one word both for something we do in research and for something we do in the classroom, then aren’t those two somethings perhaps the same? A good classroom proof would convince a skeptical mathematician, as well as explain to a naive undergraduate. Conversely, proofs in the research literature sometimes do explain, not only convince (Gale, 1991).

Let me end with a more precise statement of the matter:

Mathematical proof can convince, and it can explain. In mathematical research, its primary role is convincing. At the high-school or undergraduate level, its primary role is explaining.

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Fresh Breezes in the Philosophy of Mathematics

Since Pythagoras, philosophy of mathematics tried to account for mathematical existence and the nature of mathematical objects.

Numbers, circles, \(n\)-dimensional manifolds, all are different from everything else we think about. They’re neither physical nor mental. Not mental, because the Pythagorean theorem or any other well-established mathematical fact is independent of what you or I think. Whether we know it and believe it or don’t know it and don’t believe it, the Pythagorean theorem is still true. Yet it’s not physical either! Plato and Aristotle explained that the triangles and circles of the geometer are not physical triangles or circles, but something “ideal.”

Spiritual, empirical, psychological, formalist, and logicist explanations have been offered. None give a credible account of what we do when we do mathematics. Presently some authors are constructing a humanist answer.

An Israeli mathematics education researcher, Anna Sfard, recently found an interesting insight. In learning a mathematical concept, children first learn it as algorithm—procedure, or method. Later, the algorithm is transformed into an object. She calls this “reification.” It’s difficult to achieve, often needing help from teacher. This story is close to theories of the Russian psychologist, Lev Vygotsky.

For example, subtraction is an algorithm. It isn’t hard. It reifies into negative numbers—very hard!

Which mathematical entities are frozen algorithms? What’s the interaction between doing and being, algorithm and entity? *This is a question in philosophy of mathematics based on mathematical practice, on seeing mathematics as a human activity. It’s not a foundationist question.*

Foundations lost

In books on philosophy of mathematics (Korner, or Benacerraf & Putnam) you read of the leading problem, “foundations.” How can we establish mathematical knowledge as certain, indubitable, free of any possible doubt? Three historically important solutions to this problem were logicism (Platonism), formalism, intuitionism. All were unsuccessful. For logicism and formalism, no major new idea has come up in over half a century. Intuitionism and its daughter constructivism did strive to carry out the program of Brouwer streamlined by Bishop. But their goal of remaking mathematics constructively is more remote today than 60 or 70 years ago.

This article originated as an invited talk to the 1993 annual joint meeting of the sections on mathematics and on philosophy of the New York Academy of Science. Thanks to Prof. Bruce Chandler and Prof. Harold Edwards for the invitation to the New York Academy. Double thanks to Prof. Hao Wang of Rockefeller University, whose hospitality in the spring of 1993 was generous and inspiring.
The surviving scrap of foundationalism was named “neo-Fregeanism” by Philip Kitcher. This notion still dominates the philosophy of mathematics. It says: “Philosophical thinking about mathematics need not concern itself with anything but sets, and set theory’s twin sister, logic.” But most researchers, users, teachers, historians of mathematics aren’t primarily interested in sets.

**Phil / m and phil / sci**

One weird phenomenon of modern philosophy is that philosophy of science and philosophy of mathematics are almost disjoint. Authors in philosophy of science rarely refer to philosophy of mathematics, and vice versa. An author who writes on both subjects, in any one article sticks to one or the other. It’s like baseball and football—play one or the other, but not both at the same time.

I like to compare philosophy of mathematics today to philosophy of science in the 30’s and 40’s. That subject was dominated by logical positivists: Rudolf Carnap and his friends of the “Wiener Kreis” (Vienna Circle). As a result of taking Bertrand Russell and Ludwig Wittgenstein too seriously, they believed they knew the correct methodology for scientific work: (1) state the axioms; (2) give correspondence rules between words and physical observables; (3) derive the theory, as Euclid derived geometry, or Mach derived mechanics.

It was noticed after a while that what logical positivists said had little in common with what scientists did or wanted to do. New ideas in philosophy of science came from Karl Popper, Tom Kuhn, Imre Lakatos, Paul Feyerabend. These subversives disagreed with each other. But they all thought philosophers of science could think about what scientists actually do, not bring presuppositions and instructions for scientists to ignore.

Philosophy of mathematics is overdue for its Popper, Kuhn, Lakatos, and Feyerabend. It’s overdue for analysis of what mathematicians actually do, and the philosophical issues therein.

In fact, this turn is taking place. Wittgenstein and Lakatos helped start it. In recent years Michael Polanyi, George Polya, Alfred Renyi, Leslie White, Ray Wilder, Greg Chaitin, Phil Davis, Paul Ernest, Nick Goodman, Phil Kitcher, Penelope Maddy, Michael Resnik, Gian-Carlo Rota, Brian Rotman, Gabriel Stolzenberg, Robert Thomas, Tom Tymoczko, Jean Paul van Bendegem, and Hao Wang have participated.

Here are ideas some of these people hold.

1) Mathematics is human. It’s part of and fits into human culture. (Not Frege’s abstract, timeless, tenseless, objective reality.)

2) Mathematical knowledge is fallible. Like science, mathematics can advance by making mistakes and then correcting and recorrecting them. (This “fallibilism” is brilliantly argued in Lakatos’ *Proofs and Refutations*.)

3) There are different versions of proof or rigor, depending on time, place, and other things. The use of computers in proofs is a nontraditional version of rigor.

4) Empirical evidence, numerical experimentation, probabilistic proof all help us decide what to believe in mathematics. Aristotelian logic isn’t necessarily always the best way of deciding.

5) Mathematical objects are a special variety of social-cultural-historical object. We can tell mathematics from literature or religion. Nevertheless, mathematical
objects are shared ideas, like Moby Dick in literature, or the Immaculate Conception in religion.

How do humanists answer the big question, “What’s the nature of mathematical objects?"

The question seems difficult because of a centuries-old assumption in Western philosophy: “In the world there are two kinds of things. What’s not physical is mental; what’s not mental is physical.” When Frege proved that mathematics is neither physical nor mental, he accounted for it by means of a third kind of entity—"abstract objects"—about which he could say nothing except that they’re neither physical nor mental.

Mental is thought, individual consciousness, subjectivity; wishes, fears, perceptions, hopes, desires, private thoughts.

Matter is what takes up space, has weight, can be studied by scientific instruments. Mountains, bugs, the stars, gamma rays.


Does the New York Academy of Science exist? Undoubtedly. Is it mental? If the Secretary and the President of the Academy died of amnesia, the life of the Academy would continue. The Academy isn’t just somebody’s thoughts! Even if the building were blown up and the trustees moved the Academy to Yonkers, it would go on. Its physical and mental embodiments are necessary, but they’re not it. The Academy isn’t just the minds and bodies of anyone. Neither is it just the stones of its building.

What is it? It’s a social institution. The mental and physical aren’t sufficient to describe the New York Academy of Science. Nor are they sufficient to describe most of the things that most concern us. Marriage and divorce, employment, shopping, prices and salaries, war and peace, professional sports and television shows. All have mental and physical aspects, but they aren’t mental or physical entities. They’re social entities.

There are not two but three basic kinds of things in the world.

Now, what about mathematical objects—let’s just say numbers. If everything’s either mental, physical, or social, then what are numbers? We’ve already seen that numbers aren’t mental or physical. By the law of the excluded middle, they must be social. But let’s not be peremptory. Let’s consider it a hypothesis. Is mathematics social-cultural-historical?

Certainly it’s historical. The history of mathematics is a developed subject. Historians have studied mathematics back to the Babylonians. We don’t know the remote origin of mathematics, or the remote origin of writing, speech, religion, or the family. That origin was part of the self-creation of the human race. Archeology, linguistics, genetics, ethnology tell us a little more. Counting and talking both had their human beginnings.

Mathematics is a social entity. Mathematicians never were isolated hermits. Today they’re in academic, government or industrial jobs, paid directly or indirectly by the government.

Srinivasa Ramanujan, the self-taught Indian mathematical genius, worked hard to be recognized by the English mathematics establishment. Once he was invited, he went to England, at a cost to his family, his religious commitment, and his ability
to find daily food he could eat. His did so in order to work with mathematicians who understood what he was doing.

In the 16th and 17th centuries, Fermat, Huygens, Leibnitz were assiduous letter writers, constantly trading ideas with colleagues in other cities and other countries.

Today a new result is certified as part of mathematics after experts read it and pronounce it good. We monitor our product. Acceptance by the profession is essential to be recognized or accepted as a mathematician.

The overall content of mathematics and its direction of movement respond to the pressures of society. The militarization of U.S. mathematics in World War II is an example.

Newton’s calculus was a tool in his theory of gravitation. His gravitation theory was a response to the need for better understanding of the motions of planets. The motions of planets were important because England was a maritime nation. Navigational methods better than those of Spain and Portugal had cash value for England.

In saying this, I don’t underestimate the insistence of pure mathematicians on autonomy.

Taking the test

To test a philosophy of mathematics, ask it questions:

(1) What makes mathematics different?
(2) What is mathematics about?
(3) Why does mathematics achieve near-universal consensus?
(4) How do we acquire knowledge of mathematics, apart from proof?
(5) Why are mathematical results independent of time, place, race, nationality and gender, in spite of the social nature of mathematics?
(6) Does the infinite exist? If so, how?
(7) Why does pure mathematics so often become useful?

The humanist approach gives better answers to questions 1 through 5 than the neo-Fregean, the intuitionist-constructivists, or any other proposed philosophy I know of.

Questions 6 and 7 are harder. I don’t say humanism answers these questions. But neither does anybody else.

In conclusion, I want to destroy one of the most popular arrows opponents like to shoot at mathematical humanism.

\[2 + 2 = 4,\] they say, everywhere and always. In fact, \[2 + 2 = 4\] before there were human societies, or even human beings. When 2 brontosaurus went to the water hole and met two other brontosaurus, there were four brontosaurus at the water hole. The truths of mathematics are universal, independent not only of individual consciousness but of social consciousness.

This is Platonism, the view that Wittgenstein attacked so fiercely, and the view, let’s face it, that most mathematicians accept.

How can a humanist answer?

First of all, “two” plays two roles. It’s an adjective and it’s a noun. When you say “two brontosaurus,” “two” is an adjective. “Two brontosaurus plus two brontosaurus equals four brontosaurus” is a statement about brontosaurus, not about numbers. Even if you say “Two discrete, reasonably permanent, non-interacting objects collected together with two others of the same ilk makes four
such objects,” you are talking about properties of discrete, reasonably permanent non-interacting objects. That’s a statement in elementary physics.

The noun “two,” on the other hand, as everybody since Pythagoras knows, doesn’t name a physically observable thing. It names some abstract or ideal entity. Plato, Descartes, Frege knew that two is an ideal object. They explained what they meant by an ideal object only in negative terms—not mental, not physical. I’m pointing out that these abstract ideal objects are social concepts.

“But,” says the Platonist, “how can you explain the fact that always and everywhere, regardless of time and place, politics or religion, race or sex, 2 + 2 always equals 4? The only way to account for it is to say it’s an objective truth, which we all recognize because it’s an objective truth. Otherwise, the universal agreement that 2 + 2 = 4 would be an inexplicable miracle.”

To this I answer, “It’s bad logic to say something must be true because you can’t think of any other explanation. That’s how philosophers used to prove the existence of a Supreme Creator—they couldn’t conceive any other way for there to be a universe.

“You say that because I haven’t got an explanation that satisfies you about the objectivity of mathematics, therefore I must believe in abstract entities whose relation to the physical world is obscure, which number incredibly remote uncountable infinities, and which are apprehended by our mental or physical faculties in a quite unexplained manner.

“I don’t believe in them. You believe in them only by closing your eyes to their absurdity.”

“I’m aware that some social or intersubjective concepts have the rigidity, the reproducibility, of physical science. The reproducibility of a mathematical calculation is comparable only to the reproducibility of a physical measurement or experiment.”

Somebody might ask, “Why does the physical world have attributes which are so consistent, so reproducible? Why is the gravitational constant the same from one day to the next? Why is the speed of light in vacuum so reliable?”

No physicist or philosopher feels obliged to answer such questions. The possibility of a science of physics is something we accept. We start from there, we don’t try to go back of it. Heidegger asked, “Why is there a universe?” I don’t know what progress he made. Not a promising investigation.

As there’s lawfulness and stability in parts of the physical world, there’s lawfulness and stability in parts of the social-conceptual world. I don’t know why this is so. I’m sure it’s a fruitless question, as fruitless as the same question about the physical world.

Study of the lawful, predictable parts of the physical world has a name. That name is “physics.” Study of the lawful, predictable parts of the social-conceptual world has a name. That name is “mathematics.”

References


Definition of mathematics

Mathematics is a science, like physics or astronomy; it constitutes a body of established facts, achieved by a reliable method, verified by practice, and agreed on by a consensus of qualified experts. But its subject matter is not visible or ponderable, not empirical; its subject matter is ideas, concepts, which exist only in the shared consciousness of human beings. Thus it is both a science and a “humanity.” It is about mental objects with reproducible properties.

For example, “the triangle” in Euclidean geometry, or the counting numbers 1, 2, 3, 4, in arithmetic, are concepts which we can communicate, and which, as we can verify, keep their properties as they are communicated. These concepts are reproducible, they possess a certain rigidity, a reliability and consistency, and so they permit conclusive, irresistible reasoning—which is what we call “proof.”

“Proof,” not in the formal or formalized sense, but in the sense in which mathematicians mean proof—conclusive demonstrations that compel agreement by all who understand the concepts involved. Abstract concepts subject to such conclusive reasoning or proof are called mathematical concepts.

Mathematics is the subject where answers can definitely be marked right or wrong, either in the classroom or at the research level. Mathematics is the subject where statements are capable in principle of being proved or disproved, and where proof or disproof bring unanimous agreement by all qualified experts—all who understand the concepts and methods involved.

Reasoning about mental objects (concepts, ideas) that compels assent (on the part of everyone who understands the concepts involved) is what we call “mathematical”. This is what is meant by “mathematical certainty”. It does not imply infallibility! History shows that the concepts about which we reason with such conviction have sometimes surprised us on closer acquaintance, and forced us to re-examine and improve our reasoning.

Ah, but on the library shelves, in the math section, all those formulas and proofs, isn’t that math? No, as long as it just sits on the shelf, it’s just ink on paper. It becomes mathematics—it comes alive—when somebody starts to read it. And of course, it was alive when it was being thought and written by some mathematician.

The old standard dictionary definition of mathematics was something like, “the study of the properties of numbers and geometrical figures.” This was good enough up to some time in the 19th century. But today mathematics includes abstract algebra, logic, and probability, none of which is part of traditional arithmetic or geometry.

What distinguishes mathematics from other sciences, whether physical, biological, or socio-cultural? The other sciences study some concrete objects, which are
visible, ponderable or detectable by physical apparatus. The things mathematics studies are neither visible nor ponderable nor detectable by physical apparatus.

On the other hand, what distinguishes mathematics from philosophy, literary criticism, legal theory or economic theory, where *shared concepts are the subject of study*? In those fields, we find argument and reasoning about abstract entities, but usually it can not be conclusive. Usually it leaves room for continuing unresolved dispute and disagreement. If, in some field of abstract thought, such as linguistics for example, concepts do arise which lend themselves to conclusive and decisive reasoning, that field is then characterized as “mathematical”, and we have “mathematical linguistics.”

Certainly mathematics itself isn’t the only place where conclusive reasoning occurs! Rigorous reasoning can occur anywhere—in law, in textual analysis of literature, and in ordinary daily life apart from academics. Historians can use unimpeachable reasoning to establish a sequence of events, or to refute anachronistic claims. But although historical dates are subject to rigorous reasoning, they are not *mathematical objects*, because they are tied to specific places and persons. Information about them comes, ultimately, from someone’s visual or auditory perceptions.

Mathematical conclusions are decisive. Just as physical or chemical knowledge can be independently verified by any competent experimenter, an algebraic or geometric proof can be checked and recognized as a proof by any competent algebraist or geometer. There has been one famous disagreement about valid mathematical proofs, Luitjens Brouwer and Errett Bishop rejected “proof by contradiction.” That disagreement resulted in the development of a variant, “intuitionistic” or “constructivist” mathematics. Intuitionistic or constructivist mathematics makes a stricter demand on what is a “rigorous proof.” Knowing how to recognize and accept a “rigorous proof” is the condition for membership in the community of mathematicians, whether the usual “classical” or the minority “constructivist” version.

Other, hitherto unthought-of kinds of mathematical behavior will yet arise. A definition of mathematics should accept the yet-to-be-created new mathematical subjects that are sure to arise in coming decades, not to say centuries. How will we identify such hitherto unseen behavior as mathematical? How has it been decided in the past, that some new branch of study is not just “mathematical” (containing some mathematical features), but really *mathematics*—requiring to be included within mathematics itself?

One famous example was probability—gambling or betting. Fermat and Pascal demonstrated “rigorous” (irrefutable, compelling) conclusions about some games of chance. Therefore their work was mathematical, even though it was outside the bounds of mathematics as previously understood. Subsequent work of Bernoulli, De Moivre, Laplace and Chebychev was mathematics, for the same reason. Ultimately Kolmogorov axiomatized probability in the context of abstract measure theory. In doing so he was axiomatizing an already existing, ancient branch of mathematics.

A more recent example is set theory. Infinite sets were not part of mathematics before Georg Cantor explicitly based them on the notion of one-to-one correspondence. On that basis, he was able to make compelling arguments, and then set theory (with some resistance) became a mathematical subject.

Since Aristotle, formal logic has helped to clarify mathematical reasoning, and rigorous argument in general. It draws conclusions on the basis of the logical
form of statements—their “syntax.” But most mathematical argument is based more on the content of mathematical statements than on their logical form. It is done without referring to the rules of formal logic, even without awareness of them. In the process of actively discovering or creating mathematics, logicians and other mathematicians reason by analogy, by trial and error, or by any other kind of guessing or experimentation that might be helpful. In fact, formal logic itself is well-established as a part of mathematics! As such, it is subject to conclusive reasoning that is informal, like any other part of mathematics. Logicians reason informally in proving theorems about formal logic. (This remark of Imre Lakatos [Proofs and Refutations, introduction], is now a commonplace).

George Lakoff and Rafael Nunez, in Where Mathematics Comes From, showed that mathematical proof often can be understood as based on “embodied metaphors.” That explanation of proof cannot be formalized. In fact, mathematical proof is too varied to be pinned down in a single precise, universal description.

Saunders MacLane, among others, said, “What characterizes mathematics is that it’s precise.” But what, precisely, should be meant here, by “precise”? Not numerical precision. A huge part of modern mathematics, including MacLane’s contribution, is geometrical or syntactical, not numerical. Should “precise” mean formally explicit, expressed in a formal symbolism? No. There are famous examples in mathematics of conclusive visual reasoning, accepted as mathematical proof prior to any post hoc formalization. Several famous mathematicians have said “You don’t really understand a mathematical concept until you can explain it to the first person you meet in the street.”

Probably the correct interpretation of “precise” should be simply, “subject to conclusive, irrefutable reasoning.” So I am accepting the familiar claim, “Mathematics is characterized above all by precision,” but only after “unpacking” what we should mean by “precise.”

What about “applied mathematics”? Applied mathematics uses whatever arguments and methods it can—analogy, special examples, numerical approximations, physical models—to learn about hurricanes, say, or epidemics. It is mathematical activity, to the extent that it makes use of mathematical concepts and results, which are, by definition, concepts and results capable of strict mathematical reasoning—rigorous proof. Mathematical activity or behavior includes: thinking, wondering, dreaming, learning about mathematics; solving math problems, at all levels, from pre-kindergarten up through postdocs and Fields Prize winners; and teaching mathematics, at all levels. (If not, then we’d call it bad teaching.) It includes ordinary commercial calculations too, and routine plugging of numbers into formulas by engineers and technicians. And geometrical reasoning, and probabilistic reasoning, and combinatorial reasoning, and any formal logical reasoning. All the way back to the mathematical behavior of the Maya calendar makers, and the ancient Polynesian navigators.
Introduction to “18 Unconventional Essays on the Nature of Mathematics”

This book comes from the Internet. Browsing the Web, I stumbled on philosophers, cognitive scientists, sociologists, computer scientists, even mathematicians!—saying original, provocative things about mathematics. And many of these people had probably never heard of each other! So I have collected them here. This way, they can read each other’s work. I also bring back a few provocative oldies that deserve publicity.

The authors are philosophers, mathematicians, a cognitive scientist, an anthropologist, a computer scientist, and a couple of sociologists. (Among the mathematicians are two Fields Medal winners and two Steele Prize winners.) None are historians, I regret to say, but there are two historically oriented articles. These essays don’t share any common program or ideology. The standard for admission was: Nothing boring! Nothing trite, nothing trivial!

Every essay is challenging, thought-provoking, and original.

Back in the 1970s when I started writing about mathematics (instead of just doing mathematics), I had to complain about the literature. Philosophy of science was already well into its modern revival (largely stimulated by the book of Thomas Kuhn). But philosophy of mathematics still seemed to be mostly foundationist ping-pong, in the ancient style of Rudolf Carnap or Willard Van Ormond Quine. The great exception was Proofs and Refutations by Imre Lakatos. But that exciting book was still virtually unknown and unread, by either mathematicians or philosophers. (I wrote an article entitled “Introducing Imre Lakatos” in the Mathematical Intelligencer in 1978.)

Since then, what a change! In the last few years newcomers—linguists, neuroscientists, cognitive scientists, computer scientists, sociologists—are bringing new ideas, studying mathematics with new tools. (George Lakoff-Rafael Nunez, Stanislas Dehaene, Brian Butterworth, Keith Devlin).

In previous centuries, old questions—“What is Man?” “What is Mind?” “What is Language?”—were transformed from philosophical questions, free for speculation, into scientific problems. The subjects of linguistics, psychology and anthropology detached from philosophy to become autonomous disciplines. Maybe the question, “What is mathematics?” is coming into recognition as a scientific problem.

In 1981, in The Mathematical Experience, speaking about the prevailing alternative views of the nature of mathematics, Phil Davis and I asked, “Do we really have to choose between a formalism that is falsified by our everyday experience, and a Platonism that postulates a mythical fairyland where the uncountable and the inaccessible lie waiting to be observed by the mathematician whom God blessed with a good enough intuition? It is reasonable to propose a different task for
mathematical philosophy, not to seek indubitable truth, but to give an account of mathematical knowledge as it really is—fallible, corrigible, tentative, and evolving, as is every other kind of human knowledge. Instead of continuing to look in vain for foundations, or feeling disoriented and illegitimate for lack of foundations, we have tried to look at what mathematics really is, and account for it as a part of human knowledge in general. We have tried to reflect honestly on what we do when we use, teach, invent, or discover mathematics.” (p. 406)

Before long, the historian Michael Crowe said these words were “a programme that I find extremely attractive.” In 1986-1987 Crowe visited Donald Gillies at King’s College in London. Gillies had been a student of Imre Lakatos. In 1992 Gillies published an anthology, *Revolutions in Mathematics*, where historians of mathematics like Crowe collaborated with philosophers of mathematics like Gillies.

Such collaboration developed further in Emily Grosholz and Herbert Breger’s anthology, *The Growth of Mathematical Knowledge* (Kluwer, 2000). Emily Grosholz wrote, “during the last decade, a growing number of younger philosophers of mathematics have turned their attention to the history of mathematics and tried to make use of it in their investigations. The most exciting of these concern how mathematical discovery takes place, how new discoveries are structured and integrated into existing knowledge, and what light these processes shed on the existence and applicability of mathematical objects.” She mentions books edited by Philip Kitcher and William Aspray, by Krueger, and by Javier Echeverria. She finds the Gillies volume “perhaps the most satisfactory synthesis.”

History of mathematics is today a lively and thriving enterprise. It is tempting to start listing my favorite historians, but I will limit myself to singling out the monumental work by Sanford L. Segal, *Mathematicians Under the Nazis* (Princeton, 2003).

Already in my 1979 article “Some Proposals for Reviving the Philosophy of Mathematics,” (reprinted in Thomas Tymoczko’s anthology, *New Directions in the Philosophy of Mathematics*, Birkhauser, 1986), and at greater length in my two subsequent books, I explained that, contrary to fictionalism, mathematical objects do exist—really! But, contrary to Platonism, their existence is not transcendental, or independent of humanity. It is created by human activity, and is part of human culture. I cited the 1947 essay by the famous anthropologist Leslie White, which is reprinted here. And at last, in 2003 and 2004, a few philosophers are also recognizing that mathematical objects are real and are our creations. (Jessica Carter, “Ontology and Mathematical Practice”, *Philosophia Mathematica* 12 (3), October 2004; M. Panza, “Mathematical Proofs,” *Synthese*, 134, 2003; M. Muntersbjorn, “Representational innovation and mathematical ontology,” *Synthese*, 134, 2003). I know of recent conferences in Mexico, Belgium, Denmark, Italy, Spain, Switzerland, and Hungary on philosophical issues of mathematical practice.

While others are starting to pay more attention to our ways, we mathematicians ourselves are having to look more deeply at what we are doing. A Special Interest Group on philosophy is now active in the Mathematical Association of America. A famous proposal by Arthur Jaffe and Frank Quinn in 1993 in the *Bulletin of the American Mathematical Society*, to accept not-so-rigorous mathematics by labeling it as such, provoked a flood of controversy. (The essay by William Thurston in this volume was his contribution to that controversy.)
After the rest of this book had gone to the editor at Springer-Verlag, I found an article on the Web by Jonathan M. Borwein, the leader of the Centre for Experimental and Constructive Mathematics at Simon Fraser University in Vancouver. He quoted approvingly this five-point manifesto of mine:

1. **Mathematics is human.** It is part of and fits into human culture. It does not match Frege’s concept of an abstract, timeless, tenseless, objective reality.

2. **Mathematical knowledge is fallible.** As in science, mathematics can advance by making mistakes and then correcting or even re-correcting them. The “fallibilism” of mathematics is brilliantly argued in Lakatos’ *Proofs and Refutations*.

3. **There are different versions of proof or rigor.** Standards of rigor can vary depending on time, place, and other things. The use of computers in formal proofs, exemplified by the computer-assisted proof of the four color theorem in 1977, is just one example of an emerging nontraditional standard of rigor.

4. **Empirical evidence, numerical experimentation and probabilistic proof all can help us decide what to believe in mathematics.** Aristotelian logic isn’t necessarily always the best way of deciding.

5. **Mathematical objects are a special variety of a social-cultural-historical object.** Contrary to the assertions of certain post-modern detractors, mathematics cannot be dismissed as merely a new form of literature or religion. Nevertheless, many mathematical objects can be seen as shared ideas, like Moby Dick in literature, or the Immaculate Conception in religion.


As more and more important proofs approach and go beyond the limits of conventional verification, mathematicians are having to face honestly the embarrassing ambiguity and temporal dependence of our central sacred icon—rigorous proof.

In his 1986 anthology Thomas Tymoczko called attention to the troublesome philosophical issues raised by the recent proof of the famous four-color theorem. This was the first time the solution of a major mathematical problem had relied essentially on machine computation.

Today, the status of several other famous problems raises even more prominent and severe difficulties. The story of Thomas Hales’ “99% accepted” proof of the Kepler conjecture makes clear that something new and strange is happening in the very center of the mathematical enterprise (see George Szpiro, *Kepler’s Conjecture*, Wiley, 2003). (Then there is also the on-going decades-long ups-and-downs, the many thousands of pages proof, of the classification of simple finite groups. See Ron Solomon, “On Finite Simple Groups and their Classification,” *Notices of the AMS* 42 (2), February 1995, 231-239.)

Johannes Kepler in 1611 considered how spherical balls can be packed to fill space as densely as possible. There are three natural ways to pack spheres, and it’s clear which of the three is best. Kepler guessed that this way is in fact the best possible. It turns out that this is fiendishly hard to prove. Wu-Yi Hsiang of the University of California, Berkeley, claimed to have a proof in 1993, but he failed to convince his colleagues and competitors. He has not relinquished his claim and continues to hold to it. Thomas Hales of the University of Michigan announced
a proof by a different method in 1997. His proof follows suggestions made earlier by Laszlo Fejes-Toth, and it involves, like the famous computer proof of the four-color theorem, computer checking of thousands of separate cases, many of them individually very laborious. The *Annals of Mathematics* invited Hales to submit his manuscript. It is 250 pages long. A committee of 12 experts was appointed to referee the paper, coordinated by Gabor Fejes-Toth, Laszlo’s son. After four years, the committee announced that they had found no errors, but still could not certify the correctness. They simply ran out of energy and gave up. Roberet Macpherson, the editor of the *Annals*, wrote, “The news from the referees is bad, from my perspective. They have not been able to certify the correctness of the proof, and will not be able to certify it in the future, because they have run out of energy to devote to the problem. This is not what I had hoped for.” He reluctantly acceded to their decision, and accepted the theoretical part of Hales’ paper, leaving the computer part for publication elsewhere. Hales then announced that he was affiliating with a group of computer scientists known as the QED Project. This dormant project had as its original stated goal: to computerize all of mathematics! Hales’ new project, the Flyspeck Project, proposes to do a computer coding and verification of his proof of the Kepler conjecture—a proof which, in ordinary mathematical form, was already too long and complicated to be completely checked, in four years, by a committee of 12 leading human experts. Project Flyspeck is expected to involve the work of hundreds of people and take 20 years. Donald Mackenzie’s article in this volume sheds some light on these issues.

Such a story undermines our faith that mathematical proof will remain as we have always thought of it—that after reasonable time and effort, its correctness must be definitely decidable by unanimous consensus of competent specialists.

In fact, even without regard to Hales’ theorem, it is easy to see that in principle there must be an upper bound on the length and complexity of the longest proof that at any time can be completely checked and verified by the mathematical community. In principle it is possible for a recognized, established mathematician to submit a proof longer than this upper bound. What should be the status of such a proof? Should it be accepted for publication? What degree of conviction or credibility should we attribute to its conclusion? Should it depend on our estimation of the reliability of its creator? May we use it as a building block in our own research? What if it has “applications” in physics? Such judgments are made every day in the “real world” of ordinary life. But in mathematics??!!

As I explained in my book, *What is Mathematics, Really?* (1997, Oxford) the words “mathematical proof” have two different meanings—and the difference is not usually acknowledged. One meaning, found in logic texts and philosophy journals, is “a sequence of formalized statements, starting with unproved statements about undefined terms, and proceeding by steps permitted in first-order predicate calculus.” The other meaning, not found in a precise or formal statement anywhere, is “an argument accepted as conclusive by the present-day mathematical community.” The problem is to clarify and understand—not justify!!—the second meaning. A first stab at clarification might be, “an argument accepted as conclusive by the highest levels of authority in the present-day mathematical community.” Such a clarification rests on several implicit hypotheses:

1. that there is a “mathematical community”.
2. that this “community” has accepted “high levels of authority”.

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(3) that these “high authorities” have a legitimacy based on some generally accepted rationale.

(4) that the highest level of authority can agree on what to accept.

(5) that arguments accepted as proofs by the recognized highest levels of authority in the mathematical community will remain accepted, at least for a very long time, at least with very high probability.

What seems to be threatening is that increasing length and complexity of proposed proofs, whether involving heavy use of computers or not, may go beyond the capacity of recognized authorities to reach a convincingly informed consensus.
Part 2

“Mostly for the left hand”
Introduction

These articles are more light-hearted than those in Part 1. I permit myself some elements of fantasy, and even satire. The first seven question unquestioned assumptions about what we do when we do mathematics. *Rhetoric and Mathematics* was an invited talk at a conference on The Rhetoric of The Human Sciences, at the University of Iowa. In their preface to the proceedings of that conference, the editors, John S. Nelson, Allan Megill and Donald N. McCloskey, wrote, “Philip Davis and Reuben Hersh examine both rhetorical mathematics and mathematical rhetoric to show that even the citadel of abstraction, rigor, and precision, is thoroughly rhetorical.” (We reprinted this article in *Descartes’ Dream.*) The last two articles in this group, *Under-represented, then Over-represented: A memoir of Jews in American Mathematics* and *Paul Cohen and Forcing in 1963*, are memoirs. *Under-represented, then Over-represented: A memoir of Jews in American Mathematics* should be grouped with *Mathematics and Ethics* and *Ethics for Mathematicians*, as dealing with ethical questions. *Paul Cohen and Forcing in 1963* recounts my difficult collaboration with a famous mathematician, Paul J. Cohen. I offer a brief description of his method of “forcing”, meant for the mathematician unpracticed in mathematical logic.
Rhetoric and Mathematics
(with Philip J. Davis)

If rhetoric is the art of persuasion, then mathematics may seem to be its antithesis. This is believed, not because mathematics does not persuade, but rather because it seemingly needs no art to perform its persuasion. The matter does it all; the manner need only let the matter speak for itself.

In Euclid we find only bare statements of the “common notions” (the “axioms” or “postulates”) followed by a rigorous and unmerciful chain of theorem, proof, theorem, proof. Indeed, in the high school geometry in which Euclid was force-fed to uncounted millions of schoolchildren, “proof” was reduced to a formal scheme in which two adjacent columns, “statements” on the left and “reasons” on the right, led inexorably from the “given” to the “to prove,” from hypotheses to conclusion.

From the definitions and the axioms, the theorem is inescapable. Anyone who understands its statement will agree to its truth; to fail to agree would be to declare oneself incompetent before class and teacher.

“Mathematical certainty” is a byword for a level of certainty to which other subjects can only aspire. As a consequence, the level of advancement of a science has come to be judged by the extent to which it is mathematical. First come astronomy, mechanics, and the rest of theoretical physics. Of the biological sciences, genetics is top dog, because it has theorems and calculations. Among the so-called social sciences, economics is the most mathematical and offers its practitioners the best job market, as well as the possibility of a Nobel prize.

Mathematization is offered as the only way for a field of study to attain the rank of a science. Mathematization means formalization, casting the field of study into the axiomatic mode and thereby, it is supposed, purging it of the taint of the lawyerly tricks used by those who are unable to let facts and logic speak for themselves. For those who want to challenge this dogma, to assert the claim of rhetoric as a necessary and valid aspect of any human endeavor, mathematics appears as the dragon which must be slain.

Now, the purpose of the present article is to undermine these claims for mathematization. We say “undermine,” not “refute” or “destroy,” for we are well aware that the claims for mathematization are not made without reason. But their validity is limited. As skeptical a look should be cast upon mathematical theories as upon theories stated in “ordinary language.”

Our goal is to show that mathematics is not really the antithesis of rhetoric, but rather that rhetoric may sometimes be mathematical, and that mathematics may sometimes be rhetorical. Our first task will be to point out (what is already
generally admitted) that mathematical language, mathematical trappings, are used as a rhetorical device in various fields of endeavor, and especially in the so-called behavior sciences. Our second and major task will be to show that within the practice of mathematics itself, among the professional mathematicians, continual and essential use is made of rhetorical modes of argument and persuasion, in addition to purely formal or logical procedures.

Part 1: Mathematics as Rhetoric

It is generally believed that there are two branches of mathematics: pure and applied. We wish to point out that there are three: pure mathematics, applied mathematics, and rhetorical mathematics.

Pure mathematics is number theory, or geometry, or algebra, or analysis. It is what mathematicians do to please themselves, or each other. When they are pleased with the way something comes out, they are likely to say that it is elegant, or deep. What does that mean? Well, “deep” means difficult, nonobvious, requiring excavation of many layers beneath the surface. And “elegant” means surprising, unexpected, accomplishing much with comparatively little labor, by means of an ingenious device or a penetrating insight.

Applied mathematics, our second category, is what mathematicians do to accomplish the tasks set by the rest of society. It is numerical weather prediction, or statistical quality control of electric light bulb manufacture, or plotting of the trajectory of a rocket to Saturn. More and more often, these days, the tasks are set and paid for by the military and involve the preparation of the premature end of life on this planet.

Finally, there is rhetorical mathematics. What is that? It is what is neither pure nor applied. Not pure, because nothing of mathematical interest is done, no new mathematical ideas are brought forward, no mathematical difficulties are overcome; and not applied, because no real-world consequences are produced. No practical results issue from rhetorical mathematics—except publications, reports, and grant proposals. The word “rhetoric” means many things. One of its invidious meanings is empty verbiage or pretentious obfuscation. Mathematics can be rhetoric in this sense of the term. We call it rhetorical mathematics.

For example, you might develop a “mathematical model” for international conflict. The model might be just a list of axioms: an axiomatic model. Or it might be a collection of strategies with an associated payoff matrix: a game-theoretic model. Or again, maybe a collection of “state variables” to specify the international military-political situation, together with a set of equations relating the values of the state variables today to their values tomorrow. Program this into your computer, and you’ve got a simulation model.

It doesn’t really matter which way you do it. You can calculate, publish, readjust your model (or throw it out and start again from scratch), calculate again, and publish again.

Why is this activity not applied mathematics? The standard picture of applied mathematics, which can be found in the first few pages of many textbooks, breaks down the work of the applied mathematician into three phases, which can be represented schematically in an arrow diagram (figure 4.1). The upper level is theory; the lower level is physical reality. The mathematical study of real-world problems
Figure 4.1. Applied mathematics

(as distinct from problems in pure mathematics) begins by construction of a mathematical model. This means the representation of quantities of physical interest by mathematical variables (most often numerical, but sometimes nonnumerical: for instance, geometrical or logical) and the derivation from physical experience of relations among these variables (most often algebraic and differential equations and inequalities).

The second step, mathematical analysis, may in some cases be solving the equation, obtaining an explicit formula for some variable, such as temperature, population size, or position of a planet. In other cases an explicit solution may not be attainable, but some approximate or qualitative conclusions can be obtained by mathematical reasoning; for example, the planet will remain within a certain distance of the sun; or, the population will at first increase rapidly and then level off and approach a certain limiting value; or, the temperature depends smoothly and monotonically on the diffusivity of the medium.

In addition to strict mathematical reasoning, step 2 may involve ad hoc simplifications, such as replacing some variables by constants, or dropping some “small” terms from the equations. Such steps may sometimes be justified by physical reasoning; sometimes they may merely be tentative trials, whose validity remains to be decided by the final result. Step 2 nowadays most often involves a machine computation. The act of setting up a computer program to analyze a real-world problem requires as a preliminary the introduction of variables and relations to model the problem in question. The machine computation may sometimes serve as a labor-saving substitute for thought and human analysis. But most often a certain
amount of thought prior to computing is essential if the computation is not to be in one way or another misguided and useless.

The third step, interpretation in real-world terms of the mathematical or computational results, may take several forms. It may be a prediction that the system of interest will behave in a certain way. It may be an explanation, showing that certain causes could have (or could not have) certain effects. In either case, the value of the whole modeling and analyzing procedure remains undetermined until the interpretation, the final result of step 3, is tested against observation or experiment, against real-world data. The merit or validity of a model depends first of all, on, the inherent reasonableness or plausibility of the assumptions involved in step 1; second, on the tractability of the model, the possibility of carrying out in step 2 mathematical operations leading to conclusions of some novelty and interest; and finally, in step 3, on the goodness-of-fit of the results, on the degree to which the theoretical results conform to the real-world data.

This “Schaum’s outline” of scientific methodology is intended to give criteria by which one may evaluate the claims for applications of mathematics in one or another field of study. The three-step paradigm is conventional and perhaps simplistic. Any particular piece of research may be limited to only one step of the three. Or all three steps may be iterated several times, as a model is gradually refined and corrected. Again, it may sometimes be impossible or inconvenient to make a clear-cut demarcation between one step and the next.

Granted all this, there are certain criteria by which the mathematician judges whether an “application” of mathematics is genuine or bogus:

- Does the depth of the real-world problem justify the complexity of the mathematical model?
- Are any genuine mathematical reasonings or nontrivial calculations carried out which require the resources of the mathematical model being proposed?
- Are the coefficients or parameters in the equations capable of being determined in a meaningful and reasonably accurate way?
- Are the conclusions capable of being tested against real-world data? Do any nonobvious practical conclusions follow from the analysis?

The introduction of mathematical methods into economics, psychology, and other branches of the so-called behavioral sciences has always been accompanied by controversy. The opponents of mathematization may have had good grounds for their resistance. But their arguments could be discounted by raising the suspicion that they did not understand the mathematical methods they were challenging. For this reason it is important to state publicly that among professional mathematicians the skepticism about behavioral-science mathematics is much stronger than it is among nonmathematical behavioral scientists.

This skepticism is rarely stated in print. Unlike philosophers and literary critics, mathematicians dislike controversy. They are not used to it and will usually keep their mouths shut to avoid it. (A famous instance was Gauss’s suppression of his own discovery of non-Euclidean geometry, for fear of a clamor from the “Boeotians.”)

An additional reason why we mathematicians seldom state in print our skepticism about behavioral-science mathematics is this: we know that some of it must be worthwhile. So we cannot condemn all of it. As a practical matter, it would be
a dreary undertaking to separate the wheat from the chaff. As a consequence we say nothing, but behind the back of the speaker on mathematical psychodynamics, we raise our eyebrows at each other and shrug.

Perhaps the knowledge that mathematicians share the opinion will strengthen the resolve of those who wish to oppose rhetorical mathematics. If they need advice or encouragement from a professional mathematician, they need go no farther than the mathematics department on their own campus. They should look for the best mathematician they can find. It does not matter whether this person is pure or applied; what matters is that he has high mathematical standards.

We can restate our negative definition of rhetorical mathematics in positive terms. Rhetorical mathematics is a form of academic gamesmanship. It depends above all on the high prestige accorded to mathematics by twentieth-century North America. Rhetorical mathematics presents itself as applied mathematics. But it is easy to tell them apart. Applied mathematics sooner or later leads to an experiment or a measurement. Either initially or ultimately, work in applied mathematics leads back to the phenomenon being modeled. Rhetorical mathematics is often incapable in principle of being tested against reality. For instance, the model may contain numerical parameters that are obviously incapable of measurement (e.g., a model of international conflict, with coefficients equal to the “aggressiveness” of the major powers).

An amusing example is brought to light in Neal Koblitz’s essay “Mathematics as Propaganda.” He quotes from *Political Order in Changing Societies*, a definitive work on problems of developing countries by the very influential Samuel Huntington. On page 55 of the book are found three equations relating certain social and political concepts:

\[
\text{social mobilization} \over \text{economic development} = \text{social frustration} \left( \frac{a}{b} = c \right)
\]

\[
\text{social frustration} \over \text{mobility opportunities} = \text{political participation} \left( \frac{c}{d} = e \right)
\]

\[
\text{political participation} \over \text{political institutionalization} = \text{political instability} \left( \frac{e}{f} = g \right)
\]

As Koblitz remarks, “Huntington never bothers to inform the reader in what sense these are equations. It is doubtful that any of the terms \(a - g\) can be measured and assigned a single numerical value. What are the units of measurement? Will Huntington allow us to operate with these equations using the well-known techniques of ninth grade algebra? If so, we could infer, for instance, that

\[
a = bc = bde = bdfg,
\]

i.e., that ‘social mobilization is equal to economic development times mobility opportunities times political institutionalization times political instability!’”
Part 2: Rhetoric in Mathematics

We turn now from rhetorical mathematics to mathematical rhetoric. We want to look at mathematical utterances or writings (the talk or writing of mathematicians in the pursuit of their work as mathematicians) and see what rhetorical aspects we can identify.

On the basis of the customary definition of rhetoric as natural discourse which serves to convince, rhetoric in mathematics would simply be common language put to the purpose of convincing us that something or other about mathematics is the case. What might we want to argue rhetorically? Certainly we would want to argue the utility of mathematics in its many applications. The philosophy of mathematics is also built up by rhetorical argumentation. But the truth of mathematics—moving down one level from a discussion of the truth to the truth itself—is considered to be established by means which are the antithesis of rhetoric. The claim made in the classroom, in the textbook, and in a good deal of philosophical writing is that mathematical truth is established by a unique mode of argumentation, which consists of passing from hypothesis to conclusion by means of a sequence of small logical steps, each of which is in principle mechanizable. T. O. Sloane has written (“Rhetoric,” Encyclopaedia Britannica), “All utterance, except perhaps the mathematical formula, is aimed at influencing a particular audience at a particular time and place.” Mathematical utterances, it would seem, stand apart. But the small measure of doubt which Professor Sloane has allowed himself can be enlarged greatly. Mathematical proof has its rhetorical moments and its rhetorical elements.

Suppose you were to eavesdrop on a college mathematics class which is sufficiently advanced that the instructor sets considerable store by mathematical proof. Imagine that you have broken into the lecture in the middle of such a proof. In theory, you should be hearing the presentation of those small logical transformations which are to lead inexorably from hypothesis to conclusion. Part of what you hear will indeed be such a litany. But other phrases will undoubtedly intervene: “It is easy to show that...”, “By an obvious generalization...”, “a long, but elementary computation, which I leave to the student, will verify that...”

These phrases are not proof: they are rhetoric in the service of proof. A hilarious compendium of rhetorical devices, used as proof substitutes, has recently been circulating among graduate students in mathematics and computer science. We quote a few lines from this work, which was compiled by Dana Angluin of the Yale Computer Science Department.

How to Prove it.
Proof by example:
The author gives only the case $n = 2$ and suggests that it contains most of the ideas of the general proof.

Proof by intimidation:
“trivial”

Proof by eminent authority:
“I saw Karp in the elevator and he said...”

Proof by cumbersome notation:
Best done with access to at least four alphabets and special symbols.

And so on for a total of twenty-four categories.
The objection may be raised that all these rhetorical handwavings, desk-poundings, appeals to intuition, to pictures, to meta-arguments, to the lack of counterevidence, to the results of papers which have not yet appeared, reflect only the laziness of the lecturer or author. Somewhere behind each theorem which appears in the mathematical literature, there must stand a sequence of logical transformations moving from hypothesis to conclusion, absolutely comprehensible, certified as such by the authorities in the field, verifiable as such by even the novice, and accepted by the whole mathematical community. This impression is absolutely false. Yet it is commonly held by people outside the mathematics profession. Mathematics students sometimes carry this picture in their minds until they are themselves involved in research; at this point they experience a sudden and unexpected shock when they realize that the real world of mathematics is far from the ideal world.

In the real world of mathematics, a mathematical paper does two things. It testifies that the author has convinced herself and a circle of friends that certain “results” are true. And it presents a part of the evidence on which this conviction is based.

It presents part, not all, because certain “routine” calculations are deemed unworthy of print. Readers are expected to reproduce them for themselves. More important, certain “heuristic” reasonings, including perhaps the motivation which led in the first place to undertaking the investigation, are deemed “inessential” or “irrelevant” for purposes of publication. Knowing this unstated background motivation is what it takes to be a qualified reader of the article.

But how does one acquire this background? Almost always, by word of mouth from some other member of the intended audience, some other person already initiated into the particular area of research in question.

And what does it mean for a mathematician to have convinced himself that certain results are true? In other words, what constitutes a mathematical proof as recognized by a practicing mathematician? Disturbing and shocking as it may be, the truth is that no explicit answer can be given. One can only point at what is actually done in each branch of mathematics. All proofs are incomplete, from the viewpoint of formal logic. How do we decide which of these incomplete proofs are wrong, and which are correct, in the sense that they are convincing and acceptable to qualified professionals?

This can be answered only by mastering the mathematical theory in question. The answer involves knowing the difference between a serious difficulty and a routine argument. A mathematician who is a certified expert in algebraic number theory might be quite unable to tell a correct from an incorrect proof in nonstandard analysis.

All that one can say is that part of being a qualified expert in, say, algebraic number theory is knowing which are the crucial points in an argument where skepticism should be focused; which are the “delicate” points, as against the routine points, in an argument; which are the plausible-seeming arguments that are known to be fallacious.

A mathematics research article (or reference work or treatise) is never written out in complete logical detail. If it were, no one would want or be able to read it. Its logical completeness would not make it more comprehensible; rather, it would make it incomprehensible, except perhaps to computing machines. (We return to the computing machine angle below.)
If it is not completeness in the sense of formal logic, then in actual practice what does guarantee correctness of mathematical proofs? Well, there is the referee, or referees, whose approval is a necessary condition for publication. Do the referees fill in and check all the logical details of every argument? Not at all. After all, they are busy people, and refereeing is done free, as a service to the profession, on top of all their other duties. It would be difficult to obtain any broad picture of what referees actually do, since this is an activity which is private and semi-anonymous (the referee’s identity is known only to the editors). Certainly there is a tremendous variation in referees. Some read every line and check every calculation; they refuse to referee any paper they cannot check in this way. It is our impression that only a small percentage of the papers published in mathematical journals receive this kind of refereeing.

For one thing, only another mathematician whose interests and training are very close to the author’s would be willing and able to do this kind of checking. Such a referee would likely be favorably prejudiced toward the submitted article and thus might be a poor judge of its interest and importance for the mathematical community at large. Someone more detached from the author’s special interest might be more objective, but probably less intensive in reading. A well-known American probabilist once described the refereeing process as follows: “You look for the most delicate part of the argument, check that carefully, and if that’s correct, you figure the whole thing is probably right.”

Undoubtedly, other factors will also influence the referee’s judgment. Do the methods and result “fit in,” seem reasonable, in the referee’s general context or picture of the field? Is the author known to be established and reliable, or is the author an unknown, or worse still, someone known to be unoriginal or liable to error?

If an article appears in print, it is hard to be sure what that means, in terms of anyone but the author’s having thoroughly understood its contents. It might help if one knew at first hand the editorial and refereeing policies of the journal in question. An editor has been quoted to us to this effect: “By choosing the referee in one way or another, I can guarantee that any particular article will be either accepted or rejected.”

Once an article is published, it might be thought that it is subject to the scrutiny of the whole mathematical community. Far from it. Most published mathematical articles attract very few readers and are forgotten within a few months, except by their authors and perhaps the author’s graduate students.

There are, of course, articles which are widely read and influential. “Widely read” must be understood in a relative sense; in most mathematical specialties, the total of active practitioners (publishers of research articles) is only a few hundred or so. The results that appear in an influential article will be read by dozens of scores of people and will be presented in seminars across the country and around the world. There is a premium, a reward, waiting for the student or mathematician who can find a serious error in such a paper. There is also an incentive to find extensions, generalizations, applications, alternative proofs, connections with other results.

If a mathematical result attracts widespread attention and survives continued scrutiny and analysis, it enters what might be called the tried and tested part of mathematics.
Does it then have guaranteed certainty? Of course not. The geometry of Euclid was studied intensively for two thousand years, yet it had major logical gaps which were first detected in the 1880s. How could we ever be sure that we are not also blind to some flaw in our reasoning?

Aha, someone may answer, we could be sure if we would only take the trouble, however troublesome it might be, to code our mathematical proofs in some appropriate computer language, insist that proofs be restricted to logical steps whose conditions could be incorporated into a computer program, and thereby make our proofs verifiable by machine.

As a matter of fact, this idea has actually been tried. One of the most arduous efforts in this direction was carried out in the 1970s by the Dutch mathematician N. G. de Bruijn and his associates. They developed a special computer language, AUTOMATH, with an associated Automath program. Their goal was to automate the process of checking the correctness of mathematical proofs. After years of intensive experimentation, the Automath project has been virtually abandoned. There are several reasons for this:

1. The formalized counterpart of normal proof material is difficult to write down and can be very lengthy.
2. Even if these translations into Automath were available in great abundance, how would one verify that they were correct, that the Automath program is itself correct, that the machine program has been correctly written, that it all has been run correctly?
3. Mathematicians and computer scientists are not really interested in doing this kind of thing.

The Automath approach represents an unrealizable dream. At the turn of the century, one might have said that a proof is that which is verifiable in an absolutely mechanical fashion. Now that a much more thoroughgoing mechanization is possible, there has been a reversal, and one hears it said that computerizability is not the hallmark of a proper proof. At the same time, the accepted practice of the mathematical community has hardly changed, except for the enlargement of the computer component.

We can show the difficulties of this “formalism” at an elementary level by looking at an attempt to present a complete, rigorous proof of a very simple theorem. Even for a very tiny piece of mathematics, the task of giving an absolutely air-tight formal proof turns out to be amazingly complicated. Professedly rigorous proofs usually have holes that are covered over by intuition. Consider the example displayed in table 4.1. This table is reproduced, with a few changes, from an excellent undergraduate textbook. It is used there to illustrate the workings of axiomatic systems, in preparation for developing the theory of non-Euclidean geometry. It is comparable to the proofs that are given in advanced works, but it is much less complex, and the individual steps are spelled out in much more detail. Table 4.1 shows three axioms, which have to do with committees and their members, and one theorem: “Every person is a member of at least two committees.” This theorem follows indeed from the axioms. This may be seen with the help of the diagram in Figure 4.2. But the point of the example is to give a completely rigorous proof. This purportedly rigorous proof is presented in 10 steps in the bottom half of table 4.1.

†This was written in 1987. As mentioned on pp. 80 and 178, today this claim is false.
A Simple Example of a Deductive System

The primitive terms are “person” and “collection.”

Definitions: A “committee” is a collection of one or more persons. A person in a committee is called a “member” of that committee. Two committees are equal if every member of the first is a member of the second and vice versa. Two committees having no members in common are called “disjoint” committees.

Axioms: 1. Every person is a member of at least one committee.
2. For every pair of persons there is one and only one committee of which both are members.
3. For every committee there is one and only one disjoint committee.

Theorem: Every person is a member of at least two committees.

Proof:

<table>
<thead>
<tr>
<th>Statement</th>
<th>Reason</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Let $p$ be a person.</td>
<td>Hypothesis; naming</td>
</tr>
<tr>
<td>2. $p$ is a member of some committee $C$.</td>
<td>Axiom 1; naming</td>
</tr>
<tr>
<td>3. Let $D$ be the committee which is disjoint from $C$.</td>
<td>Axiom 3; naming</td>
</tr>
<tr>
<td>4. Let $r$ be a member of $D$.</td>
<td>Definition of “committee”; naming</td>
</tr>
<tr>
<td>5. $r$ is not a member of $C$.</td>
<td>Definition of “disjoint”</td>
</tr>
<tr>
<td>6. There is a committee $E$ of which $p$ and $r$ are members.</td>
<td>Axiom 2; naming</td>
</tr>
<tr>
<td>7. $C$ and $E$ are not equal.</td>
<td>Definition of “equal”; 5 and 6</td>
</tr>
<tr>
<td>8. $p$ is a member both of $C$ and $E$.</td>
<td>2 and 6</td>
</tr>
<tr>
<td>9. $p$ is a member of at least two committees.</td>
<td>7 and 8</td>
</tr>
</tbody>
</table>

10. Therefore every person is a member of at least two committees. Generalization

Q.E.D.

Without disputing the conclusion—namely, that it follows from axioms 1, 2, and 3 that every person is a member of at least two committees—let us examine the claim that the written material located between the symbols “Proof” and “Q.E.D.” constitutes a proof. There is no formal definition of what an acceptable proof is. There is an informal idea that a proof is a sequence of statements written in an unambiguous and strictly formal language which proceeds from the axioms to the conclusion by means of allowed and formalized logical transformations.

As we read through the proof, we find that there is one step that is more troublesome than the others. This is step 7. We pause there, and our mind has to grind a bit before going on. Why are $C$ and $E$ not equal? Spell out the reasons a bit more. They are not equal because $r$ is a member of $E$ by line (6) but not a member of $C$ by line (5); therefore, by the definition of the equality of committees, $C$ and $E$ are not equal. This argument requires that we keep in the forefront of our mind three facts and then verify, mentally, that the situation implies nonequality. This conclusion is deduced from the definition, which speaks only of equality. Thus, in
our mind, we have to juggle simultaneously a few more facts: what equality means and how we can proceed to get nonequality out of it. In order to make clear what is going on, the author attaches to his exposition a symbolic diagram (figure 4.2), which he says is not really a part of the proof. The picture (which is not part of the “proof”) supplies the conviction and clarity which are not adequately achieved by the “real proof.” This leaves us with a very peculiar situation: the proof does not convince; what does convince is not the proof.

In all human-human interfaces or human-machine interfaces, there is always the problem of verifying that what is asserted to be so is, in fact, so. For example, we assert that we have added two integers properly, or entered such and such data into the computer properly, or the computer asserts that it has carried out such and such a process properly. The passage from the assertion to the acceptance must proceed ultimately by extra-logical criteria.

This problem confronts us constantly. We find in the “reason” column of the above proof two mysterious words: “naming” and “generalization.” There is no explanation of how “naming” and “generalization” are used in the proof. Now, if there is nothing worth discussing about these ideas in their application to the proof, why did the author bother mentioning them? Both are, in fact, difficult concepts, and philosophers have dedicated whole books to them. If they are important in the present context, how do we verify that the naming process or the generalization process has been carried out properly?

Look at “generalization.” In step 1, a typical person is selected and named. Since it is a typical person, it is not specified which person it is. The idea is that if one reasons about a typical person, and uses only the characteristics which that person shares with all other persons, then one’s deductions will apply to all
persons (line [10]). Should it not be verified, then, as part of the proof, that only those characteristics have been used? What are the formal criteria for so doing? By raising such questions, one can force the proof into deeper and deeper levels of justification. What stands in the reason column now, the single word “generalization,” is pure rhetoric.

A rather different point is this. Suppose we have set up certain abstract axioms. How do we know that there exists a system which satisfies these axioms? If there is no such system, then we are not really talking about anything at all. If there is such a system, its existence might be made known to us by display: “such and such, with such and such definitions, is an instance of a system that fulfills axioms 1-3.” Would we then merely glance at this statement and agree with a nod of the head, or does the statement require formal verification that a purported model of a system is, indeed, a model? Again we have been driven into a deeper level of verification.

The way out of these difficulties is to give up the needless and useless goal of total rigor or complete formalization. Instead, we recognize that mathematical argument is addressed to a human audience, which possesses a background knowledge enabling it to understand the intentions of the speaker or author.

In stating that mathematical argument is not mechanical or formal, we have also stated implicitly what it is—namely, a human interchange based on shared meanings, not all of which are verbal or formulaic.

Closure

Let us conclude. The myth of totally rigorous, totally formalized mathematics is indeed a myth. Mathematics in real life is a form of social interaction where “proof” is a complex of the formal and the informal, of calculations and casual comments, of convincing argument and appeals to the imagination and the intuition.

The competent professional knows what are the crucial points of his argument—the points where the audience should focus its skepticism. Those are the points where he will take care to supply sufficient detail. The rest of the proof will be abbreviated. This is not a matter of the author’s laziness. On the contrary, to make a proof too detailed would be more damaging to its readability than to make it too brief. Complete mathematical proof does not mean reduction to a computer program. Complete proof simply means proof in sufficient detail to convince the intended audience—a group of professionals with training and mode of thought comparable to that of the author. Consequently, our confidence in the correctness of our results is not absolute, nor is it fundamentally different in kind from our confidence in our judgments of the realities of ordinary daily life.

References

Math Lingo vs. Plain English: Double Entendre

Once upon a time, when I was a teaching assistant, teaching a class of the kind mockingly called “Math for Poets,” an obnoxious freshman said to me, “Zero isn’t a number.”

I have forgotten my answer, but I remember finding her remark a shocking expression of profound ignorance.

Years later, it dawned on me—she was right! If I say “I own a number of calculus books” or “I have a number of friends at the Courant Institute,” I don’t mean zero books or zero friends. I don’t even mean one book or one friend. I mean two or more. That’s what “number” means in plain English. I read recently that the famous phenomenologist Edmund Husserl meant by “number” something greater or equal to 2. So did Plato.

In mathematical talk, “number” has several meanings. None is the plain English meaning. The ordinary math teacher, like me back then, is so deeply embedded in math lingo that he/she doesn’t notice the inconsistency. But the inconsistency can confuse students.

I say “math lingo,” not language. It’s a jargon, a semidialect of English (or some other natural language), not a complete language. You can’t say “I have a headache” or “You bore me” in math lingo.

In math lingo, a straight line is the simplest example of a curve. In plain English, quite otherwise: a straight line isn’t a curve, and a curve isn’t a straight line.

In English, what we call a “line segment” is just a “line.” What we call a “line” is “an infinite line.” “Difference,” “product,” “factor,” “prime” all have different meanings in plain English and in math lingo. I may ask a student, “If you subtract zero from zero, what’s the difference?” While answering math-linguistically, “zero,” she may be thinking, plain-Englishly, “That’s right! Who cares? What’s the difference?”

In English, “adding” increases what you’ve got. In math lingo, it may increase it, decrease it or neither, depending on whether you happen to be adding something positive, negative or zero.

Correspondingly, subtracting decreases. In math lingo, it may decrease or increase or neither.

In English, “adding” and “subtracting” are opposite. In math lingo, they’re opposite, and yet they’re the same! For adding a number is the same as subtracting some other number (its negative).

In English, “multiplying” means repeated adding. It makes things bigger. In math lingo, multiplying makes them bigger, smaller, or neither, depending on what you multiply with.
Correspondingly, “divide” means cut into pieces, possibly equal pieces. In math lingo, “divide” is the same as “multiply,” in the sense that dividing by a number other than zero is the same as multiplying by some other number (its reciprocal).

There’s a familiar conundrum about amoebas: amoebas multiply by dividing. To untangle this nonsensical but correct statement, you must see the difference between the mathematical and the plain English meanings of “multiply” and “divide.”

What should you do about all this? Be aware of it and point it out to students. By appropriate examples, make them realize that what they hear in class or read in the text is technical jargon, not plain English. Otherwise, when they try to remember what you said in yesterday’s lecture, they may remember it with the wrong meaning (the plain English).

Anneli Lax reminded me of one of the commonest linguistic pitfalls: the little one-letter word “a.” Her example is “Show that a number divisible by 6 is even.”

No seasoned math teacher is surprised to receive the wrong answer, “42 is divisible by 6. 42 is even.” Why is this answer wrong? 42 is divisible by 6, and 42 is even. What’s wrong is that the question has been misunderstood. By “a,” the questioner meant “every”; the student misinterpreted it as “some.” This is a quantification problem, which in principle could be cured by using symbolic logic instead of English. But in a case like this, something deeper is wrong. The student should realize that with the interpretation “some,” the question is too trivial to be on the test. Grounding in the context saves the student from most verbal pitfalls. One goal of teaching is to ground the student in the context. Linguistic ambiguities can hurt.

In logic, the pitfalls of “or” and “implies” are familiar.

Take “or.” In plain English, “Tea or coffee?” means one or the other, not both. It’s called the “exclusive or.”

“Are you coming or going?”

“Was that your husband or your boy friend?”

“Do it now or later?”

All are exclusive. It’s hard to think of a colloquial example of the other “or,” the inclusive one. A reasonable example might be, “Like a hug or a kiss?”

In logic, “or” is inclusive by convention. “A or B” is true if A or B or both is the case. I think it’s customary to explain on the first day of elementary logic class that logicians have decreed “or” to be inclusive. A student can accept that logicians felt they had to pick one or the other. Perhaps they had a reason for picking the inclusive.

Peter Lax tells about the famous logician Abraham Fraenkel, of German origin and Israeli residence. Once in Jerusalem or Tel Aviv he was on a bus scheduled to leave the station at 9 A.M. At 9:05 the bus was still sitting in the station. Fraenkel waved a bus schedule at the bus driver, who asked, “What are you, a German or a professor?” Fraenkel inquired in return, “Do you use the inclusive ‘or’ or the exclusive?”

“Implies” is worse. In plain English, “A implies B” means that if A is true, B must be true. If A is false, the “implies” statement is vacuous, neither true nor false.

But in logic, the “law of the excluded middle” insists that every statement be either true or false. The statement “A implies B” has to be either true or false,
even if A is false. Logicians chose “true.” So in logic, if A is false, then A implies B, \( \text{whatever } B \text{ may be} \). This is so unintuitive, I say logicians should have used another word, even made up a word. It’s too late for that. But the student is told that “implies” in logic is different from “implies” in plain English. In pre-calculus, calculus, and post-calculus, we should be equally considerate to warn of linguistic traps.

I have just carelessly used “equally.” “Equal” is used freely, from kindergarten to postgraduate. It’s never defined or explained.

In plain English, its meaning varies. Sometimes it’s “identical, indistinguishable.” Sometimes it’s “worth the same number of dollars.” Or “just as good” for some purpose.

Math lingo sometimes says “equal,” sometimes “equivalent,” the latter if an equivalence relation has been defined. Then we explain that an equivalence relation is Reflexive, Symmetric, and Transitive; it defines a partition on a set.

But what does \( \text{equal} \) mean? When we say \( 1/2 = 2/4 \), we don’t mean \( 1/2 \) is indistinguishable from \( 2/4 \). They have different numerators. They have different denominators. We regard them as \( \text{equivalent} \) for good and sufficient reasons. All this may be explained in an advanced course, on the rare occasion when a detailed construction of the rationals is carried out. But already in the fourth grade the = relation is an equivalence relation between fractions, not an identity. No one ever explains this, so there’s no way for the student to understand =, except in terms of models like slices of apple pie.

This nonunderstanding was manifested frighteningly when a calculus student was asked, “What is the minimum of the function

\[
y = x^2 + 2x + 5
\]

and answered “correctly”

\[
x^2 + 2x + 5 = 2x + 2 = -1 = 4 \text{ minimum}
\]

Maybe this is the outcome of years in high school spent factoring, multiplying, and dividing expressions that always remained equal.

In plain English, set and group are synonyms. When we teach groups, we define set and group, then charge ahead. But some students wonder, “What’s the difference? A group is the same as a set.” Mention this plain English equivalence, and state explicitly that in math these words have different meanings.

The same is true of sequence and series. Their plain English meanings are the same—what in math lingo we call “a finite list.” “Series” is more colloquial than sequence—for example, it’s the World Series, not the World Sequence! Here the danger of confusion is more serious than with set and group. The mathematical meanings of sequence and series are so close that the distinction between them is crucial. In teaching series, we should acknowledge that we’re giving a new meaning to a common word: putting + signs instead of commas between the terms.

The first day of first-semester calculus I like to talk about driving to Santa Fe. Distance from Albuquerque is a function of time. Speed is another function of time. But what is “function” in English? If you ask, “Of what is the speed a function?,” you’re told, “It’s a function of how much gas you give” or “Of how hard you push the accelerator pedal.” “Function” in English (apart from the irrelevant reference to weddings and Bar Mitzvahs) involves causal dependence. “How fast you learn is a function of how hard you study,” for example. How can anything
be a function of time? But the students swallow that. They understand a graph with a time axis. Then I say, “Distance is a monotonic increasing function of time, so the inverse function exists. Time is a function of distance.” How can time, the independent, uncaused variable, be caused by distance? We try to teach our technical meaning of “function” without noticing the meaning the student brings into class.

We're aware that “limit” and “converge” are deep concepts. We sweat over them. But we don’t acknowledge the complication caused by plain English. A “limit” in English is a barrier, a boundary beyond which one may not pass. This may partly explain why students want to approach a limit only from one side, not in alternating fashion. As for “converge.” In practical computation, an algorithm converges when it settles down to one value and stays there—stays till whoever’s doing the calculation is satisfied. That’s the English of converge—“settle down” “close to” some “limit.” In teaching our uncomputational, abstract meaning of “converge,” we should talk about the colloquial meaning and explain the difference.

In advanced mathematics, there’s more linguistic confusion. Surds (absurd), irrational and imaginary numbers, singular perturbations, degenerate kernels, strange attractors—all sound dangerous, undesirable, things to avoid. Yet a degenerate kernel or a singular perturbation may be more useful than a nondegenerate or regular one.

We also talk about “function spaces.” The points in a function space are functions. But a function is a graph—a curve. How can a curve be a point? A point, which has no parts! We don’t acknowledge the change of meaning. Just give a definition and two examples, then charge ahead.

An example of the opposite kind (due to Peter Lax) is “simple curve.” Draw a confusing tangle that doesn’t intersect itself. It’s complicated. We say it’s simple.

What about “partial?” A partial order isn’t a special kind of order. A partial differential equation isn’t part of an ordinary differential equation. And an ordinary differential equation may well be extraordinary.

Exercise: (a) give the plain English meaning of prime; differentiate; integrate.
(b) check your answers against a standard dictionary.
(c) make up three slogans, one using each of these three words, that could appear on picket signs at a demonstration.

It’s fortunate that some double meanings are so far apart they can be used for a joke. A manifold is part of an automobile engine (I think), and a commutator is part of a direct current electric motor.

Acknowledgments

Veronka John-Steiner, Anneli Lax, and Peter Lax gave suggestions and encouragement. [1] is an inspiring example of frank talk about college math teaching.

References

Independent Thinking

Lisa had asked me to do an admissions interview. The twins were applying for the seventh grade. The Academy doesn’t usually like to take home-schoolers without being really sure they are ready for a more conventional setting. Ludwig and Imre had each scored 99.5% on arithmetic, but there was one kind of funny response.

The two boys looked like the victims of a strict upbringing. They were wearing jackets and ties, and respectful expressions.

R: Why do you want to come here to the Academy?
Ludwig: Our mother thinks it’s time we learn to mingle.
Imre: It’s supposed to be better than public school.
R: OK, and how do you feel about it yourselves?
Imre: Fine.
Ludwig: Sure. It’s OK.
R: All right. Do you like math?
Imre: It’s fine.
Ludwig: The test was easy.
R: You’ll find that math gets harder as you go along.
No response.
R: You both missed the same question on the test. Do you remember this one: 2, 4, 8, 16, ?
Ludwig: Yes, I remember that. It was easy too.
R: You answered, 16.
I: That’s wrong. I got the right answer.
R: You answered, 2.
I: Yes, that is the answer.
R: No, you were both wrong. The answer is 32.
I: How do you know?
R: How do I know? I’m the math teacher here!
L: Well, 16 really is the right answer.
R: No, I’ll explain it to you. Starting with 2, you double each number. 4 is twice 2. 8 is twice 4. 16 is twice 8. So the next number is 32, because that’s 2 times 16.
L: All right. I see how you get that.
R: So now, if we want to continue one step further, what would the next number be?
L: It would be 32.
I: No, no, the answer is still 2!
I was stunned. For a minute, I was tempted to shout. But I didn’t. I took a breath, forced a smile and said
R: All right, Ludwig. Why do you think the answer is 32?
L: Because 32 is big enough, you can stop there.
I: You dope! Ludwig, can’t you see that you don’t just stop, you start over when you come to the end?
R: All right. You each have an explanation, kind of. Ludwig, you think that whatever is the last number you’re given is big enough, you can just stop there. Is that right?
L: I don’t know. What’s wrong with that?
R: And Imre, you think that the last number in the sequence is a signal to go back to the beginning. Is that right?
I: Well, you can’t just stay in the same place forever, can you?
R: But what’s wrong with going on doubling each time?
L: That’s OK, you can do that if you want to.
I: Sure, no problem.
R: Thank you. I’m glad you admit that it’s OK to say 64 is the next number.
L: Why not?
R: Let me ask you this. You realize, don’t you, that you can always go on doubling as far as you like?
I: You can? How do you know?
L: It seems that after a while you would get tired and give up.
R: Well, that’s true, I guess. I mean, in principle.
L: What’s the principle?
I: Yes, teach us your principle.
R: That’s the principle. You can always go on.
L: You mean, you can always go on because you can always go on.
R: Are you trying to be funny?
I: No, he never tries to be funny, Mr. Hersh.
R: Good. I want you to think independently, but don’t try to be funny. No response.
R: Forget about doubling. Can you count?
I: Sure. 1, 2, 3, 4, 5, 6, 7…
R: OK. Very good. Now you see that there’s no end to counting, don’t you? You can always go on? You can always add one more?
L: Well, what do you mean, always?
R: It doesn’t matter what I mean, always. You can just always add one more!
I: Did always ever happen yet?
R: You’re getting smart again. No response.
R: Look, everyone knows you can always add one more. It’s obvious. How come you didn’t learn that at home, long ago?
L: We never talked about always. We could ask our mother about it tonight.
I: No, she would just say, decide for yourself. It’s up to you.
R: Well, that’s good. You should learn to think for yourself. Don’t take anybody’s word for things. Be independent and critical.
L: All right.
R: So look, you know decimal notation and place value, I can see that from your admission test. No response.
R: Right? You know you can just add a zero at the end, that’s the same as multiplying by 10? And you know how to add, so you can always add a 1 to any number?
I: Yes, we know that. It’s easy.
R: Well, then, you must see that you can always go on, you can always add 1, or even multiply by 10.
L: Well, that’s all right if you say so.
R: No, no not because I say so! Think for yourself! Can’t you see that it is so?
I: Yes, we know that. It’s easy.
R: Well, Ludwig, what you say is true, but you’re just not getting the point. Getting tired or dying or running out of paper isn’t math. It’s biology or sociology or whatever you want to call it. We’re doing math here.
L: Does that mean math says you can always go on because that’s what math says?
R: Right! Right! You’re getting it, finally.
I: Where is that written? Is it in some book?
R: No, it’s not in any book. It doesn’t have to be in any book, because everybody knows it. Only because you haven’t been in regular school, you are just finding out about it now.
L: What if we asked some other math teacher? Would they say the same thing?
R: Absolutely. Every math teacher in the world will say the same thing.
I: How do you know?
R: Because otherwise they wouldn’t be allowed to teach math.
L: No response.
R: So, let’s go back to the beginning. What’s the next number? 2, 4, 8, 16, 32, 64…
L: No response.
R: If you want to get into the Academy, you better answer.
L: Maybe I’d like public school better.
I: I don’t know. This is harder than I thought it would be.
R: Come on, you know the answer is 128.
L: I know that’s what you want me to say.
I: Yes, that’s true. We have to know what you want us to say, and say that.
R: No, you still don’t understand. It’s not what I want. It’s really the right answer. You know 128 is really the right answer.
I: The right answer is the answer you want.
R: Well of course I want you to give the right answer! I’m the teacher!
L: No response.
R: Well, we’ll let your mother know about admission to the Academy.
I: I was really glad I had the chance to interview them. I sure don’t want them in any class of mine. In public school, they’ll just be told to say what they’re told to say. At the Academy, we insist on independent thinking.
The “Origin” of Geometry

The German phenomenologist Edmund Husserl wrote a famous essay, “The Origin of Geometry” that called for a new kind of “historical” research, to recover the “original” meaning of geometry, to the man, whoever he was, who first invented it.

It seems to me not that hard to imagine the origin of geometry. Once upon a time, even twice or several times, someone first noticed some simple facts. For example, when one stick lies across another stick, there are four spaces that you can see. You can see that they are equal in pairs, opposite to opposite. It happened something like this, perhaps at some campfire, 20 or 30,000 years ago.

In the dead ashes are lying two sticks, one across the other.

Ancient #1: Look at that! Do you see that?
Ancient #2: What? See what?
1: Those two sticks. How they cross—see, they make four spaces. Two big ones, two little ones. The big ones are across from each other on the sides, and the little ones are across from each other, on the top and bottom.
2: So what? (Kicks one of the stocks.) Now what happened to your four spaces?
1: You changed them around. Now the top and bottom are bigger, and the ones on the side are littler. There are still four spaces. And you still have little facing little, big facing big.
2: Yes, that’s the way it is now.
1: Turn them any way you like, you always get four spaces, and they are equal in opposite pairs.
2: I don’t believe it.
1: How can’t you believe it? Can’t you see it?
2: Just watch now. I turn the top stick, little by little. The top and bottom spaces get smaller, the side spaces get bigger.
1: All right.
2: What if I stop now? Where are your big and little spaces now, Mr. Smart Aleck Wise Guy?
1: You stopped before they could switch around. Before the little spaces became bigger than the big ones and the big spaces became smaller than the little ones.
2: Yes, that’s what I did. That shows you’re way off, you’re screwed up.
1: When the sticks cross now, they make four equal spaces. That’s a special interesting way to make two sticks cross. I like that. You did something good.
2: Let’s go chase a rabbit and eat it.

Something like this must have happened more than once. Someone noticed something interesting about a couple of sticks, or bits of straw, or crossed fingers. Something that has to be so, whether you want it or not. An invariant. A geometric fact.
After a while, a name is invented for those four spaces where the sticks cross. “Angles.” Two crossed sticks, or fingers, or forearms, make four angles. The opposite angles are equal, two by two. Some word is invented for the general case. “Line.”

The little anecdote becomes profound, if we raise it to the level of ontology. Was it always true that two lines intersecting in a plane divide the plane into four regions, pairwise alternately congruent? Is that a fact about the Universe? Was it true before there were any fireplaces or Ancient People, before there were thoughts at all?

This revives an argument I have had with two friend-opponents, both of whom happen to be named “Martin.” I take the liberty of representing them in an interlocutor named “Merton.” Merton is wrong, but he is very smart. I will do full justice to his intelligence, his persistence, and his commitment to Platonism. (My arguments with the two Martins were about the facts of arithmetic rather than the facts of geometry. The basic issues are the same.)

**Merton:** Of course that simple theorem is true. It’s a true fact about lines in the plane. You recognize that it’s true, and then you see how to prove it. It was true before you noticed it, and it will be true after there’s no one around to notice it or prove it.

**Reuben:** Let’s look at the sticks in the fireplace, or, if you’re more comfortable, let’s look at the diagram in my geometry book. What do you see?

**M:** What you just explained with your simple-minded story. Two intersecting lines divide the plane into four parts, etc., blah blah blah.

**R:** Is that really what you see? Isn’t that really your interpretation of what you see?

**M:** What do you mean, really? Really what?

**R:** Do you even really see a plane?

**M:** Well, if you want to be literalistic, of course I see a piece of paper, a page in a book.

**R:** Yes. And there’s a diagram on the page. Describe it.

**M:** Two lines intersecting at an angle.

**R:** Lines? Really lines?

**M:** Well, no, of course they’re only segments of lines. That’s all you can draw on the page.

**R:** Then how can it be that you see the whole plane being divided into four parts?

**M:** What I mean is, the diagram is meant to represent the two infinite lines, which extend without limit, even though they can’t be drawn on the page.

**R:** That’s right. I agree. That’s what the diagram is meant to represent. We understand the diagram, and what it’s meant to represent. However, I know some hardheaded people who like to give sensible people like you and me a hard time. One of them is a promising graduate student, Melissa. I know what she would say about this.

**M:** Do you? And what is that?

**R:** She’d say she sees a kind of cross, which could be shrunk or extended, but would never divide the plane into separate regions. It just makes a hole in the middle, you can always go around it.

**M:** Oh. Well, if you want to be difficult, you can always misinterpret anything.
R: Right. So you and I agree that Melissa is just being a pest. We understand what the diagram is meant to represent, an infinite plane in which two infinite lines intersect, thereby dividing that infinite plane into four wedges, which are two-by-two congruent.

M: Right. Of course, it’s obvious.

R: And this fact on which we agree is a fact about what?

M: About this figure, this diagram.

R: No, not really. Melissa has you on that.

M: OK. It’s a fact about the correct and intended interpretation of the diagram.

R: Now, an intended interpretation, as you well put it, is what kind of a thing?

M: What do you mean? Well, it’s abstract, it’s not physical.

R: Isn’t intending and interpreting a kind of activity that is engaged in by creatures like you and me?

M: Oh, I see what you’re getting at. You think you’re leading me into a trap where I’ll be forced to admit that mathematical facts like this one are referring to shared thinking or understanding of human beings, rather than to objective properties of entities that exist independently of human thought.

R: I wouldn’t call it a trap. Just try to think about what we’ve been saying, and what it means in terms of what some philosophers like to call “ontology”. The question of what exists, or, if you prefer, what we’re talking about when we talk about something or anything.

M: No, it’s a trap, you know exactly what you’re doing and where all this has been leading.

R: Don’t upset yourself, Merton. This should just be a friendly chat about topics of mutual interest.

M: Well, I really don’t have a lot of time for this kind of thing. I should be getting back to work.

So I imagine the conversation going. But I may be unfair. I may be wrong about Merton’s willingness to face up to the meaning of mathematical talk. Let’s scratch out the last bit of dialogue, and continue in a serious and thoughtful way.

M: It doesn’t make a difference how we talk or what words we might use. Of course there is an intention and an understanding involved in looking at this diagram. There is an objective reality which cannot be diagramed directly, but only suggested by incomplete but correctly understood diagrams. Such is the case here.

R: That’s interesting. We see two line segments crossing, and we know that we could interpret them as parts of two infinite lines. Is that right?

M: Of course it is. You know it is.

R: Just as we know that the lines are not supposed to have any thickness, even though in the actual drawing we couldn’t see them unless they had a positive thickness?

M: That’s more or less the same issue.

R: And the segments are supposed to be perfectly straight, even though if we were fussy enough with super-accurate measurements we might detect a little curvature?

M: Yes, yes, right, right.

R: If what we see were really a small part of a much bigger picture, it could be that in the bigger picture it turned out that the two segments we see are parts of curved lines, lines that might even intersect again some place far away?

M: Why would you want to suppose anything as weird as that?
R: I'm trying to clarify and examine the way we have this correct understanding. And are you sure it is correct? You know, the Earth isn't really flat.

M: Yes, so I've heard.

R: So—

M: Don't say it! I know what you're trying to do. You're trying to tell me that if we actually did extend these two lines as physical marks here on the surface of this earth, they would intersect again on the opposite side. Right?

R: Well, I might have been thinking that way. But since you anticipated me, I can be even more ridiculous. How do I know that these two line segments are actually, as you now propose, arcs of great circles? Why not small pieces of almost anything? If I am sure they can be extended indefinitely, how do I know how they should be extended? There's no limit to the complicated and peculiar ways they could be extended, here on the surface of this earth, or even in that infinite plane which we both understand was intended to be the meaning of this diagram.

M: Well, what is the point of creating confusion? The meaning of the diagram is perfectly clear.

R: I agree. What makes it so clear? Not that there's only one way to interpret it. Melissa's way would really be the way the diagram was intended, if it was in a complex-variable or a topology text, illustrating a punctured plane. Of course, you have to know whether you're looking at a topology book or a book on plane Euclidean geometry. Even a book on classical plane projective geometry would imply a different interpretation of the diagram. The diagram by itself doesn't force one correct interpretation on us. Yet of course you're right, there is one correct interpretation, and I suppose hardly any geometry teacher or student fails to make that interpretation. That's what we do. So do I, and so do you, Merton.

M: You insist on subjectivizing and relativizing object reality. I suppose there's no way I can stop you from doing that. And why would I even try to stop you? Go right ahead, be as subjective as you like. The objective mathematical reality isn't damaged by that. Only anybody you manage to confuse with your pointless sophistry.

R: Try to keep calm, Merton. It's just a friendly chat. Do you remember the old junior high-school conundrum, "If a tree falls in the forest and nobody is there to hear it, was there a sound?"

M: Yes, I'm afraid I have to admit I do remember that one.

R: It's easy to answer, isn't it? Tell me if you agree with my answer.

M: Sure.

R: The only reason there is any difficulty is the failure to explain what you mean by "a sound". You might mean my subjective sensation of hearing anything—a noise, a voice, or whatever, and call that experience of mine or that sensation, "a sound."

M: Right, you could possibly do that.

R: On the other hand, if you have had high-school physics, you might remember that in physics sound refers to vibrations propagated as waves through various media, possibly the atmosphere, or through a solid if you hear people talking on the other side of a closed door, and so on. That's also a way people use the word, "sound." Right?

M: Definitely right.
R: So the puzzle about the tree falling in the forest is no puzzle. There is no sound in the first sense of the word, there is a sound in the second sense.
M: A masterful exposition.
R: I know you’re being sincere, not sarcastic, so thanks.
M: You’re suggesting a similar ambiguity in the nature of mathematical reality.
R: No, you’re suggesting it, and I’m denying it.
M: How’s that? When did I do that?
R: You’re aware that mathematical notions or conceptions exist in people’s heads or their thoughts or their conversations or their writings. You haven’t said so, but of course you know it is so.
M: How could I not know that?
R: Right. That corresponds to the notion of sound as subjective sensation.
M: I see where you’re going.
R: And you’re just saying that apart from that, there is the objective mathematical reality, just as there is the objective physical event that is also referred to as sound.
M: Yes, I would accept that analogy.
R: And I say it’s a false analogy.
M: What’s false?
R: Simply that there is no observable, identifiable, locatable, describably inhuman objective mathematical reality which you or anyone can show me, that would correspond to the sound waves of acoustics.
M: What do you mean? You know that it’s objectively true that two intersecting lines in the plane cut it into four parts. You could even make a physical model of it, and experience the separation physically by putting up two very long intersecting fences. What do I have to do to make you see that that’s a fact about lines in the plane, independent of whether anyone ever notices it or says it?
R: I fully agree. It is such a fact. But the lines in the plane are simply our mutually agreed on, intended and understood interpretations of this diagram. They’re an idea that we understand very clearly, and can talk about with complete agreement. And that idea or conception indeed has objective properties, in the sense that many other clearly understood, agreed-on mutual ideas do have objective properties. Like the facts of law, of music, of literature, of proper behavior in public places in the U.S. in the year 2002. There are lots of facts about such things, and they are real facts, objectively and have objective properties. They are what we obviously see and experience them as—mutual understanding. There’s no need and no basis in logic or science to imagine that these ideas and understandings can or did exist without us as a society, a culture, a profession, to understand them.
M: You mean that if somewhere else in the universe at some time in the very remote past or future, two lines intersect in the plane, they wouldn’t make four pieces?
R: If people, or creatures much like people, had such thoughts, then and there, those thoughts would have the same consequences. That is a big “if”. You can just as well imagine other cultural artifacts reoccurring in your science-fiction imagination. People can imagine as they please. But of course there couldn’t be an infinite plane and two infinite lines in that plane, ever, anywhere, as far as our physical understanding permits. Such things are used in physics to state theories of mathematical physics. They are not phenomena, data, observations, or possible phenomena, data, or observations. Therefore to talk about what would happen if such phenomena were impossibly supposed to occur on Antares or Sirius, way back
when the world was young, is vacuous. What happens when one stick lies on top of another is a physical regularity. We notice it, conceptualize it, and therefore create mathematics.

References


The Wedding

Philosophy: The bride and groom are here, and have agreed to be wed today, in the presence of several witnesses. Let the wedding begin. Logic, speak to Geometry.

Logic: I come to you, Geometry, my beautiful bride-to-be, as your guide and your elder, your counselor and corrector, your only lord and master, to have and to hold, for good or for ill, in sickness and in health, till death do us part.

Geometry: You come to me, Logic, my groom and my husband, as my guide and my junior, my counselor and interrupter, my self-named lord and master, to have and to hold, for good or for ill, in sickness and in health, till the unknown and unknowable, whatever is to come at the end of our joint dominion.

L: Do you, Geometry, take me as your guide and your elder, your counselor and corrector, your only lord and master, to have and to hold, for good or for ill, in sickness and in health, till death do us part?

G: I do, with a reservation.

Philosophy: We will hear your explanation.

G: I am the elder, you are the younger, Lord Logic.

P: Let it be so recorded, Geometry is the elder. Is that your only reservation?

G: It could be. Unless you wish to hear more.

L: You may go on.

G: You bring me strength and control. You bring me your darling and precious offspring, 1, 2, 3, and many other beautiful numbers. I am happy to be your bride.

L: This is well explained.

G: I bring you shape, form, and continuity. The possibility of fruitful intercourse, and offspring uncountable.

L: You know I love you, Geometry.

G: I know you love me, Logic. I do not know if I can love you. You are hard. You are unforgiving. You think you can be my lord and master.

L: Look, is this a wedding, or what?

G: It was announced and scheduled as a wedding. The guests are here, with gifts.

L: Yes, beloved friends and family have come. Mechanics and Statistics, of course. Even the Stock Market, the Census Bureau, and the Atomic Energy Commission are here.

G: Don’t worry about them. They don’t understand anything, anyhow.

L: Easy for you to say. Who will pay the bills?

G: Oh, Logic, you are hopeless. The bills! Is this love or business?

L: It’s the business of love and the love of business.

G: Another paradox! I hate your silly little paradoxes! I hate them! Why did I ever agree to do this?

L: Because you need me.
G: Yes. It’s true. I need you. But why and for what?
L: Without me to look after you, what would become of you?
G: I don’t know. What would become of me?
L: Look at your wretched, lost cousins. Numerology. Weather and Financial Prediction. Old Pythagoras’s great-grand-children. Where are they now?
G: In the Gutter.
L: Yes. In the Gutter.
G: Why are they all in the Gutter?
L: Because they tried to live without Logic.
G: Will you be good to me? Will you be kind? Will you ever be kind?
L: I will be good for you. I will be as kind as I am able to be.
P: Let it be ordered and written that on this day, in this place, in the presence of witnesses both honorable and dishonorable, Logic and Geometry were lawfully wedded, to have and to hold, for good or for ill, in sickness and in health, till death do them part. And may God some day forgive us for what we do here today.

(I believe it was Hermann Weyl who once said that the angel Algebra and the devil Topology were struggling for the soul of Mathematics.)
Mathematics and Ethics

I want to start off by correcting any possible false impression that I’m going to tell you what is ethical, or that I’ve solved any big problem regarding mathematics and ethics, because I certainly haven’t and make no such claim. Of course, the next question you ask is, why am I standing up here anyhow? It’s only because I have thought about the question, and in the process of thinking about it I have had some ideas that I’d like to offer you.

The observation that got me started on this was that in many professional fields there has been for a while a well-established concern with ethics. What that means varies from field to field. But the idea that a professional association of engineers or statisticians might concern itself with ethical behavior in that field is not radical at all. It’s a very standard thing. Often it’s done officially by the establishment. Often there are active concerns on the part of special organizations, editorials in journals, and so on.

One of the first organizations of this type that I had contact with, long before I was a mathematician, was the Society for Social Responsibility in Science. I’m not sure it still exists. In its day, the 1950s and the 1960s, it was primarily concerned with nuclear arms, nuclear warfare, nuclear destruction of the human race. It consisted largely of physicists, many of them Quakers or Quaker sympathizers. They took the position that there was a question of social responsibility, for the physicist particularly, about whether to work on nuclear weapons. Some people refused to work on nuclear weapons or quit military jobs. Whether you agree with that or not, this was a legitimate issue in the physics community [7], [8], [13], [15].

Another example arose with the environmentalist movement. Ralph Nader was an outstanding spokesman. This movement involved biologists and also chemists because chemists do a lot of polluting. Not chemists themselves, but the things that chemists create. There again was a question of social responsibility, which is one aspect of ethics.

I have in my hand two actual codes of ethics. One was adopted by the statisticians’ society, and the other by the professional engineers’ society. These are not so political. They have more to do with proper behavior toward one’s client—ethical issues of that sort. No doubt you could find other examples.

For a mathematician, it’s natural to ask why we don’t seem to be concerned about ethical issues or discuss them? It is true, as many of you know, that recently there was a referendum in the American Mathematical Society (AMS). There was a long, drawn-out political hassle, and in the end five motions were passed by

This paper is based on a talk that was given first to the New England Section of the Mathematical Association of America in November 1987 in Waltham, MA, as the Dan Christie Memorial Lecture, and again in November 1988 to the Southern California Section of the MAA meeting in Claremont.
the membership. The one that is probably most controversial says that the AMS should not involve itself in helping the Star Wars (SDI) activity to recruit among AMS members. That issue certainly has ethical implications. But it was a one-time, ad hoc thing, not an indication of continuing concern or involvement with ethical issues by mathematicians. In my opinion, the reason it became a big issue in the AMS was that there had already developed strong opposition to the SDI among physicists and computer scientists, both in individual departments and in national organizations. I think that was why some mathematicians felt we should also get involved. In the end, after a lot of back-and-forth haggling, the membership approved the anti-SDI motion. So there is an example of an ethical issue that did come before and actually passed the American Mathematical Society. That’s not the main thing I want to talk about. I just mention it because some of you might have it on your mind and might remember it.

The thing that is striking, you see, is that in all the other examples I’ve given—the biologists’ involvement in environmental issues, and the chemists as well, and the physicists in nuclear war, and the statisticians requiring that if you are a good statistician you won’t give away your client’s data—these are all different, but they have one thing in common. They are all in some way intrinsic to the actual practice of the particular profession. The physicists are the ones who make the bombs, the chemists are the ones who pollute, and so on. When I thought about the situation of mathematicians, I found I was oscillating between two different viewpoints. On the one hand, a mathematician is somebody who solves a problem or proves a theorem and, of course, publishes it. And it’s hard to see significant ethical content in improving the value of a constant in some formula or calculating something new—say, the cohomology of some group. You might say it’s beautiful or you might say it’s difficult, but it’s hard to see any good or evil there in the way physicists and biologists, and so on, do have ethical problems. On the other hand, if you step back from that particular way of looking at the role of mathematicians and just think about your own activity or mine, think of what we actually do daily and yearly, there are constant decisions and conflicts involving right and wrong.

The ethical demands of all the scientific groups seem to fall into three categories: What you owe the client, what you owe your profession, and what you owe the public. Now, if you are a mathematics professor, the word “client” may be unfamiliar. Who is the client, anyhow? But there is always a client in the sense of the one who’s paying your salary. The ethics of the statisticians and engineers prominently feature duties to the client. And then there is the profession. What do you owe your partner, your colleague, or your fellow professional? In some ethical codes that’s up at the top. I think that’s the way it is with lawyers. Doing something unethical means treating some other lawyer unfairly. Duty to the public is an afterthought.

Now to the mathematicians. I can list five different categories of people to whom we have duties: staff, students, colleagues, administrators, and ourselves. First are the staff, the people who do the work that we don’t want to do. It would be interesting to think about the situation or treatment of the non-faculty employees of your department. Do you regard it as equitable? If you don’t, does anybody ever try to do anything about it?

Then there are the students. For instance, there is the problem of mathematical illiteracy. I don’t mean to suggest that we owe students mathematical illiteracy.
Rather, the existence of mathematical illiteracy poses an ethical issue. Is the prevalence of mathematical illiteracy among students in part a responsibility of us, their teachers? If so, what can we do about it? This issue needs to be mentioned because so many of us deny our responsibility and blame the high schools. Next example: grading. Again, we don’t usually think of this as an ethical issue. We try to make it a mechanical matter, a rule, and let a machine do it. But despite our machinery, there are always hassles and disagreements about grades. I think that the grade I finally give, whether it’s a number or a letter, is not just an objective application of some rule, but also to some extent an ethical choice. What do I think is more important, more valuable than something else? I would say grading should be included in the ethical life of the mathematician. I’ve had a student from some place in the Near East tell me that if I didn’t change his grade, he’d have to go home and go into the army and get killed, and it would be my fault. For all I knew it might have been true, except I didn’t change his grade and he was still there a year later. That’s an extreme example of an ethical issue: murder associated with grades.

Finally, gender and ethnicity. This has been subject of a good deal of talk in recent years. There are, to some extent, special programs to help women and to help Blacks, Hispanics, and Indians attain a higher level in mathematics. Not many people here, I would guess, are involved in that activity. And there are certainly differences of opinion about it. But it’s a clear case of an ethical issue [2], [6], [10], [11], [12], [18], [19].

Colleagues. This, I think, is the big one, the one that most of us are most involved in. Hiring, tenure, promotion—these are the issues that department meetings hassle about. Sometimes, I suppose, decisions are made entirely on an objective basis of what’s best for the department. And then again, sometimes people help their friends. But before you get down to who gets hired or who gets tenure, there have to be assumptions about what’s important, what’s legitimate, what you want to do. It’s usually supposed that this is already given. Everybody should already know what the department needs to do to improve itself. But actually, that’s not tenable. The standards for hiring, promotions, and so on are subject to differences of opinion, depending on what you believe in and what you think is the right thing for the department to be doing. In other words, your ethical stance.

Here is a story about an ethical problem in relations between colleagues. It’s a little out of date, but interesting. You probably know that back in the 1930s many mathematicians were leaving Germany in order not to be killed. Emil Artin was one of the great algebraists of his time. Artin wasn’t Jewish, but his wife Natasha was half-Jewish, and they had two kids. Artin was approached by Helmut Hasse, who was another outstanding algebraist. Hasse was almost a pure Aryan, though he did have a Jewish great-grandfather. He had become the head of the Institute at Göttingen after Courant and Weyl and Neugebauer had been kicked out. Artin was planning to leave because of his wife’s being half-Jewish, their children quarter-Jewish. Hasse said he could give Artin a deal. The kids could be made Aryan. [14], [16], [17], [20]!

Do you see any ethical questions there? Hasse was a great mathematician. After the war he was quoted as being annoyed that some of the de-Nazification programs instituted by the American army were too severe. And he wasn’t the worst. There were people like Teichmuller, and Bieberbach, brilliant mathematicians who were whole-hearted, all-out Nazis. Their ideology affected their professional work too,
driving people like Landau off the lecture platform. Probably it could never happen here. But racism is a problem everywhere. It's not only a political problem, it's an ethical problem. We tend, many of us, to throw it under the rug, to think it's of no relevance to us. But maybe learning a little history will enlighten us about that. So much for ethical problems between colleagues.

Finally, what do we owe to the Dean, the Provost, the Chancellor? What they usually expect is that you should get grants, visibility, and things like that. That demand from administrators is based on certain values. It's based on a particular idea of what the university is and what the department should become. If those values are accepted, then our present situation follows absolutely. The mathematics department should get out there and bring it in! But this value system is also arguable. There are some of us who think otherwise. And recognizing that there is an ethical conflict here can only help to clarify our possibilities and our alternatives.

Now, to yourself! Does anybody here remember Polonius, in *Hamlet*? Eventually Polonius gets Hamlet's sword through his gut, but he leaves us this memorable line: “And this above all, to thine own self be true.”

So far I've carefully avoided giving you my own values. So there's no way anyone can disagree with me. I've just listed points of value judgment in our profession. I'm sure there are others that I have forgotten. But you're undoubtedly about to point out that all this really has nothing to do with mathematics. It has to do with academic life. A French professor or a mechanical engineering professor would be involved in the same issues. I've been talking as if we're all academics. Of course, this isn't true. Some people here must be working in industry or other things. But being an instructor or professor involves you in all these interactions with people: students, faculty, staff, administration. And these all have an ethical component. However, this does not really deal with the issue I started with, which was what about mathematicians as mathematicians?

Just because we're mathematicians, are there issues we have to face in the same way engineers have ethical issues they have to face? Here I think we are forced to recognize the irritatingly vague line between pure and applied mathematics. To the extent that it is really involved in the so-called “real world,” applied mathematics brings in the same ethical issues as engineering or any other applied science. For instance, nowadays people are using big computers to figure out secondary oil recovery. The people who do this are both geophysicists and applied mathematicians. The ethical issues for applied mathematicians are the same as for geophysicists. What are the consequences of this activity for the environment, for the economy? To the extent that applied mathematicians get involved with a real world activity like geology or engineering, they have to deal with the ethical issues of that field, not because they are mathematicians but because they are involved in that application.

Therefore, let me acknowledge the separation and ask: What about pure mathematics and mathematicians who merely prove theorems? Is there any ethical component comparable to what you find in other fields of science? Of course, depending on what you include as ethics, you can say yes or no. “It's unethical to prove an ugly theorem.” “It's unethical to republish under a different title a trivial paper that you have already published.” As expressions of the taste or the standards of the field, these statements are correct. But still, one laughs at the word “ethical” here. It just doesn’t make sense to use the same language for such issues of taste
in pure mathematics as for air pollution or nuclear war. There are “ethical” issues in pure mathematical research. But they cannot withstand comparison with the major issues of human survival arising in “real world” science.

In pure mathematics, when restricted just to research and not considering the rest of our professional life, the ethical component is very small. Not zero, but so small it’s hard to take very seriously. In fact this may be a characteristic, a defining characteristic of pure mathematics. I can’t think of any other field of which you could say that. That’s why people say mathematicians live in an ivory tower. One answer to this could be, “Well, this is fine! There’s no need for mathematicians to have a code of ethics, because what we do matters so little that we can do whatever we like.” And I might agree with that. I’m not going to start advocating a code of ethics in mathematics at this point. But when I think about this attitude, I find it scary. Because it means that if we become totally immersed in research on pure mathematics, we can enter a mental state that is rather inhuman, totally cut off from humanity. That’s a thing we could worry about a little bit.

Therefore, I come to a conclusion for most of us, those who are not doing pure research a hundred percent of the time or who are not in the institutes for advanced studies, but have students and colleagues and staff and administrators. We mathematicians, I think, have a special need to take all these other responsibilities very seriously. Because unlike people in other fields, our research work does not automatically involve human concern. My conclusion: If our research work is almost devoid of ethical content, then it becomes all the more essential to heed our general ethical obligation as citizens, teachers, and colleagues, lest the temptation of the ivory tower rob us of our human nature.

References


Ethics for Mathematicians

Almost 20 years ago, I noticed that the American Mathematical Society, unlike most professional organizations such as chemists, geologists, statisticians, etc., had no official Code of Ethics. This observation stimulated some pondering on my part, which was vented in a talk to a regional meeting of the Mathematical Association of America, and an article in the *Mathematical Intelligencer* in 1990.

I conjectured that part of the reason such an official code was absent was that, while research in pure mathematics can be good or bad, it is pretty harmless, compared to the possible danger of unethical behavior in, say, nuclear physics or chemical engineering. Such ethical issues in mathematical research as “Don’t steal other people’s results” and “Try to do the best work you can” seemed relatively “small potatoes.” But I thought that this lack of serious human consequences posed a danger for the researcher—a danger of dehumanization. I suggested counteracting this danger by active concern for the welfare of those with whom the researcher interacts—staff, students, and colleagues.

In 1995 the AMS adopted a code of ethics. The committee that developed the Code was chaired by Linda Keen, who was my fellow student at the Courant Institute in New York back in the ’60s.

The Code is divided into four sections. Section I, “Mathematical Research and its Presentation”, is mainly about proper attribution of credit, and avoidance of plagiarism or other improper claims of research results. Section II, “Social Responsibility of Mathematicians”, is mostly about confidentiality of recommendations, avoiding conflict of interest in reviewing and refereeing, and keeping careful records. There is a sentence forbidding discrimination by race, gender and so on. Also, mathematicians should disclose any dangers to public health or safety, and should not be exploited by temporary positions at unreasonably low salaries or heavy work load. Section III, “Education and Granting of Degrees”, says Ph.D.’s should be granted only for proper cause, and without plagiarism. Section IV, “Publications”, calls for prompt refereeing and respect for confidentiality.

Linda wrote to me, “I don’t know how successful the guidelines have been. That is, are they read by anyone? Has anyone used them to show a chair or dean in dealing with a problem? Are junior faculty given a good basis on which to think they will be tenured? Are editors and referees more careful about being timely? Do people keep confidence on privileged knowledge such as what someone is working on because they saw it in a proposal or as a referee? There are lots and lots of other questions one can ask about what is or isn’t ethical behavior.”

Developing a Code of Ethics for the AMS was a good thing to do. But it doesn’t confront the ethical conflicts I saw in my life as a mathematician. Many issues voted on in my department meetings had ethical aspects. Many choices I made day to day, as a teacher and colleague, had ethical relevance. What does it
mean for a faculty member in a mathematics department at a university to behave ethically?

Based on my experience and observation since I entered the USA math world 50 years ago, I see three rival ethical codes to choose between: the Organization’s Code, the Profession’s Code, and the Human Code. (By “ethical code,” I simply mean “a set of rules of desired or approved behavior.”) These rules would be modified to fit other times and places. They apply also to other fields of academia, not only to math professors. But they do apply in particular to math professors. I will state clearly what the three codes are, as I see them.

The Organization’s Code is the demands and expectations of the University Administration (Chairs, Deans, Provosts, representing Regents, Legislature, Board of Overseers etc. The people who control the money.) I list five of their Commandments:

I. Bring in money. (Get grants and support from government agencies, private foundations, wealthy individuals or corporations, to the maximum possible amount, with the Institution (University) receiving its share.)

II. Maintain and improve the institution’s Image. (Get publicity for your research, or at least for the quality of your teaching. DON’T attract attention by doing or saying anything controversial, such as attacking government policy, or disputing popular religious or superstitious beliefs.)

III. Keep the students quiet. (DON’T encourage rebellious activity. DON’T upset them by demanding too much effort, or by frightening them with the expectation of Bad Grades.)

IV. DON’T upset Standards by just giving everyone an A. (This is called Grade Inflation.)

V. DON’T embarrass the University by talking in public about the previous Commandments.

That’s the Organization Ethics. Most math professors don’t like them very much. We prefer our Professional Standards. (Professional Standards are the criteria by which a profession measures the prestige of one of its members.)

First, there’s “Rule Zero”: be alert to the current prestige of individuals, institutions and topics. Four “corollaries” follow:

I. Get your Ph.D. at a prestigious department, under the mentorship of a prestigious professor.

II. Work in a prestigious area of research, on problems of interest to prestigious researchers.

III. Publish soon and often, in prestigious journals.

IV. Avoid low-prestige topics and people.

Despite pious words to the contrary, pedagogy remains low-prestige. “Applications” and “computing” used to be low-prestige, but today “financial mathematics”, for instance, is doing better, prestige-wise. Military mathematics has always been “low prestige” with some prestigious mathematicians, and “high prestige” with some others. “Philosophy of mathematics” is off the charts, like playing the tuba or doing abstract sculpture—an irrelevant hobby.

Perhaps these commandments for maximizing professional prestige seem not to be in the domain of ethics. If you violated them you’d be called “impractical” rather than “unethical”. But choosing to maximize your professional prestige would be part of choosing what you consider a good life. On the other hand, maximizing
your popularity with Chairs, Deans, and Provosts implies a different choice of a good life.

The Organizational and the Prestige codes are partly compatible. Normal academic success depends on balancing the two. But there is another, a third ethical standard, not strictly required for academic success, that has influenced some mathematicians I know. An outstanding example of that third standard was my friend Carla Wofsy. When asked to explain her success as both a teacher and a scientist, she innocently stated two simple rules:

1. Whatever you do, do it perfectly.
2. Do all you can to help the person who is now standing in front of you.

Of course, Carla was somebody special.

But I have known other people in academia who seemed to use her rule, “Do your best to help everyone you encounter. Try not to hurt anyone.”

These three codes of ethics are not necessarily in direct conflict. They seem to be applicable in three different kinds of situations. But they can collide. For example, if a department chooses faculty primarily for prestige reasons, it may not be acting in the best interest of its students. Writing the budget—allocating money—always reflects priorities. The administration’s priorities, the priorities of professional prestige, or human welfare priorities?

When you vote in a meeting to set some department policy, what is the decisive consideration?

“Getting the Dean and Provost off our backs”?

“Increasing our department’s prestige, compared to our rivals”?

Or “Being helpful to the people involved”?

A teacher can help students or hurt them. He may be conscious of them as human beings whom he has the opportunity to help and to serve. Or he may view them just as passive recipients of his lecture. Or he may even regard them as annoying distractions from serious work. His attitude to his students reflects his priorities, that is to say, his values, his ethical choices.

You can even help or hurt the people who answer the phone or who empty your waste basket.

Everybody has ethics. The question is, which ethics do you have?

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Under-represented Then Over-represented: A Memoir of Jews in American Mathematics

Over-represented. When I studied at the Courant Institute of NYU from 1957 to 1962, its Jewish (specifically, Ashkenazi) flavor was impossible to miss. Of course, it was in large part the creation of Richard Courant, who came to New York in 1934 as a Jewish refugee expelled by Adolf Hitler from his post as the leader of the great and famous mathematical school at Göttingen in Germany. Two of NYU’s most important professors, Kurt Friedrichs and Fritz John, had been Courant’s students at Göttingen. Lipman Bers was also a refugee. Of the younger members of the brilliant faculty, Peter Lax (my mentor and adviser) and his good friend Louis Nirenberg, world leaders in their specialty of partial differential equations, were themselves graduates of NYU (and Jewish). More on the applied side were Joe Keller and Harold Grad (also Jewish, also NYU graduates). Jack Schwartz, a New York Jew, had recently come down from New Haven to join the Courant faculty. Martin Davis had been one of Jack’s fellow undergraduates at City College. Morris Kline (Jewish, of course) had actually preceded Courant as an NYU professor. In addition there were Anneli Lax, Warren Hirsch, Jerry Berkowitz, Lazar Bromberg, and Max Goldstein. (What about Wilhelm Magnus, a German like Courant, Friedrichs and John? Only later on did I come to understand that he was not Jewish, but Catholic and a staunch anti-Nazi. He left Germany, not as a refugee from Hitler, but as a postwar immigrant—sponsored and invited by Courant.) And my first boss at Courant, the professor for whom I was a homework grader, had a Jewish last name (Morawetz) and an Irish first name (Cathleen!) I soon came to understand that her father was the well-known Irish mathematician John Lighton Synge, and her husband the well-known Jewish chemist Herbert Morawetz. The only real anomaly here was Jim Stoker—a highly respected geometer and applied mathematician, but apparently a—a what? A WASP (white Anglo-Saxon Protestant)! Yet not such an anomaly. In the mid-thirties Stoker went to the Federal Institute of Technology in Zürich to get a Ph.D. in mechanics. One of the first courses he took was on geometry with Heinz Hopf. He fell in love with the subject and the teacher, and took a Ph.D. in math instead. When he’d finished, Hopf wrote to Courant about this junger Americaner who would fit in very well with Courant’s plans. I never did get to know Stoker, somehow. I did meet, talk with, and take courses from all the others I have mentioned. Yes, you could say Jews were over-represented at Courant in my time.

But after all, the department had virtually been created by a Jewish refugee, at an institution with a student body that Constance Reid describes as “composed largely of the sons and daughters of working-class Jewish immigrants.” I later read, in her biography of Courant, that in deciding to settle at NYU, Courant had
been counting on New York’s large supply of smart youngsters, what he referred to as “a reservoir of talent,” to fill the ranks of NYU’s graduate program, and indeed, at least to some degree, of its mathematics faculty.

So the “over-representation” there seemed perfectly natural.

From NYU, after five years as a grad student, I was lucky enough to spend two years as an instructor in Palo Alto, California, at Stanford University, a great opportunity for me to develop mathematically and to meet mathematicians. First of all, Ralph Phillips, the collaborator of my mentor Peter Lax. Not a New Yorker—a “real American,” you might say, if you think New York is not exactly the real America. But still—Jewish! My supervisor in my teaching duties was Paul Berg—like myself, a Jewish NYU product. Three young hotshots were Don Ornstein, Paul Cohen, and Bob Osserman. My fellow instructors included Si Hellerstein, Steve Shatz, Lenny Sarason, and Rohit Parikh. Rohit is from India, and I don’t think he is Jewish. The chairman at Stanford was David Gilbarg. Yes, Jewish. Gilbarg’s collaborator in nonlinear elliptic partial differential equations was Bob Finn. Sounds pretty non-Jewish. Remember Mark Twain and Huckleberry Finn? But when I got to know Bob Finn, it turned out that “Finn” was a shortening from “Finkelstein.” So what? My own surname, Hersh, was invented by my father, Hersh Fish Laznowski, when he became a U.S. citizen some time around 1921, and decided to become Americanized as Philip Hersh.

In my two years at Stanford, I met some of the department’s very famous faculty of European origin. Most conspicuous, at least to me, was Lars Hörmander, a tall blond Swede who was notorious for being mathematically perfect. Certainly not Jewish. (I didn’t know that one of his mentors and advisers back in Lund had been the Hungarian Jew Marcel Riesz.) Anyway, the other famous Europeans at Stanford were Stefan Bergman from Poland, George Polya and Gabor Szegő from Hungary, Charles Loewner from Czechoslovakia, and Hans Samelson from Germany. There was also Menahem Max Schiffer from Israel and Sam Karlin in statistics. All brilliant; all Jews.

Well, why not? It has been said, more than once, that by driving the Jewish mathematicians and physicists from Europe to America, Hitler gave the U.S. a present more valuable than anything else you can think of. Stanford grabbed more than its share.

When my two years at Stanford were up, I took employment at the University of New Mexico in Albuquerque, where I have pretty much remained ever since. The chairman here was Julius Blum—another refugee. He had escaped from Berlin, and earned his Ph.D., in statistics, at Berkeley. He was close friends with another statistician in the UNM department—Judah Rosenblatt. Judah’s statistics Ph.D. was from Columbia. I was quickly informed of Judah’s other big distinction—his grandfather was none other than Yossele Rosenblatt, the most famous American cantor, whose recordings of Judaic musical liturgy were beloved by many, especially by the Orthodox. Along with Julius and Judah, the department was dominated by Bernie Epstein, author of a popular graduate text on PDE’s, and Ignace Kolodner, another refugee, who also was a Courant Ph.D.! This was far from New York or California, in the semidesert of tricultural (Hispanic, Native American, and Anglo-Cowboy) New Mexico.

Jewish over-representation!
Over-representation? Why? How come so many Jews? A natural question, but one seldom asked in conversation, and never asked in print. Too ticklish, too much chance to be misunderstood, or give offense, or get in trouble one way or the other. You don’t want to seem anti-Jewish, you don’t want to seem too Jewish, you don’t want to seem hung up on the Jewish question; much better to just act like you don’t notice something a little unexpected, calling for explanation. Strangely enough, even in historical articles about the immigration of European refugee mathematicians to the U.S. in the 1930’s, the words “Jew” and “Jewish” are usually avoided. Now Ioan James, a leading homotopy-theorist who held the famous Savilian Chair at Oxford once held by G. H. Hardy, and since retiring turned to mathematical biography, has published a collection of brief biographies concentrating on Jewish mathematicians and physicists \[4\]. James’ book, coming out in 2009, 45 years after I first arrived in New Mexico from Palo Alto, is almost the first place I have seen Jewish over-representation acknowledged as an obvious fact of some interest.

What exactly is “over-representation”? Well, I haven’t attempted a head count. For one thing, I don’t want to get into the question, “Who is a Jew?” Carl Gustav Jacob Jacobi, Gotthold Eisenstein, and Leopold Kronecker are always listed as the first important Jewish mathematicians, yet all three were Christians—that is to say, they all three underwent conversion or baptism. A rabbi would say, “Jewish means son or daughter of a Jewish mother.” Of Courant’s two protégés at the Courant Institute, Kurt Friedrichs was “Jewish” only by marriage to a Jewish wife. Fritz John was “half-Jewish” (on his father’s side). In the simplistic view of the general population, they were both somehow “Jewish refugees.” Hermann Weyl, Hilbert’s greatest pupil, was also “Jewish” only by virtue of his marriage to a Jewish woman. Cathleen Morawetz, of course, is really Irish, regardless of her Czech Jewish husband and married name. The famous topologist Mary Ellen Rudin at the University of Wisconsin is another daughter of gentiles with a Jewish name by marriage. (She was born and raised in rural Texas, and turned into a mathematician by Robert Lee Moore himself.) And what about the famous “refugee” algebraist, Emil Artin? He had no “Jewish blood,” but he was married to the half-Jewish Natascha. One of the oft-repeated stories of Nazi idiocy is of the offer Artin received from Helmut Hasse (Courant’s successor as head of mathematics at Göttingen). To convince Artin not to leave Göttingen for the U.S. in 1934, Hasse actually offered to have Artin’s quarter-Jewish children declared officially “Aryan”! (Of course, no such offer could be made to their half-Jewish mother.) (By the way, Hasse, a German nationalist who comfortably served under the Nazis, was actually, secretly, contaminated by the blood of a Jewish forebear; in fact, he was distantly related to the composer Felix Mendelssohn. Hasse was very proud of that before the Nazis, but tried to hide it after they came into power. Carl Ludwig Siegel, a colleague and a staunch anti-Nazi, always referred to him as Herr Hasse-Mendelssohn.)

Of course, it’s not just America. As a grad student of Peter Lax, I became aware of the “Hungarian miracle,” meaning, the amazing number of first-class Hungarian mathematicians in the 1920s and ‘30s. The list would start with the brothers Marcel and Frigyes Riesz, the collaborators Gabor Szegő and George Polya, the “titled” John von Neumann and Theodore von Karman, and Lipot Fejer, Rozsa Peter, Michael Fekete, Paul Erdős, Paul Turan, Alfred Renyi, Arthur Erdelyi, Cornelius...
Lanczos. No one mentions the very strange fact that every single one of them was Jewish! (Nowadays, of course, not all great Hungarian mathematicians are Jews.)

Or look at Italy, whose Jews are Sephardic, not Ashkenazi. We Ashkenazi don’t recognize Italian names as Jewish. When Ascher (later to be Oscar) Zariski left the Ukraine, he went to Rome to study algebraic geometry with Guido Castelnuovo, Federigo Enriques, and Francesco Severi. Two out of the three were Jews. (Severi, the non-Jew, would later disgrace himself as a collaborator with Mussolini’s fascism.) And there are more Italian Jews in mathematics: Giulio Ascoli, Vito Volterra, Guido Fubini, Luigi Cremona, both Corrado and Beniamino Segre, and Salvatore Pincherle. (I am told that in Italy, a surname that is also a place name is an indicator of Jewishness.) Not to mention Beppo Levi and Tullio Levi-Civita.

So counting American Jewish mathematicians is a hopeless task for several reasons, of which the lack of a definition of “Jewish” is only one. Nevertheless, if we don’t insist on numerical precision, I take it as obvious and uncontroversial that the proportion of Jews among American mathematicians has been, in recent decades, much greater than the proportion of Jews in the U.S. population as a whole. That is what I mean by “over-representation.”

What is not usually mentioned is the remarkable contrast with the situation earlier—before World War II.

**Under-represented.** There were really only four prominent Jewish mathematicians in the U.S. before World War II—James Joseph Sylvester, Norbert Wiener, Solomon Lefschetz and Salomon Bochner. Is that not strange? Huge hordes of Jews in the ‘50s and after—almost none in the ‘30s and earlier (apart from refugees arriving after 1934.)

Sylvester, of course, doesn’t really count. He was English, not American. As a victim of English anti-Jewish discrimination, he came to the U.S. twice. He came at the very beginning of his career to the University of Virginia, where he was victimized as a Jew, a foreigner, and a suspected opponent of slavery, and forced to flee in fear of his life after an altercation with a “student” [8]. Then, much later, as a famous algebraist, he was recruited to create the first real math department in the U.S., at Johns Hopkins. After a few fruitful years there, he went home to receive, finally, his rightful position at Oxford, as Savilian professor.

Norbert Wiener was MIT’s first, and for a long time almost the only, American Jewish mathematician. From his autobiographies [13, 14] we learn the astounding fact that he didn’t learn he was Jewish until he was grown up, and then found out that he was possibly descended from the great and famous Jewish physician of the golden age of Arabic rule in Spain, Moses Maimonides. Norbert’s father Leo was a Harvard professor of languages, and Norbert felt strongly that his own mathematical attainments entitled him to a position in the mathematics department at Harvard. However, that department never hired a Jew in a regular faculty position, until Oscar Zariski was hired in 1947. Why not Wiener? Well, there may have been more than one reason. Wiener was certainly a great mathematician, but he was also insecure, neurotic, alternately pretentious and apologetic, near-sighted, and rotund. Wiener himself was sure that the obstacle to his getting an offer from Harvard was the notorious, unacknowledged anti-Semitic bias of the dominant, most influential Harvard mathematician, the great and famous George David Birkhoff. To be fair, Birkhoff was not equally and uniformly hostile to every single Jew. For
example, he sponsored an invitation to the young Polish Jewish prodigy, Stanislaus Ulam, to become a member of the Harvard Fellows, in 1939 when Ulam was lucky enough to be visiting the U.S. at the time of Hitler’s attack on Poland. Ulam was a banker’s son, and had fine upper-class Polish manners. And ultimately, after the War, when Zariski received the long overdue offer from Harvard that let him escape from the heavy burden of teaching at Johns Hopkins, he was surprised and pleased to learn that Birkhoff had supported his joining the department. In fact, Birkhoff didn’t mind having some Jewish students. There is a solidly authenticated report of a phone conversation between Birkhoff and the chair of the math department at the University of Rochester, where a Jewish refugee had not received the job offer that Birkhoff felt Rochester should have made. It seems Birkhoff assumed, rightly or wrongly, that Rochester’s failure to come through with the expected offer was an expression of anti-Semitic bias, for he was heard shouting over the phone at the Rochester math chairman, “Who do you think you are, Harvard?”

James 4 reports a conversation between Birkhoff and an officer of the Rockefeller Foundation (p. 260) “who noted afterwards, ‘B. speaks long and earnestly concerning the Jewish question and the importation of Jewish scholars. He has no theoretical prejudice against the race and on the contrary every wish to be absolutely fair and sympathetic. He does however think that we must be more realistic than we are at present concerning the dangers in the situation and he is privately and entirely confidentially more or less sympathetic with the difficulties of Germany. He does not approve of their methods, but he is inclined to agree that the results were necessary.’ ”

Well, there’s only one Harvard. What about the other elite Ivy Leaguers?

Yale? In 1947 Nathan Jacobson, the leading algebraist, was the first Jew to make it into Yale’s math department. He wrote about some of his experiences as such, in notes to his collected papers 3. There’s a book by Oren 9 about how the barrier against Jews on the Yale faculty was gradually broken down.

Princeton? That looks better, for the great topologist Solomon Lefschetz joined that department as early as 1924. Lefschetz has a remarkable story. His family were Russian merchants who moved to Paris. In France he studied engineering rather than mathematics, because as a foreigner he had no chance for an academic appointment in that country. He came to the U.S. in 1905 to get some practical experience as an engineer. While he was working for Westinghouse in Pittsburgh, a terrible accident occurred. Both of his hands were destroyed! But instead of yielding to despair, he changed careers. He did graduate work at Clark University in Worcester, Massachusetts, earned a doctorate in algebraic geometry, and became a professor in Kansas. There, in total mathematical isolation, he made seminal discoveries in algebraic topology that attracted attention at Princeton. The American topologist James Waddell Alexander, in the math department at Princeton, got Lefschetz a visiting appointment, and then a regular position. In time, Lefschetz became chairman at Princeton, and is given credit for the leadership that made it one of the foremost mathematics departments in the U.S. and in the world. All this is well known, and on the record. What I have not seen in print, but learned directly from Lefschetz’s student Abe Hillman, is the fact that, as one would expect, the appointment of the foreign Jew Solomon Lefschetz to the faculty at Princeton was far from easy. The administration of Princeton University resisted bitterly. But Alexander was not only a leading topologist; he was a member of a first-rank family
in Princeton, socially and financially [5]. He was a socialist, an active supporter of Norman Thomas’s campaigns for the presidency. (He was also a prominent mountaineer; in fact, he preferred to enter his office by climbing the outside of Fine Hall and then coming in through the window.) His great-great-grandfather Archibald Alexander was the first professor and Principal of Princeton Theological Seminary, from 1812 until 1851. Several members of the family were president or vice-president of the Equitable Life Insurance Company. Alexander’s father was a well-known artist, whose circle of friends in Paris and America included Claude Debussy, Henry James, Stephane Mallarme, Auguste Rodin, and John Singer Sargent. Because of his social and financial connections, he was able to bring pressure beyond what a mere mathematics department could exert, and succeed in making Solomon Lefschetz a Princeton professor.

Like Norbert Wiener, Solomon Lefschetz did not fit in perfectly with the WASP-y academia of the 1920s Ivy League. Gian-Carlo Rota has painted an unforgettable picture of him, in his memoir [12]. Far from being timid or retiring because of his severe physical handicap, Lefschetz was a roaring lion, fearless and intimidating in all mathematical or academic controversies. He became president of the American Mathematical Society in 1935, but not without opposition from G. D. Birkhoff, who wrote in a private letter to the secretary of the AMS, “I have a feeling that Lefschetz will be likely to be less pleasant even than he had been, in that from now on he will try to work strongly and positively for his own race. They are exceedingly confident of their own power and influence in the good old USA.” Birkhoff was deluded. Far from favoring Jews, Lefschetz as a Princeton professor usually refused to accept Jewish students, for he thought they probably would not be able to get academic jobs. Perhaps he thought that having a Lefschetz as their adviser would only make it harder for them. Lefschetz did hire the Jewish refugee Salomon Bochner—a major coup in his campaign to raise his department to world class. But the two great mathematicians soon clashed. My friend Martin Davis recalls that at their beer parties the grad students made sure Solomon and Salomon did not overlap, for they did not speak to each other.

I am also indebted to Abe Hillman for some word-of-mouth history of Columbia University’s math department. Like NYU, it is located in New York City, a major center of Jewish population. But unlike NYU, it doesn’t seek to identify with the city; rather, it seeks to be viewed as in the same class as the other elite Ivy League schools, Princeton, Yale, and Harvard. Indeed, since its New York location might render it susceptible to a large Jewish participation, it has an even stronger motivation to preserve its non-Jewish image. Nevertheless, there was a Jewish mathematician at Columbia as early as 1900. Edward Kasner was the first Jewish appointee, and his appointment is credited to the efforts of his mentor Cassius Jackson Keyser, a leading and influential member of the Columbia math department. J. F. Ritt was appointed in 1921, the second Jewish member of the department, and the adviser of my friend Abe Hillman before he switched from Columbia to Princeton. Hillman told me that as a student Ritt had transferred from City College to George Washington University in his senior year, because he believed that a degree from City had some degree of Jewishness associated to it. He always signed himself as J. F. Ritt, not Joseph Fels, as another measure of self-protection.

A third Jewish mathematician of note was associated with Columbia. Jesse Douglas, a student of Kasner, was one of the very first winners of the Fields Medal,
along with Lars Ahlfors in 1936, for his solution of the Plateau problem, to construct a minimal surface bounded by an arbitrary space curve. Douglas’s name is almost forgotten today. He is a rather tragic figure, one of several important mathematicians gravely handicapped by what are now called bipolar, and used to be called manic-depressive, symptoms. He had a junior position at M.I.T., which he lost as a result of inability to perform consistently in the classroom. Although a Columbia graduate, and a member of the National Academy of Sciences, he never was offered a regular position at Columbia. According to Hillman, Ritt was opposed to hiring Douglas at Columbia, for two reasons: because it might attract unfavorable attention to have three Jews in the math department there, and also because Douglas was the student of Ritt’s rival, Kasner, the other Jew in the math department. Douglas was forced to support himself by holding three different part-time teaching jobs in three different colleges in the New York area. Hillman was able to help him by recruiting support from Herbert Robbins, who was at Columbia, but in the statistics department (not the math department), and was willing to do Douglas a good turn. With Robbins’ support, Douglas did get a full time job at City College.

Here at the University of New Mexico, there was a brief interaction with the “Jewish problem” in the 1930s, now totally forgotten even though it was written up by Carroll Newsom in his autobiography. Newsom was chairman of the math department at UNM in the 30s, before he went on to higher things as president of NYU and then head of the publishing giant Prentice-Hall. Newsom writes that he was asked to help in the effort to find jobs for Jewish refugees from Europe, and he decided to do so. In fact, he hired Arthur Rosenthal, who was a well-known Austrian Jewish mathematician, co-author of the major treatise *Set Functions*, with Hans Hahn. Newsom writes that he was subject to serious attack by New Mexicans who objected to giving a job to a foreigner and a Jew. Newsom did not give in to this pressure, and in fact took pride in his own courage in hiring Rosenthal. Rosenthal left New Mexico after a few years, and went to Purdue.

So in elite U.S. math departments in the ‘20s and ‘30s of the last century there was no over-representation of Jews, but rather under-representation. Ralph Phillips, who got his degree in 1939, had the advantage or disadvantage that his name does not sound Jewish. He applied for jobs in a number of departments, and received invitations for interviews. But then, when his prospective employers met him and learned that he was Jewish, their interest in hiring him evaporated. M.I.T. was one great university that cancelled its interest in Phillips when they found out he was a Jew.

In his article about these experiences, Phillips mentions Birkhoff’s malign influence. But Birkhoff was not without defenders. Saunders MacLane, who collaborated with G. D. Birkhoff’s son Garrett in their well-known algebra textbook, wrote a response defending Birkhoff from Phillips. He did not deny that Birkhoff was a bit of an anti-Semite, but he argued that it was unfair to single out Birkhoff, for in those days its was normal or common to be anti-Semitic—“everybody” did it. MacLane’s defense seems defective, however, for Cassius Keyser and James Alexander prove that not everyone was anti-Semitic, even in the ‘20s and ‘30s.

**Today.** What this story makes plain is that there was a great transformation in American society, with respect to Jews, as a consequence of World War II. The U.S. was attacked by Hitler’s ally, Japan, and so became committed to all-out war
against Hitler and Nazism. But Hitler and Nazism meant first of all, and above all, extreme and unlimited hatred of Jews. So anti-Semitism became un-American. Hitler was America’s enemy, and Hitler was the supreme anti-Semite. So it became untenable to exclude Jews from academia, or from Wall Street, or from the Cabinet of the U.S. President.

Once the barriers were down, it turned out that a lot of Jewish students were interested in math, and before long under-representation became over-representation. It is pretty clear that there used to be major cultural differences between the community of American Jews and the mainstream, non-Jewish, American community. Jews were bookish, studious; they were used to arguing and reading. Their tradition of business and commerce was associated with calculation and arithmetic. All this, it is easy to believe, makes it natural that a disproportionate number are attracted to math. Some people even think that there is something Talmudic about mathematics!

What is certain is that the Jewish domination of American mathematics has passed its peak. One need only look at the names of the winners of scientific talent contests in recent years. No longer are most of the names Jewish. Instead, most of the names are Chinese, or Vietnamese, or Japanese, or Korean, or Indian. There are some Jewish names, but most of these turn out to be the names of children who have come to the U.S. from Bulgaria or Romania. American-born Jews are a diminishing presence in American mathematics.

Why? Easily explained, although again I only have strong impressions and anecdotal evidence. To put it in brief, we have become assimilated and Americanized. Unlike our grandparents’ generation, we are just as likely to play golf and drink cocktails as the gentiles (don’t say goyim). Our bright youngsters choose law school or business school, not science. We get divorced; even vote Republican. Jews have been accepted in America, and so (allowing for lots of exceptions) we have become, more and more, just like other Americans.

(Thanks to Jerry Alexanderson, Chandler Davis, Martin Davis, Bonnie Gold, Jerry Goldstein, Richard J. Griego, Michael Henle, Peter Lax, Elena Marchisotto, Warren Page, Peter Ross, and Steve Rosencrans for helpful comments and suggestions.)


References


Paul Cohen and Forcing in 1963

I recently became aware of some writings of the prominent French metaphysical philosopher, Alain Badiou. His book *Number and Numbers* [3] actually proposes John Conway's surreal numbers as the fundamental components or descriptors of Being itself! An earlier work of his, *Being and Event* [2], used Paul Cohen's famous invention of “forcing” to explain how true novelty and free choice are possible in the Universe. (I published a skeptical review of *Number and Numbers* [9], and I was recently consulted by a colleague from the University of New Mexico philosophy department regarding Badiou’s pretentious misuse of Cohen’s forcing.) These experiences bring back vividly my interactions with Paul in the early 1960s. He had just astounded the mathematical world by proving the independence of the Continuum Hypothesis and the Axiom of Choice from the system of Zermelo-Fraenkel. I was a lowly instructor at Stanford University on a two-year appointment, having completed my Ph.D. at New York University and I had not yet attained my tenure-track post here in New Mexico.

In my present mature years, I look back at those interactions in a riper perspective. But at the time, I found interaction with Paul Cohen difficult and troubling. My job at Stanford was a result of the beneficence of my NYU mentor Peter Lax and his Stanford collaborator Ralph Phillips. Although I had written a creditable doctoral thesis, I was acutely conscious of how little I knew or understood in the broad field of contemporary advanced mathematics. When encountering other mathematicians, my goal was self-protection—to cover up my ignorance and try to pass for a real mathematician. Paul’s aggressive, brash way of coming on to people was the utter opposite of my own.

In social origin, Paul was not that different from me—a tallish, nerdish New York Jew. In fact, Paul’s family origin was in poverty and deprivation. But he had always been triumphant in competitive mathematical environments—Stuyvesant High in Manhattan, Brooklyn College, the University of Chicago graduate school. He approached a new relationship by demonstrating that he was the smartest person in the room. When we met, my timidity and his aggressiveness led to my immediate withdrawal, verbally if not physically. Yet we did develop a sort of relationship.

I think Paul was lonesome. He wanted to impress and control people, not necessarily drive them away. In fact, he would work out his ideas by talking to some willing listener, often a qualified grad student or postdoc (or, on occasion, one of the mature mathematicians Paul considered worth listening to). I didn’t serve Paul’s needs, either as a sounding board or as a playful competitor. But we happened to be neighbors in Menlo Park, an unpretentious suburb of Palo Alto, the site of Stanford University. Paul was beginning to struggle to assimilate his new status as a superstar. (He had already been well established as a promising young star.) He asked my wife Phyllis for advice in buying better clothing in order
to look like more of “a gentleman.” (She was glad to help him, of course.) He even enjoyed bits of conversation with our son Daniel, then aged 6 or 7.

On an extended visit to Sweden, a great change occurred in Paul’s life. He met his future wife, Christina, and she accompanied him home to Menlo Park. As I knew her back then, Christina was an attractive, cheerful, friendly, bright young woman, not particularly inclined to the academic or intellectual style. Perhaps that was why Paul was reluctant, hesitant, to let people know about her. Even her name “Christina” was an issue for a Jewish boy from Brooklyn. How and when would Paul tell his mother about Christina? His mother would certainly have to come from Brooklyn for the wedding. But first, he somehow had to find a rabbi willing to perform the ceremony. Not an easy task, even in the Bay area of Northern California! But it all worked out, they did get married, and they had three beautiful children.

The reason I ultimately established a solid connection with Paul Cohen was that, years before I even entered graduate school in New York, I had worked at *Scientific American* magazine as an editorial assistant. Paul had been invited by *Scientific American* to write about his work on the Continuum Hypothesis. *Scientific American* was then still following a policy of publishing articles written by scientists themselves. Such manuscripts often required heavy rewriting in order to be accessible to the *Scientific American* readership. But Paul’s manuscript had actually been rejected. Not only was it not publishable, it was impossible for the editors of *Scientific American* to edit it so as to make it publishable. (Of course they did pay Paul the usual fee for his work.) That was why Paul asked me if I would like to try and rewrite his manuscript to make it publishable in *Scientific American*. Of course I was flattered and delighted at this invitation to connect with the peak of mathematical achievement.

In the manuscript and in talks he had given, Paul had tried to explain his work by analogy with the history of geometry. Just as the fifth postulate of Euclid, the parallel postulate, had long been troublesome, and finally, by the establishment of non-Euclidean geometry, been proved to be independent of the other axioms, so the Axiom of Choice, in set theory, was long troublesome. Paul had created a model for the Zermelo-Fraenkel (ZF) axioms of set theory, in which the axiom of choice failed; therefore that axiom is independent of the other axioms, just as Euclid’s parallel postulate is independent of Euclid’s other axioms.

That analogy became the outline of my article, taking ideas from Paul’s rejected manuscript, his other writings on the subject, and his oral presentations that I had heard. When I showed him my rewrite, he questioned my slight attempts at a touch of humor, and suggested I publish it under my own name. Of course I preferred to be a coauthor with the great and famous Paul Cohen, not just the solo author of a *Scientific American* article. And so the collaboration came about.

It was already evident that the technique of “forcing,” which Paul used on the Continuum Hypothesis and the Axiom of Choice, is a powerful method to show that other open problems are undecidable. Within a few years many open problems were shown by others to be independent of ZF (indeed, of ZFC: Zermelo-Fraenkel plus the Axiom of Choice) by the method of forcing. And logicians have developed and elaborated forcing in forms and guises far beyond Cohen’s original version.

Given some statement formulated in an axiom system, can you create one model of that system in which the statement is provable, and another model in which it
is disprovable? If so, by the Completeness Theorem, which equates consistency with having a model, it will follow that both the given statement and its negation are consistent with that axiom system. (For example, the pseudosphere, a model for non-Euclidean geometry, shows that non-Euclidean geometry is consistent, and therefore that Euclid’s parallel postulate is not provable.) Cohen (following on Gödel) had achieved that result for the Zermelo-Fraenkel axiom system of set theory, with regard to both the Continuum Hypothesis (even assuming the Axiom of Choice) and the Axiom of Choice.

“Forcing” quickly became a necessary topic in any second-year (advanced) course in logic or metalogic. Logicians modified it, generalized it, reworked it, and applied it. On the other hand, mathematicians who are not logicians sometimes find it obscure and difficult to grasp. I can offer a few helpful words. Of course they will just be rewordings of what Cohen himself and subsequent logicians have already said in more technical or less readable ways.

The first essential thing to grasp is the famous Löwenheim-Skolem theorem, which says: “If your axiom system is consistent (if it has any model at all) then it has a countable model—a countable set in which the relations of your axiom system are defined and satisfied.” This is paradoxical and confusing, for it means, in particular, that the system of real numbers, which is uncountable, can be modeled by a countable set. In this countable model of the real number system, how can the theorem “the real numbers are uncountable” still hold true? The paradox is simply explained. The statement “the real numbers are uncountable” merely says that there is no one-to-one mapping between the real numbers and the natural numbers. So, in the model, there cannot be any such mapping between whatever is interpreted as the set of natural numbers and whatever is interpreted as the set of real numbers. The expression “set of real numbers” is being interpreted by a set that is actually countable. But that set can’t be “counted” (mapped onto the natural numbers) in the model.

The Löwenheim-Skolem theorem is essential in Cohen’s treatment because it gives him a countable model in which to carry out the construction called “forcing,” and thereby analyze the Continuum Hypothesis and the Axiom of Choice. In fact, since we can label everything in any countable set by using sequences of zeroes and ones, everything we do can be done just by working with sequences of zeroes and ones.

Given, then, the countable model of the Zermelo-Fraenkel axioms, which Löwenheim-Skolem provides, what is our task—or rather, what was Cohen’s task? Simply to enlarge this countable model—add more elements to it, each of which is just a sequence of zeroes and ones—in such a way that the enlarged countable set is still a model of ZFC, but in which the Continuum Hypothesis is false. This sounds confusing, I admit. The Continuum Hypothesis is a statement about the continuum, about an uncountable set. How can you claim to prove anything about this uncountable set without having an uncountable set to talk about? The answer has two parts. First, the method of forcing is a way to add a new sequence of zeroes and ones, one step at a time, in such a way as to preserve the ZFC axioms. This is not so surprising, after all. There are only a countable number of such axioms, and so it should be possible sequentially to check that they are all obeyed as we introduce a new element into the model. However, to write down exactly how to achieve this was a great technical feat, and it’s still quite a chore to read through.
As we add each new bit to the sequence of zeroes and ones we are introducing into the model, the difficulty is in ensuring continually that no previously chosen bit has to be rejected. The rules to accomplish this are precisely what we mean by “forcing”. The new sequence is restrained, “forced” to satisfy certain conditions, in order not to spoil the model, in order to keep the validity of the ZF axioms. Cohen showed how to do this, while keeping his freedom to impose some new conditions on the new elements, for the sake of making some desired statement provably true in the new model. A key point is to adjoin new reals that are “generic,” that is, do not code any special information about the model we started with.

In fact, there are plenty more new sequences of zeroes and ones available to insert into the model. There are actually uncountably many such sequences available (since, as Cantor proved, the set of infinite sequences of zeroes and ones is an uncountable set.) So Cohen was free to use his forcing method to introduce into the countable Löwenheim-Skolem model a number of generic reals that are in one-to-one correspondence with an ordinal in the original model, which is larger than countable infinity in that model, but still without introducing the complete set of sequences of zeroes and ones (which would have represented the set of all subsets, the “power set,” of the natural numbers). In doing so, he was creating a model of ZFC in which there is an uncountable set strictly smaller than the continuum—that is, in which the Continuum Hypothesis is provably false.

Cohen achieved his results after a determined, year-long struggle. Solomon Feferman, one of the up-and-coming logicians at Stanford, served as a principal sounding board. Naturally, Paul went through many wrong ideas before he came up with the right ones. And then getting it all down in correct form was a major job. Logicians were impressed, and they needed a little time to absorb Paul’s radical new method before they could confidently endorse his result. But he was impatient with people who weren’t convinced immediately. So he went straight to the top—to Gödel himself. As he prepared the written version to show to Gödel, Feferman and others brought problematic points to his attention and suggested how they might be dealt with.

I’ve been told there were two episodes. The first time Cohen went to Gödel’s house in Princeton, Prof. Gödel came to the door, took the manuscript without a word, and shut the door. After a few days, Paul was invited back, and this time was welcomed inside.

Still, Gödel needed a little time to edit Paul’s article before he sent it to the Proceedings of the National Academy of Sciences. Paul found it hard to be patient, even with Gödel. He wrote to Gödel pleading with him to hurry up with a public endorsement of Paul’s achievement. Gödel answered reassuringly.

After I left Stanford for New Mexico, I had one more major encounter with Paul Cohen. He came to UNM to lecture on his work. Stan Ulam was living in Santa Fe and was working in Los Alamos. He sent word that he wished to meet Paul Cohen. I had the honor of introducing Cohen and Ulam to each other. After that introduction, Ulam always referred to me as “Paul Cohen’s friend,” and would ask me about Paul. I usually had no news to tell him.

It’s well known that Paul’s only concern became Riemann’s conjecture on the zeroes of the zeta function. For someone who had solved problem number one on Hilbert’s list, nothing short of the Riemann conjecture was worthy of his time. At
one point a rumor flew around the world—“Cohen has proved Riemann’s conjecture!” It was the irksome duty of the chairman of Stanford’s math department to answer a deluge of queries, over and over, “No, it’s not true.”

References

Part 3

Selected book reviews
Introduction

These reviews were included because they touch on philosophical, methodological or ethical aspects of mathematics. The Byers review (26) appeared in the Notices of the American Mathematical Society. I found his book so exciting that I felt compelled to review it, and to have my review published in the most visible publication. The books by van Deemter (25) and Ruelle (27) were sent to me for review by Bob O’Malley, the editor of the SIAM Review. Van Deemter’s book on vagueness was mainly oriented toward computer programming related to “artificial intelligence.” But, as the author realizes, the problem of vagueness is very deep and pervasive. It cannot be resolved or argued away. After due analysis and discussion, it must be lived with. Ruelle’s book is an outstanding example of a mathematician deeply and honestly describing what he does. I quote extensively from it in To Establish New Mathematics, We use our Mental Models and Build on Established Mathematics.

The next three reviews all appeared in The Mathematical Intelligencer. Masha Gessen is an acute, witty observer of intellectual life in Russia. Her book is a fascinating portrait of Grigori (Grisha) Perelman, who proved the Poincare conjecture and the Thurston program, and then refused all U.S. university job offers, refused the Fields Medal of the International Mathematical Congress, and refused $1 million dollars offered to him by the Clay Foundation of New York. Ian Stewart is a prolific English geometer-analyst and expositor of mathematics, whose advice to a young mathematician makes an interesting contrast to his own career. Alain Badiou, a name unknown in the U.S. among mathematicians and even among many philosophers, has been described as “more than the greatest living French philosopher”. He uses John Conway’s “surreal numbers” to construct all reality. Badiou actively organizes and writes on behalf of a post-Maoist “revolutionary” tendency in French left-wing politics.
A top-ranking mathematical Pooh-Bah once pontificated at me something like this: “The essence of mathematical language is that it is precise.” But he neglected to provide a precise definition of “precise.” In fact, I don’t recall having seen any author offer such a definition, so an appropriate rejoinder (alas, it is now too late!) would have been, “How can you seriously demand precision without a precise definition of precise itself?”

On the contrary, I would claim that everything we imagine to be “precise” turns out actually to be vague if we just look at it closely enough.

Here “vague” is, surprisingly, given a rather precise meaning. Along with author van Deemter, I say a concept is “vague” if it’s not always possible to decide definitely whether it applies. “Vague” in this sense is then a kind of opposite of the standard (going back to Cantor himself) definition of a “set.” A “set,” you recall, is supposed to be a collection of things where any candidate for membership receives a definite answer: “Yes” or “No.” “In” or “Out.” “One” or “Zero.”

The most prominent and important example of precision is, of course, the digital computer. In its modern electronic manifestation, we assume (take for granted, do not question) that every memory location is either magnetized or demagnetized (neglecting precise details of engineering and manufacture.) The very notion of a computer as conceived by Turing or von Neumann is based on this hypothesis. However, in practice, in reality, all actual machines are subject to defective materials and to wearing out, to being used up. That is one of the reasons why real-world (not conceptual) computers have occasional glitches, bugs, hiccups, and hang-ups. We don’t quite attain perfectly precise zero-one absoluteness after all! In practice, we have to recognize, cope with, and try to understand vagueness, even in that most sacrosanct Aristotelian realm of digital computation, let alone in homely, down-to-earth concepts like “obesity” or “poverty.” Van Deemter uses a lot of space to convince us that the poverty line or the obesity criterion is vague. It hardly seems worth the trouble. Who would argue the point? Once that is out of the way, he presents some interesting material, unfamiliar to me, about how vagueness is treated in recent literature on logic and in practice in contemporary computation.

The standard test problem for precisifying vagueness is called “Sorites,” from the Greek for “heap.” A popular way of explaining Sorites is by way of the concept “bald.” Certainly, a hairy man does not become bald by removing one hair. Repeat the argument a finite number of times (as many times as he has hairs) and you have “proved” the fallacy “no one is bald” or “a man with no hair is not bald.” If you prefer, you can talk about a heap of sand, which remains a heap as you remove one
grain at a time until it is all gone—but “it” is still a heap! Removing a single grain does not change a heap to a nonheap.

There is something wrong with this reasoning. But where is the fallacy?

Van Deemter reports a thriving literature on nonstandard logic, with ample references for the serious reader. One way out is to propose that there is a transition point where the heap is no longer a heap—we just aren’t sure exactly where the transition is. So we bring in a three-valued logic: Yes, No, and Maybe. There is a Maybe zone separating the Yes (plenty of sand or hair) and the No (hardly any, not enough). In other words, the basic axiom that removing one hair or one grain doesn’t matter is relaxed for part of the domain of discourse.

But the trouble doesn’t go away, because now we seem to have merely introduced two new undefinable boundaries—between Yes and Maybe, and between Maybe and No. If we want to compute or to reason, this method forces us to arbitrarily put, somewhere, two unreasonable, unintuitive jumps. It’s a poor and clumsy makeshift.

It would be better to replace the definite yes-no or one-zero logic with an incremental logic (call it “probabilistic” if you like). Removing one hair or one grain creates a small difference, almost zero, but not exactly zero. This logic is more palatable, more plausible, but it destroys one of the most fundamental basic laws of logic, namely, “Conjunction”: “If A is true and B is true, then ‘A and B’ is true.” Because although a lot of zero-changes add up to a total of zero, a lot of little changes can add up to a big change.

All this may seem too airy, hypothetical. Pure theory. One theory, “fuzzy logic,” has a record of practical application in industry. But at least in the U.S. it has not yet attained full academic respectability. Van Deemter explains that fuzzy logic isn’t a fully explicit, logically coherent methodology. It offers various procedures, possibly leaving the practitioner to a non-algorithmic “seat of the pants” or “follow your gut” resort for choosing the best action.

Yet vague concepts are not only unavoidable, they are even helpful. A simple example by Rohit Parikh shows that although “Bob” and “Ann” may have quite different understandings of the vague concept “blue,” by telling Bob that the topology book she wants him to fetch for her is “blue,” Ann is still likely to help him find the right book.

The programmer is forced to deal with vagueness by the application of machines to practical decision making (an activity sometimes farcically called “artificial intelligence”). Simply because vagueness is an inescapable feature of reality, and using computers in practical decision-making runs smack into Reality itself. Programmers are asked for programs to help CEOs design sales agendas, to help doctors diagnose and plan treatments, or to help generals deploy their bombs. Every such situation copes with vague information, vague decision criteria, and vague evaluation of results. Yet the computer consulting firm somehow or other will sell its “artificial intelligence” programs. We hear Marketing not too far in the background: “Vagueness must be made precise! Somehow! Come what may!”

Van Deemter advocates bringing vagueness fully into our scientific logic. He warns that this will require difficult concessions (like including the “excluded middle” and/or reinterpreting “and” and “or”.)

Can we accept vagueness as real and unavoidable, while still marketing “AI” programs that offer to tell generals and CEO’s their next move? The best available
advice may still be the considered judgment of human experts, present on the scene and thoroughly grounded in the realm of experience where the problem arises.

References

Review of *How Mathematicians Think* by William Byers

This book is a radically new account of mathematical discourse and mathematical thinking. It's addressed to everyone, from a lay reader who hasn't met complex numbers, up to a professional who appreciates Sarkovsky's theorem on cycles of iterated functions, or Goodstein's number-theoretic equivalent of Gödel's theorem for arithmetic with induction. No math preparation is presupposed, and everything is explained with complete clarity, yet deep contemporary issues are faced with no hesitancy. The discussion is free of pretentiousness or grandiosity. Byers straightforwardly explains the issues and clarifies them.

Starting with Imre Lakatos' 1976 *Proofs and Refutations*, some writers have been turning away from the search for a "foundation" for mathematics and instead, seeking to understand and clarify the actual practice of mathematics—*what real mathematicians really do*. Conferences toward this end have been held in Mexico, Belgium, Denmark, Italy, Spain, Sweden and Hungary. In particular, I would mention books by Bettina Heintz, Carlo Cellucci, and Alexandre Borovik. My own anthology collects essays by mathematicians, philosophers, cognitive scientists, sociologists, a computer scientist, and an anthropologist.

There's not much consensus, but at least one thing has been pretty generally taken for granted: mathematical thinking and discourse is supposed to be *precise*—that is to say, unambiguous. A mathematical statement is supposed to have a single definite meaning. What Byers's book reveals is that ambiguity is always present, from the most elementary to the most advanced level. In teaching school mathematics, it is an unacknowledged source of difficulty. At the level of research, it is often the key to growth and discovery.

Ambiguity can just mean vagueness. But also, it can mean, as Byers puts it, “a single situation or idea that is perceived in two self-consistent but mutually incompatible frames of reference.” (p. 28) In fact, he makes a persuasive case that ambiguity is actually what makes mathematical ideas so powerful:

Normally ambiguity in science and mathematics is seen as something to overcome, something that is due to an error in understanding and is removed by correcting that error. The ambiguity is rarely seen as having value in its own right, and the existence of ambiguity was often the very thing that spurred a particular development of mathematics and science....The power of ideas reside in their ambiguity. Thus any project that would eliminate ambiguity from mathematics would destroy mathematics. (p. 24)
Familiar examples of ambiguity include: Negating Euclid’s parallel postulate. Different sizes of infinite sets. Using logic to prove the limitations of logic. Infinitely rough curves, self-similar on infinitely many different scales. And on and on.

No surprise that there’s ambiguity in “infinite” or “infinity”. The philosophically inclined won’t be surprised that there’s ambiguity in “true” and even in “proof”. But even in the simplest, most “elementary” mathematical steps, there is already deep, unacknowledged ambiguity.

An obvious example is: square roots of negative numbers. It takes effort simultaneously to know that “-1 has no square root” (on the real line) and “it has two of them” (in the plane.) The student must switch contexts as needed. Sometimes there is no square root, sometimes there are two. It all depends on what are you are talking about, what are you are trying to do! But a while back, the same effort was required regarding negative numbers. We have forgotten that for D’Alembert or De Morgan, it both made sense and didn’t make sense, to contemplate a quantity less than “nothing”.

Indeed, “zero” is ambiguous! Unlike D’Alembert or DeMorgan, today we don’t say “nothing” when we mean “zero”. Zero is something—it’s a number. Yet, of course, “nothing” is what it means. Zero is a something, and what it stands for, what it means—is “nothing”! This is ambiguous, but we math teachers have buried the ambiguity so deeply, that if we ever have to talk to a student who is troubled by it, we can hardly understand what is her difficulty.

“One” is ambiguous! Frege’s famous book, *Grundlagen der Arithmetik*, was motivated by mathematicians’ inability to explain coherently what they meant by “one.” Frege’s answer was: “one” is the “concept” of singletons. But Dedekind and Peano had a different answer: “one” is just an undefined term, in the axioms for the relation of “successor.” And still another answer is given in every elementary math classroom—“one” is a slash or a tally mark, that can be repeated, to make “two,” and repeated again, to make “three.” If that’s not enough ambiguity, there’s still a deeper ambiguity in “one.” When we choose to think about all the things that belong to some system (for example, all the counting numbers) and regard that collection as “one” set—when we make a unity out of a multiplicity—we are committing an ambiguity. An ambiguity, indeed, that is a central feature of mathematical thinking. (Notice how the word “universal,” with the sense of “all-embracing”, uses the primitive root, “uni”, a single slash or tally mark.)

In fact, the relation of “equality” in general is ambiguous, for the entities on the two sides of the equals sign are usually not identical. (“\(x = x\)” is not usually interesting.) In an interesting equation, the entities on the left and the right are not identical, so the claim that they are “equal” is necessarily ambiguous, subject to different interpretations according to context. Using the simplest example imaginable, Byers elucidates the ambiguity inherent in the notion of equality:

When we encounter “1 + 1 = 2”, our first reaction is that the statement is clear and precise. We feel that we understand it completely and that there is nothing further to be said. But is that really true? The numbers “one” and “two” are in fact extremely deep and important ideas... The equation also contains an equal sign. Equality is another very basic idea whose meaning only grows the more you think about it. Then we have the equation itself, which states that the fundamental concepts of unity
and duality have a relationship with one another that we represent by “equality”—that there is unity in duality and duality in unity. This deeper structure that is implicit in the equation is typical of a situation of ambiguity. Thus even the most elementary mathematical expressions have a profundity that may not be apparent on the surface level. (p. 27)

A more advanced example of the ambiguity of the equals sign is 1 = .999....

“What is the precise meaning of the “=” sign? It surely does not mean that the number 1 is identical to that which is meant by the notation .999.... There is a problem here, and the evidence is that, in my experience, most undergraduate math majors do not believe this statement...they all agreed that .999... was very close to 1. Some even said “infinitely close”, but they were not absolutely sure what they meant by this....This notation stands both for the process of adding this particular infinite sequence of fractions and for the object, the number that is the result of that process.... Now the number 1 is clearly a mathematical object, a number. Thus the equation 1 = .999... is confusing because it seems to say that a process is equal (identical?) to an object. This appears to be a category error. How can a process, a verb, be equal to an object, a noun? Verbs and nouns are “incompatible contexts” and thus the equation is ambiguous..... I hasten to add that this ambiguity is a strength, not a weakness, of our way of writing decimals. To understand infinite decimals means to be able to move freely from one of these points of view to the other. That is, understanding involves the realization that there is “one single idea” that can be expressed as 1 or as 1 = .999... that can be understood as the process of summing an infinite series or an endless process of successive approximation as well as a concrete object, a number. This kind of creative leap is required before one can say that one understands a real number as an infinite decimal.” (p. 41)

Byers also discusses how students struggle with ideas that are less advanced than 1 = .999.... For example, here he unravels the ambiguity of the “variable” $x$ as students encounter it in the seventh grade:

Does the “$x$” in “$x + 2 = 4$” refer to any number or does it refer to the number 2? The answer is, “Both and neither.” At the beginning, $x$ could be anything. At the end, $x$ can only be 2. Yet at the end, we are saying that every number $x$ NOT = 2 is not a solution, so the equation is also about all numbers. Thus at every stage, the $x$ stands for all numbers but ALSO for the specific number 2. We are required to carry along this ambiguity throughout the entire procedure of solving the equation. It begins with something that could be anything and ends with a specific number that could not be anything else. What an exercise in subtle mental gymnastics this is! How could this way of thinking be called merely mechanical? No wonder children have
difficulty with algebra. The difficulty is the ambiguity. The resolution of the ambiguity, solving the equation, does not involve eliminating the double context but rather being able to keep the two contexts simultaneously in mind and working within that double context, jumping from one point of view to the other as the situation warrants.” (p. 42)

Mathematicians are accustomed to making use of multiple “representations” of “the same” thing. With the precise notion of “isomorphic equivalence,” we are able legitimately and smoothly to use different representations simultaneously. The group of permutations on three letters is “the same thing” as the automorphism group of the equilateral triangle, or the group of functions under composition generated by \(1/x\) and \(1 - x\), and so on and so on. And any graph is equivalent to, is virtually “the same”, as its adjacency matrix. And any solution of Laplace’s partial differential equation is an integral with a Green’s function as kernel, and it is simultaneously the minimal solution of a certain variational problem, and it is simultaneously the limit of a sequence of solutions of difference equations, and it is simultaneously the expected value of the outcome of the random motion of a Brownian particle, as well as the equilibrium distribution of heat in a homogeneous medium, and also the potential of a distribution of gravitational mass or electrostatic charge. When we make simultaneous or alternate use of “different” representations or interpretations of “the same” structure, we are using ambiguity in a controlled, algorithmic way—using the multiple-meaningness of the concepts of group, or graph, or solution of a differential equation.

In discussions of the nature of mathematics, the notion of “abstraction” is often mentioned, but rarely clarified or explicated. Byers has a remarkable explanation of abstraction. “Abstraction consists essentially in the creation and utilization of ambiguity.” For example, when functions are first introduced, either in the classroom or in the history of mathematics, they are active. The function transforms one number into another. Later, when we focus on differential operators, the functions are passive. The operator transforms one function into another. So which is it? Is a function active or passive, verb or noun? “The initial barrier to understanding, that a function can be considered simultaneously as process and object—as a rule that operates on numbers and as an object that is itself operated on by other processes—turns into the insight. That is, it is precisely the ambiguous way in which a function is viewed which is the insight.” (p. 48)

Byers doesn’t stop with mathematics itself. Not only mathematics, but even more, philosophy of mathematics is inextricably tied up with ambiguity, paradox, and contradiction. “Do we create math or do we discover it?” “Is it in our minds, or is it out there”? Contradictions, nicht wahr?

One deep ambiguity is the double meaning of “exist”. Does it mean something is “constructed” from already “constructed” entities, by some clearly understood notion of “construct”? Or does it rather mean something is contradiction-free, is “safe” to “postulate,” because it doesn’t crash into or interfere with other notions or facts that we don’t want disturbed? This is just the stale old argument between intuitionist/constructivists and standard/classical mathematicians. For Byers, the point isn’t to choose sides, to decide who is right and who is wrong. Rather, it is to perceive that this ambiguity of “exist” is intrinsic to our mathematical practice,
and is fruitful. The clash of viewpoints arising from this ambiguity brings forth interesting mathematics.

Speaking of the often mentioned but rarely analyzed unreflective Platonism of the working mathematician, Byers writes:

The ambiguity of an unsolved problem is mitigated somewhat by the Platonic attitude of the working mathematician. That is, she feels that it is objectively either true or false and that the job of the mathematician is "merely" to discover which of these a priori conditions applies. Psychologically, this Platonic point of view brings the ambiguity of the situation into enough control so that researchers have confidence the correct solution exists independent of their efforts. It moves the problem from the domain of "ambiguity as vagueness" in which anything could happen to the sort of incompatibility that has been discussed in this chapter where there are two conflicting frameworks, true or false."

"Contradictions demand resolution!" you may say. "To rise to the next level in philosophy of mathematics, we must overcome the contradiction, resolve it, not just pooh-pooh it!"

But Byers offers us an insight—this is the way it has to be! Live with it! Life is ambiguous and contradictory. Mathematics is part of life. Insofar as the philosophy of mathematics describes the total mathematical situation—process as well as content—naturally it's also bound to be ambiguous.

Well, that does in fact seem to be the case.

You might say that the work of the mathematician is to drive away ambiguity. "Precision" is what mathematics is all about. "Say what you mean, mean what you say, nothing is there except what is right on the page." Byers pushes us back, to the ambiguous situation that calls for mathematical explication. He makes us see that the ambiguity we insist on banishing is the source, the origin, of the mathematical work. "Logic moves in one direction, the direction of clarity, coherence and structure. Ambiguity moves in the other direction, that of fluidity, openness, and release. Mathematics moves back and forth between these two poles.....It is the interaction between these different aspects that gives mathematics its power." (p. 78) "Mathematical ideas are not right or wrong; they are organizers of mathematical situations. Ideas are not logical. In fact the inclusion should go the other way around—logic is not the absolute standard against which all ideas must be measured. In fact logic itself is an idea." (p. 257)

The normal mathematician—the philosopher's "working mathematician", the ordinary mathematician, the "mathematician in the street"?—may respond with a shrug and a, "So what?" We do our calculations and prove our theorems by following our noses, not by looking right or left to see where we are in the broader conceptual or "philosophic" realm. You don't need to know what is meant by "one" in order to know that one and one is two. But recall the old saying of Socrates, about the unexamined life. Most mathematical life, like most human life in general, is unexamined. Byers pulls away the covering habit and routine, to expose life-giving embarrassments hidden beneath.

You can't quite say that nobody has said this before. But nobody has said it before in this all-encompassing, coherent way, and in this readable, crystal clear
style. The examples are well known and familiar, but it’s something else to put them all together and say, “This is it! This is exactly what mathematics is all about, this is the very core and nature of mathematical thinking!”

Byers finds far-reaching consequences, beyond mathematics, for our very understanding of what it means to be human.

“Any great quest demands courage. It is a voyage into the unknown with no guaranteed results. What is the nature of this courage? It is the courage to open oneself up to the ambiguity of the specific situation. The whole thing may end up as a vast waste of time, that is, the possibility of failure is inevitably present... Our lives also have this quality of a quest, the attempt to resolve some fundamental but ill-posed question. In working on a mathematical conjecture, life’s ambiguities solidify into a concrete problem. That is, the situation of doing research is isomorphic to some extent with the situation we face in our personal lives. This is one reason that working on mathematics is so satisfying. In resolving the mathematical problem we, for a while at least, resolve that large, existential problem that is consciously or unconsciously always with us....Learners need support when they are encouraged to enter into new unexplored ambiguities. A new learning experience requires the learner to face the unknown, to face failure. Sticking with a true learning situation requires courage and teachers must respect the courage that students exhibit in facing these situations. Teachers should understand and sympathize with students’ reluctance to enter into these murky waters. After all, the teacher’s role as authority figure is often pleasing insofar as it enables the teacher to escape temporarily from their own ambiguities and vulnerability. Thus the value of learning potentially goes beyond the specific content or technique but in the largest sense is a lesson in life itself.” (p. 57)

This book strikes me as profound, unpretentious, and courageous.

References

Review of *The Mathematician’s Brain*
by David Ruelle

This book is a collection of brief essays on different aspects of mathematical work and life. There is mention of teaching and of the organizational and political sides of mathematics, but most of the book attempts to reveal to the outsider how mathematicians think. There is a refreshing directness and honesty which are best conveyed by selected quotations.

David Ruelle is famous for research in statistical mechanics and dynamical systems. He introduced the concept of “strange attractor,” and with Floris Takens of the Netherlands he produced a new model for turbulence. Since 1964 he has been a permanent member of the IHES, France’s analogue of the Institute for Advanced Study, working beside Alexandre Grothendieck, Jean Dieudonné, and Rene Thom. He is closely interested in brain research, especially as it relates to the brains of mathematicians. However, there is not much here about the actual brain itself, but Ruelle tells us a lot about his mathematical mind, which, after all, is the principal way he, like the rest of us, knows his mathematical brain.

An insider’s account of the early days at the IHES is a high point of the book. Ruelle gives a sympathetic description of the personality and working style of Alexandre Grothendieck. He feels strongly that Grothendieck’s eventual isolation from and hostility to the mathematical establishment of France are in large part due to his outsider status. Grothendieck chose to remain stateless until 1980. As a foreigner who did not pass through the Ecole Normale, he was left without support once he walked away from the IHES.

Grothendieck’s program was of daunting generality, magnitude, and difficulty. In hindsight we know how successful the enterprise has been, but it is humbling to think of the intellectual courage and force needed to get the project started and moving. We know that some of the greatest mathematical achievements of the late twentieth century are based on Grothendieck’s vision....Our great loss is that we don’t know what other new avenues of knowledge he might have opened if he had not abandoned mathematics, or been abandoned by it. (p. 33)

It may be hard to believe that a mathematician of Grothendieck’s caliber could not find an adequate academic position in France after he left the IHES. I am convinced that if Grothendieck had been a former student of the Ecole Normale and if he had been part of the system, a position commensurate with his mathematical achievements would have been found for him....Something shameful has taken place. And the disposal of
Grothendieck will remain a disgrace in the history of twentieth-century mathematics. (p. 40)

Almost all of the book is conversational and easy reading for any reader of SIAM Review. However, there is one impressive piece of technical mathematics. The Lee–Yang circle theorem is presented on pages 91–92, with a proof due to Taro Asano. This is a remarkable theorem on polynomials, a deep, puzzling result in elementary algebra, proved with a dazzling bit of elementary algebraic trickery. Since Lee and Yang are physicists, I gather that this theorem is important in statistical mechanics, but Ruelle doesn’t explain its physical interpretation.

The book begs comparison with other works, such as Hardy’s famous Apology and Gowers’ recent Very Short Introduction, as a brief, readable overview of mathematics, accessible to the cultured and literate lay person. It stands up well under such comparisons. It has an intimate, personal flavor, inviting the reader to get to know Ruelle himself, not only the mathematics he cares to expound. He turns out to be no dry, remote scholar, but a humane, opinionated, deeply thoughtful fellow human. The mathematics he chooses to present is appropriate and well explained. The philosophical and aesthetic issues he explores are important and often neglected. True, his very brief chapters can’t do much more than pose the issues and point out the difficulties. But best of all, his account of the pleasures and pains of doing mathematical research in the real world is honest and unsparing. I close with a few more samples.

The love of mathematical beauty is an essential reason why mathematicians do and teach mathematics....The beauty of mathematics lies in uncovering the hidden simplicity and complexity that coexist in the rigid logical framework that the subject imposes. (p. 129)

Constructing a mathematical theory is the essence of mathematical work. Constructing a mathematical theory is thus guessing a web of ideas, and then progressively strengthening and modifying the web until it is logically unassailable. Before that point you don’t have a theory. In fact, it is usually not assured at the beginning that you will be able to complete your construction as originally planned (otherwise, the theory would be uninteresting.). Clearly, during your construction work, you should concentrate your effort on the more uncertain links in your arguments. This is where your theory is most likely to fail, and you save time by knowing this early on. The easy and safe steps are left for later and are often handled in the final write-up by a dismissive sentence: “it is obvious that....” “it is well-known that....” (p. 114)

Doing mathematics is often an individual and solitary enterprise. But mathematics as a whole is a collective achievement. A mathematician lives in an intellectual landscape of definitions, methods, and results, and has greater or lesser knowledge of this landscape. With this knowledge, new mathematics is produced, and this invention changes more or less significantly the existing landscape of mathematics. (p. 108)
I have just described human mathematics as a labyrinth of ideas, through which the mathematician wanders, in search of the proof of a theorem. The ideas are human, and they belong to a human mathematical culture, but they are also very much constrained by the logical structure of the subject. The infinite labyrinth of mathematics has thus the dual character of human construction and logical necessity. And this endows the labyrinth with a strange beauty. It reflects the internal structure of mathematics and is, in fact, the only thing we know about this internal structure. But only through a long search of the labyrinth do we come to appreciate its beauty; only through long study do we come to taste fully the subtle and powerful aesthetic appeal of mathematical theories. (p. 96)

A mathematician who has finally understood a question may say that it was after all very simple. But this is usually an erroneous feeling. In fact, when our mathematician starts writing things down, their complexity unfolds and may end up looking formidable. A simple mathematical argument, like a simple English sentence, often makes sense only against a huge contextual background. (p. 87)

I think there is something peculiar about many (not all) mathematicians: a somewhat rigid way of thinking and behaving. The evidence on which I base this opinion is anecdotal, not clinical. To be specific, my experience is that many mathematicians will give excessive detail when answering a casual question (on the rules of the game of checkers, say, or genealogy in feudal Japan), or they will find logical difficulty with an assertion that causes no problem for most people. Or perhaps they may ask you to repeat a joke and then ask you to explain why it is funny. Let me repeat that not all mathematicians, thank God, are like that. One finds among them a great variety of psychological types, and even psychiatric disorders, provided the latter do not impair intelligence. Paranoid, manic-depressive, or obsessive tendencies are not rare among scientists in general, but there are also many who are depressingly normal and dull. The evidence for high-level mathematicians having nervous breakdowns is impressive, if anecdotal. (pp. 81–82)

Ruelle quotes Constance Reid on the breakdowns suffered by David Hilbert and Felix Klein, and Courant’s view that “almost every great scientist I have known has been subject to such deep depression.” He then writes,

“One might compare doing great mathematical work with climbing high mountains; they are admirable feats, but dangerous. The mind in one case and the body in the other are pushed to their limits, and there is a price to be paid. Apart from a nervous breakdown, the way mathematicians overuse their brain often results in absent-mindedness and lack of practical sense.” (p. 82).
Of course some mathematicians want to be masters, some want to be slaves, and some will try to involve you somehow in their particular neurosis. But you can stay away from them if you are lucky and if you so wish. Mathematical research is a highly individual enterprise. It requires mental agility and the patience to pace around an infinite and dreary logical labyrinth until you find something that has not been understood before you have: a new point of view, a new proof, a new theorem. (p. 79)

References

Review of *Perfect Rigor* by Masha Gessen

This book is a biography of Grigori (Grisha) Perelman, the Russian mathematician who is now famous for proving Thurston’s classification of 3-manifolds. (As a corollary, he proved the Poincaré conjecture–one of the outstanding open problems in mathematics.)

Thurston had conjectured, and proved in important special cases, that all 3-dimensional manifolds can be classified into combinations of 8 basic types, each of which can be represented geometrically using 3-dimensional non-Euclidean geometry. The simplest of these cases would just be the 3-sphere, which is the subject of Poincaré’s century-old conjecture.

In the course of telling Perelman’s life, Gessen tells much else that is of great interest. She leads us into the hidden inner life of “under cover” mathematics in the Soviet Union, including “special schools”, “math circles”, and “math clubs”. There, dedication to truth itself remained possible, for years on end, right under the noses of the Party and the KGB. All this was closely connected with the beneficent influence and inspiration of one man–Andrei Kolmogorov. He was of course a great international pioneer and researcher in many different fields of mathematics. But he was also the energizer and inspirer of a whole special Russian system of mathematical education and indoctrination for talented young people. Gessen paints an amazing portrait of him, hitherto quite unknown to me, including his long-time intimate friendship with the great topologist Pavel Sergeevich Alexandrov, and his dedication to an all-round life dedicated to beauty and refinement, both cultural and physical.

Masha Gessen has never met her subject, Grigori Perelman. Indeed, it seems that for a while now nobody at all has met him–except for his mother, Lyuba, who shares their modest apartment on the outskirts of St. Petersburg (the former Leningrad.) Gessen thinks that her never having met Perelman may have been an advantage in writing the book. She certainly seems to have met and thoroughly interviewed every major friend, acquaintance, and influence in Perelman’s life (except for his mother and his sister). As a result, she has been able to paint a convincing and fascinating psychological portrait of him, that makes credible and understandable his refusal of the Fields Medal and the Clay Prize, and even his present total withdrawal, not only from the mathematics community of Russia and of the world, but even from almost all human contact. This life story raises deep, disturbing questions, about the stresses and the values of a life entirely devoted to mathematics, especially in the world as it is today.

Grisha’s mother Lyuba herself is mathematically gifted. In fact, she declined the offer of a position as a graduate student of mathematics in Leningrad in order to give birth to and nurture her son Grigori. When Grigori was 10 years old, she went back to her mentor Professor Natanson, to tell him that her son was...
mathematically talented. Natanson sent Lyuba and Grisha to a certain Rukshin, a famous coach of mathematical problem-solving teams, and boss of a math club in St. Petersburg. It seems that Rukshin is more than just a famous math coach, he is the greatest math coach in the world. He has sent many contestants to the International Mathematics Olympiad. Rukshin and Grisha became inseparable companions. Under Rukshin’s coaching, Grisha actually did become one of the best, maybe the very best, mathematical problem solver in the world. First in Rukshin’s math club, and then in national and international competitions, Grisha seems almost never to have found a problem he couldn’t solve. In sessions of the math club, he sat quietly in the back. He was often the last to speak, for his solutions usually were clearly optimal. Nothing left out, nothing unnecessary, nothing open to challenge. While working on the problem, he might rub his leg, hum softly, or toss a ping pong ball back and forth. Not only did he solve the hardest problems, he then explained his solutions in perfectly clear, concise language to anyone who asked. His only difficulty seemed to be how to help anyone who failed to understand his clear explanation. In such a case, he seemed to have no recourse but to simply repeat the same explanation.

As a boy, Grisha was reasonably fit physically. At some meetings of Olympiad contestants, he played volley ball with the others. But his mental energy seems to have been totally focused on mathematics, from early childhood until maturity. Another geometer who was reported to have been Perelman’s friend while Perelman was living in the U.S. told Gessen that they often had conversations, and that the conversations were never on any topic except mathematics.

Perelman did have one setback. The first time he competed in the all-Russian mathematics Olympiad, he came in second. This was a very severe shock and disappointment. Gessen writes that Grisha decided that he hadn’t worked hard enough in preparation. He resolved never again to allow such a mishap to occur. In fact, it never did. He always came in first, before and after that one “failure.”

Like many other male mathematicians of relatively young years, Perelman gave little attention to matters of physical appearance. He always wore the same brown corduroy jacket. He did not waste time or effort about cutting his hair or his fingernails. With food also he preferred simplicity. It seems that while in the U.S. he rarely ate anything but bread and cheese. He did prefer one particular variety of black bread, which he procured, while living and working in New York, at a bakery on the far south side of Brooklyn, at Brooklyn Beach. He would walk there, an hour or so, after each day’s work at the Courant Institute in Manhattan.

Gessen’s book gives a rather brief treatment of the Poincaré conjecture itself. Many readers of this journal will know that the strongest attack on it had been made by Richard Hamilton of Columbia University. Hamilton used what he called “Ricci flow,” a non-linear parabolic partial differential equation satisfied by a certain geometrical quantity associated to a 3-dimensional manifold. The time-evolution of the solution to the equation describes a smoothing of an arbitrary 3-manifold. The smoothing action eventually would bring an arbitrary manifold to a form recognizable according to Thurston’s classification. However, before reaching that stage, the evolution could get stuck by encountering a geometrical singularity, one of several possible kinds of singularity. To get past such a singularity, it was necessary to perform what topologists like to call “surgery”—that is, a cutting and pasting operation which removes the singularity and renders the evolving manifold again
sufficiently regular. Hamilton was unable to show that such surgery was always possible. Perelman succeeded in doing so. Complex, detailed geometrical and analytical reasoning permitted Perelman to provide the necessary surgery instructions to complete Hamilton’s Ricci flow program, thereby proving both the Thurston Classification and the Poincaré Conjecture.

Perelman never submitted his solution for publication in a journal. He posted three announcements on a well-known site intended for such early warnings of new results. He never even announced that he had proved Thurston or Poincaré, merely that he had attained certain technical results about the Ricci flow. Those who were qualified to read his abstracts would understand the significance. Those who were not so qualified, need not attempt to read them.

Once the word got around the “Ricci flow community” and other interested topologists, mathematicians had to decide whether Perelman really had solved those problems. This was not very quick or easy, for his abstracts were concise, even in certain places perhaps a bit obscure. It took a year and a half for several teams of topologists to render the verdict—yes, he did it! During this process, Perelman spent time traveling the United States, giving talks, and answering questions. People found him well-prepared, patient and forthcoming.

It was always clear that this work was very likely going to win a Fields Medal and a Clay Prize. As Perelman traveled, giving talks at elite math departments, he received job offers, some very favorable. However, he expressed very little interest in any of them.

It is now clear that rather than being excited and flattered by this experience, Grisha was disappointed, repelled, perhaps even disgusted. This was not what he had expected, not what he was looking for.

Hamilton did not seek him out, did not express great enthusiasm or gratitude to him. Others who wanted to talk to him about job offers at high salaries for little work did not seem to have even studied or understood his mathematical work. In fact, Grisha was becoming a celebrity, something it seems he had never sought, expected, desired, understood, or valued. His celebrity status, even within the academic community, seemed to outweigh and overbalance the actual content of his mathematical achievement. To Grisha, this was unattractive, unpleasant, even immoral.

He practiced mathematics only for its own sake, he believed in mathematics only for its own sake. Mathematics for the sake of fame, money, or power were alien to him, perhaps even incomprehensible. Certainly alien, repellent. Unclean. Degenerate.

In Russia also there were unpleasant incidents involving money, and horrible surrounding incidents by the Russian press media. Grisha quit his position at the Steklov Institute. There was a kind of embarrassment—I wouldn’t say a scandal—when Shing-Tung Yau, one of the greatest living geometers, seemed to try to squeeze some of the credit for the proof of the Poincaré conjecture from Grisha toward two of his protégés—possibly for the sake of political clout in the People’s Republic of China. Then Sylvia Nasar and David Gruber managed to get Grisha to spend time with them in St. Petersburg, and published a somewhat sensational article in the New Yorker. Of course Grisha refused the Fields Medal, refused to attend the International Congress of Mathematicians, and finally refused ONE MILLION DOLLARS from the Clay Institute. In her book, Masha Gessen reports that Grisha...
has now broken off from his lifelong friend and mentor Rukshin. He has told people he is looking for something new to do, instead of mathematics. He continues to live in their apartment with his mother.

Masha Gessen devotes one chapter of her book to the topic of Asperger’s disease, a form of autism disproportionately found among mathematicians. She never actually suggests that Grisha Perelman suffers from Asperger’s. Whether he does or not is a medical question. But there is an issue here of good taste, and good manners. People may wonder about such things, and talk about them privately. Decent consideration for the feelings of the subject of her book would have suggested abstaining from publishing such a chapter.

Much more important is the cultural and moral question, which this story forces one to ask. Does today’s world have room for a mathematician who practices mathematics for its own sake, and only for its own sake?
Review of *Letters to a Young Mathematician*  
by Ian Stewart

Dear grandchildren, David, Jessica, and Ze’ev,

As is customary, I list you in chronological order, by your year of birth. As all of you probably know, your grandfather in New Mexico is a retired math professor. If it should some day happen, by some good book or some good teacher, that one or two of you get turned on to math, I’ll be more than eager to help you, with my information and my advice. In all fairness then, I am letting you know that the new book by my friend Ian Stewart already offers such information and advice. Therefore, for you three, and also for any other potential readers of this prestigious periodical, I am going to tell you about Stewart’s book.

First I’ll tell you about his information. Then I’ll tell you about his advice. And finally, I’ll tell you my own advice.

The book consists of 21 letters to “Meg.” The quotation marks inform us, I suppose, that “Meg” is imaginary or fictitious. In the first letter “Meg” is “at school.” Since Ian doesn’t hesitate to speak seriously and deeply to “Meg”, I guess she’s already in what we here in the States call “high-school.” Meg is wondering what mathematicians do, and how her Uncle (?) Ian became a mathematician. As time passes, Meg chooses to study math(s) at University. By the last letter, she’s concerned with what a tenure-track Assistant Professor is concerned with: the mad struggle to attain tenure. (“Tenure” is the college professors’ word for “job security”.)

I am sure you, or anyone interested in mathematical life today, will find the “letters” interesting and enjoyable. Stewart freely confides to Meg some of his own personal story, of how he was drawn to mathematics, and of some of his pleasures and successes as a mathematician. There is a really wonderful account of how an investigation into the abstract theory of groups turned out to be of great use in analyzing the gait of four-footed creatures, like dogs!. Who knew that there was even such an academic specialty as “Gait Studies”?  

Several of the chapter titles tell enough to make clear their messages: “The Breadth of Mathematics,” “Hasn’t it All Been Done?” “How Mathematicians Think”, “How to Learn Math”, “Fear of Proofs”, “Can’t Computers Solve Everything?” “Impossible Problems.” Every sentence is clear and comprehensible. The love of mathematics that impels Stewart is always there; if the reader is susceptible at all, she or he may well become infected.

Starting with Chapter 14, and going on to Chapter 20, the next to last, there is a noticeable change of tone and focus. “The Career Ladder”, “Pure or Applied,” “Where Do You Get Those Crazy Ideas?” “How to Teach Math,” “The Mathematical Community,” “Perils and Pleasures of Collaboration.” Ian is no longer talking
to a child, sharing his enthusiasm and enjoyment. He is talking to someone who is committed to mathematics, and is worried about how to make a living at it.

Of course, there is a very realistic kind of conversation to imagine. In fact, many senior mathematicians, responsible for guiding advanced undergraduates, graduate students, postdocs, and faculty just starting to teach, do have such conversations many times over their teaching careers. I imagine that while the “Meg” of the first half of the book is partly based on real acquaintance with school children, and partly a creation of Stewart’s imagination, the “Meg” of the second part may well be an amalgam of many young mathematicians Stewart has counseled.

So what kind of advice does he give? I would say, sound and sober advice. Realist advice, how to get on in the mathematical world as it really is. (Meaning, of course, not necessarily as we would most, in our heart of hearts, desire it to be.) Stewart knows what’s what, and he most kindly and sincerely wants Meg to make it, to get that job and that tenure. That means, knowing what hiring committees look for, and what promotion and tenure committees look for, etc. etc. etc. “THE REAL WORLD.”

So, very good, what could be wrong with that?
Nothing at all.

And yet I can’t help remembering a young English mathematician I met years ago who did some crazy things. For instance, he wrote and illustrated COMIC BOOKS about fractals and chaos! And since, I guess, he couldn’t get them published in English, he published them in France, IN FRENCH! He even gave me copies of those two wonderful works of his.

That wasn’t all that he did which some would have considered ill-advised. A “hot” new specialty in mathematics appeared with a wonderful name: “Catastrophe Theory.” My English friend became an active worker in this new field, and an active public advocate of it, even though everyone knew that it was controversial. Many influential senior mathematicians disliked it, considered it a shallow fad, vastly over-publicized because of its exciting name. Many of his older friends must have had doubts whether this was really the wisest career move he could make.

Of course, he also did fine work in other noncontroversial specialties. But he also did something much more unwise. He knew, of course, that many research mathematicians don’t have the highest admiration or respect for journalists. And that to many people high up in mathematics, popular books that can be understood by anybody are hardly above journalism. And yet, what did he do but write lots of popular books! Over 20 are listed in the front matter of “Letters to a Young Mathematician.”

I would say that the career of Ian Stewart is the career of a supremely successful mathematician whose first concern does not seem to have been “playing it safe.”

So, now I am ready to offer my own advice. I have tried to tell you about Stewart’s advice. You may know, from experience or from hearsay, about a parent who admits that he isn’t in all ways an ideal role model for his child. He may admit to some weaknesses or even vices. But, “Child,” says he, “do as I say, not as I do.” I am enough of an optimist to look at things the opposite way. Read Stewart’s book, enjoy it, but do as he Does, not as he Says.
Review of *Number and Numbers*  
by Alain Badiou

The name “Alain Badiou” may be unfamiliar to some readers of *The Mathematical Intelligencer*, but Slavoj Zizek calls Badiou “much more than the most influential French philosopher at this moment,” and his work “announces a new epoch in “philosophy”” (back cover.) Zizek, of course, is the “most formidably brilliant recent theorist to have emerged from Continental Europe” (*The International Encyclopedia of Philosophy*).

To readers of the *New Left Review*, Badiou is well known as a post-Maoist revolutionary thinker. After retiring from the École Normale Supérieure and the Collège Internationale de Philosophie, Badiou became affiliated with the European Graduate School in Saas-Fee, Switzerland. Although this isn’t a new book, it’s newly translated into English. As far as I have been able to learn, it has so far received little notice in the Anglophone world, either by mathematicians or philosophers. An excellent review by John Kadvany did appear in the *Notre Dame Philosophical Review*.

Badiou isn’t what Anglophone academia calls a “philosopher of mathematics.” He pays no attention to old bickerings between Brouwer and Hilbert or Quine and Carnap. He’s after bigger fish, as they say. His question isn’t, “What is mathematics?” but rather, “What is Being?” And his answer is, “Being is Mathematics.” The big news, in brief, is that Alain Badiou is in love with John Conway’s surreal numbers!

Badiou is not a deconstructivist or postdeconstructivist. He’s a metaphysician, a creator of speculative systems in the tradition of Leibniz, Hegel and Heidegger. His concerns are Being and Event. Being, I think, is roughly the same as “All that Is” or perhaps “Pure Existence” or simply “Absolute Reality.” “Event,” on the other hand, seems to mean, I think, the unpredictable inexplicable radical break from Being, which is exemplified by the sacred and ineffable highest moments of Art, Science, Love, or Revolution. Badiou’s earlier masterpiece, *Being and Event*, helpfully includes a dictionary. I found no entry for “Being,” but here is the definition of “Event”.

An event—of a given evental site—is the multiple composed of: on the one hand, elements of the site; and on the other hand, (the event).—Self-belonging is thus constitutive of the event. It is an element of the multiple which it is.— The event interposes itself between the void and itself. It will be said to be an ultra-one (relative to the situation) (pp. 506-507)

If this sounds quite unfamiliar, it may in part be because Anglophone philosophy has for a century or so been controlled by the descendents of Bertrand Russell,
who practice something called Analytic Philosophy, which aspires to be Scientific, is obsessed with Logic and Language, and has long ago kicked Metaphysics, including Ontology, into the garbage can. But Badiou is practicing Ontology and Metaphysics! Not, however, in the traditional vein of Hegel or Heidegger—he does it with Mathematics. He is after a version of the surreal that doesn’t show any trace of human hands, a version that one can believe is eternal, extra-human—pure Being.

This book starts out with interesting philosophical summaries and critiques of Frege, Dedekind, Peano and Cantor. There follows a careful and, so far as I can tell, correct presentation of the system of ordinal numbers, using the construction often attributed to von Neumann. Starting with the first ordinal, as represented by the singleton \{\Phi\}, one then gets the second ordinal, with its representative \{\Phi, \{\Phi\}\}, and then continues to build the next ordinal by adjoining to any given ordinal a new, final element, namely, itself. After one gets up to the familiar ordered set “\omega” of natural numbers, comes the decisive step: one constructs the next ordinal, by introducing a new element as the last element—namely, \omega itself! Then begin again, and continue, defining, after any given ordinal \(X\), the next ordinal, namely: \(\{X, \{X\}\}\). And again, after doing this a countably infinite number of times, create a new “limit ordinal” by defining a new last element following this new countable infinity.

This construction is explained in five chapters, with admirable detail and patience. I would be tempted to recommend it for beginning students of set theory, except that they would be deterred, not to say repelled, by Badiou’s extravagant Heideggerian metaphysical language.

But, you say, what does this standard set-theoretic material have to do with Conway’s surreal numbers? As presented by Conway, the surreal numbers don’t seem at first to be about the ordinals. They’re about the “cut”—Dedekind’s famous trick, by which he created the real numbers out of the rationals. Conway starts with NOTHING, and uses a kind of cut to create 0. Then, cutting away, he gets 1, and −1, and the integers, and the dyadic fractions, and finally, of course, like Dedekind, the real numbers. But why stop there? Make one more cut—zero on the left, and the positive reals on the right—and what do you have? An infinitesimal, of course. Contrariwise, make a cut with all the reals on the left, and what do you have? A positive infinite surreal number! Go all the way, cut as many times as there are ordinal numbers. You get a new incredibly rich and complex number system—the surreals.

These surreals are what Badiou wants, but he doesn’t want them in this step-by-step, bottom-up ingenious and elegant constructive fashion of Conway. No, Badiou has a metaphysical ax to grind, an ontology to establish (as well as a political-social agenda).

“Our philosophical project designates where Number is given as the resource of being within the limits of a situation, the ontological or mathematical situation. We must abandon the path of the thinking of Number followed by Frege or Peano, to say nothing of Russell or Wittgenstein. We must even radicalise, overflow, think up to the point of dissolution, Dedekind’s or Cantor’s enterprise.” (p. 212)

“If we truly wish to establish the being of number as the form of the pure multiple, to remove it from the schoolroom (which means also to subtract the concept from its ambient numericality), we must distance ourselves from operational
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and serial manipulations. These manipulations, so tangible in Peano, project onto the screen of modern infinity the quasi-sensible image of our domestic numbers, the 1, followed by 2, which precedes 3, and then the rest. The establishing of the correct distance between thought and countable manipulations is precisely what I call the ontologisation of the concept of number. From the point at which we presently find ourselves, it takes on the form of a most precise task: the ontologisation of the ‘universal’ series of the ordinals. To proceed, we must abandon the idea of well-orderedness and think of ordination, ordinality, in an intrinsic fashion. It is not as a measure of order, nor of disorder, that the concept of number presents itself to thought. We demand an immanent determination of its being. And so for us the question now formulates itself as follows: which predicate of the pure multiple, that can be grasped outside of all serial engenderment, founds numericality? We do not want to count, we want to think the count.” (p. 58)

Since Badiou rejects the bottom-up constructive point of view, and since he dislikes, not to say despises, the view of mathematics as a calculus, he is lucky that Harry Gonshor provided an exposition of the surreals that takes the ordinals as given, and then defines a surreal number as a mapping of an initial segment of the ordinals into the pair \{+, −\}. (The empty sequence is included as a possibility.) To understand this, first imagine the familiar binary expansions of the reals to be “continued” or “extended,” past omega, all the way through the ordinal numbers. Then, for example, we would get the first infinitesimal, as the binary sequence which is 0 in all the finite positions, and 1 in the final position that comes after all the finite positions. Now to get the surreals as Gonshor does them, replace 1 and 0 by + and −. Having presented the surreals in the Gonshor way, as \{+, −\} valued sequences (up to arbitrarily far out in the ordinals), Badiou is ready for his big coup. He defines the surreal numbers in a way intrinsic to Being, free from any construction or representation! How does he do it? I’m afraid his answer is quite a let-down. A simple trick. He says a surreal number is just a pair \{A, B\} where A is any ordinal number, and B is any subset of A. How does this work? Well, to get back from Badiou to Gonshor, take the subset B as the places in A which receive a +, and take the rest of A (the complement of B with respect to A) as the places which receive a −. In the opposite direction, starting with a surreal number given, a la Gonshor, as a mapping of an initial segment of some ordinal A into the pair \{+, −\}, you can elevate it to the high metaphysical level of a Badiou surreal \{A, B\}, simply by choosing as B the subset of A which receives the value +.

This simple relabeling is presented by Badiou as a deeply significant achievement in the understanding of not just Number, but of Being itself! Why? Because, for Badiou, instances of Being are Multiplicities, and Multiplicities are completely catalogued and described by ordinal numbers and surreal numbers.

I have two distinct objections to this claim of Professor Badiou. My first objection is based on the fact, admitted by him, that the construction of the ordinals, as also the construction of the surreals, is not compelled by either experience or logic; it is a decision that “we” make. “We” could decide otherwise. My second objection is based on the fact that any well-ordered set, or number system based on a well-ordered set, is grossly inadequate and insufficient to represent, describe or model Being, Reality or All That Is.

As to the first objection, I quote Badiou: “There exists no deduction of Number, it is solely a question of a fidelity to that which, in its inconsistent excess,
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is traced as historical consistency in the interminable movements of mathematical refoundations... These concepts arise from a decision whose written form is the axiom; a decision that reveals the opening of a new epoch for the thought of being qua being...” (pp. 212-213). Prof. Badiou is right. Going beyond the countably infinite is just a decision, Cantor’s decision and “our” decision. “Our” choice, to extend Number by the repeated use of limit ordinals, thereby creating the whole system of ordinal numbers, is not compelled, by either experience or logic. It is merely “our” decision, and “we” could decide otherwise. But this admission destroys the whole claim that Being is manifested in the ordinals and the surreals! If one had the privilege of presence at Prof. Badiou’s lecture, one would try to ask a question: “Since this decision is ‘our’ free, arbitrary choice, how can it claim to be a mirror or picture of Being, that which IS, regardless of ‘us’ and independent of ‘us’?”

Now to the second, and even more fundamental objection. What is the basis for Prof. Badiou’s claim that the surreal numbers suffice to represent or depict Being itself? Here his dictionary is helpful. One of his important terms is situation. I take “situation” to stand for any concrete specific manifestation of “Being.” “Being” perhaps is just the sum or union of all possible “situations,” and “situation” is the “Being” that is present perhaps at any particular time and place. On p. 522 of Being and Event, we find a definition: “Situation. Any consistent presented multiplicity, thus a multiple, and a regime of the count-as-one, or structure.” We also have a definition of structure. “What prescribes, for a presentation, the regime of the count-as-one. A structured presentation is a situation.” And “Multiplicity, multiple: General form of presentation, once one assumes that the One is not” (p. 514).

This seems to me to be the key fallacy of Badiou: This bare statement that the general form of every presentation is multiplicity. Badiou seems to actually say that the Multiplicity of a Situation is a complete description or specification of it! On the contrary, even a mathematical situation beyond abstract set theory is described mainly by the relations, the operations, which are defined on some set. So much more so is any “real-world” situation described by many more attributes than its mere multiplicity! Take these three “situations”: a bowl of 9 apples, or the first 9 prime numbers, or a session of the 9 members of the U.S. Supreme Court—all equivalent, with respect to multiplicity. But by saying that a situation is simply a multiplicity, Badiou can say that the surreal numbers are sufficient for a complete description of Being, and indeed are already an aspect of Being—indeed, independent of “our” knowledge or understanding, now or ever at any time. He has characterized Being above and beyond all human knowledge!

To my mind, this one-dimensional reduction of reality to a well-ordered set is embarrassingly simplistic. Indeed, it is in essence already too familiar, as a way of caricaturing reality. Anyone acquainted with Marxist analysis will recognize its similarity to the one-dimensional universal ranking of everything by Price, which is the essence of the “Free Market,” the reign of Capital. Yet it is the reign of Capital that Badiou imagines he repudiates! Badiou repeatedly explains the opposition between “number” (small n) and “Number” (big N). By Number (big N), he means the surreals, in his metaphysical-ontological representation. They are admirable and excellent. By the numbers (little n), he means the numbers that Capital uses to oppress us, in its commercial-militaristic degradation of humanity. “In our situation, that of Capital, the reign of number is thus the reign of the unthought
slavery of numericality itself.” (p. 213). Well and good. But according to a well-established Marxist insight, Capital brings the commodification of all aspects of human life. It puts a price on everything, thus making any two entities comparable in value. If we reject this commodification, this putting a price on everything, then we escape the one-dimensional thinking of our time, as manifested, for instance, in ranking everything (even mathematicians), or in the tyranny of grades and tests in school, of IQ in psychology, etc. Real situations are not one-dimensional, they are multi-dimensional, even infinite-dimensional. Mathematics used in a serious way to study non-mathematical reality cannot limit itself to any one-dimensional scale, no matter how extended or how refined!

If it weren’t presumptuous to advise the most influential philosopher in France, I’d be tempted to suggest that Professor Badiou go beyond the set theory which he has mastered so well. How much benefit he would gain by learning some geometry, or even by just leafing through the beautiful new Princeton Companion to Mathematics! If he checks around among the mathematicians there in Paris or Switzerland, he will find that our best attempts to model Reality (or Being) require all the resources of mathematics as it has advanced so far. Set theory, and even the surreal numbers, impressive and beautiful as they are, constitute but one small sector of the vast field of mathematical tools and concepts that Being demands of us.

I see Badiou as a modern Pythagorean using the latest incarnation of Number to provide objects of adoration. He calls himself a Platonist, but not a religious Platonist—a Materialist Platonist. (Multiplicity is a material phenomenon, you see.) Nevertheless, to me his rhapsodic “Meditations” on Set and Number are a bit reminiscent of Georg Cantor, who knew that his mathematical infinite was the theological infinite of the Lord God.

References

Part 4

About the Author
An amusing elementary example

Here is an amusing contribution at high-school level, published in the *College Math Journal* in 2012 (PP1). Recall the familiar story, how as a child Carl Friedrich Gauss discovered that the sum of the first 100 natural numbers is \((100 \times 101)/2\). Instead of 100, take the first \(n\) numbers, then the sum is \(n(n+1)/2\). Let me call this expression “\(S(n)\)”.

In high-school textbook chapters on mathematical induction, the student proves that the sum of the first \(n\) squares is \(S(n)(2n+1)/3\), and the sum of the first \(n\) cubes is \([S(n)]^2\). But never anything about the sums of higher powers! As a matter of fact, the appearance of \(S(n)\) in the sums of the squares and the cubes is “no accident”. Two patterns hold for all sums of powers of integers. For odd \(p\), the sum is a polynomial in \(S(n)\), of degree \((p+1)/2\). For even \(p\), it is \((2n+1)\) times a polynomial in \(S(n)\) of degree \(p/2\). These facts can be proved by mathematical induction, but here is a much easier proof. Make a list of the sum of \(p\)’th powers, moving to the right by successive additions: \(1, 1+2^p, 1+2^p+3^p, \ldots\). Then extend this list to the left, by successive subtractions! The meaning of the entries, as the sum of \(n\) terms, is lost, of course, when \(n\) equals \(0, -1, -2, -3\). But that’s OK, we are looking for information about a certain polynomial in \(n\), of degree \(p + 1\). A glance at this bi-directional list yields a surprise. This polynomial in \(n\) is symmetric around the value \(n = -1/2\), and the symmetry is even or odd, depending on the parity of \(p\). From this, the expression in terms of \(S(n)\) follows readily. These polynomials are now called “Faulhaber polynomials,” because Johannes Faulhaber in 1615 somehow had already discovered these facts.

Equally elementary is my factor-theorem derivation of Heron’s area theorem, presented in articles *How mathematicians convince each other* and *On the interdisciplinary study* above and in articles H1, H2. The complicated history of the analogous volume formula for the volume of a tetrahedron is presented in H3.
This annotated research bibliography is somewhat unrelated to the main content of the present work. I include it here simply because this is an opportunity for me to make these works more accessible to any readers who may be interested in these areas of analysis (PDE, linear operators, stochastic processes).

My thesis naturally led into half a dozen related investigations. A well-understood, highly developed tool for the initial-value problem was the Fourier transform, which replaces the operation of differentiation, in the spatial variable, by an operation of multiplication, in a new “transform” variable. A system of partial differential equations with constant coefficients, in one, two, three or more spatial dimensions, to be solved for positive time, becomes a system of ordinary differential equations in the time variable, depending on “parameters” which are the new “transform” variables. We can write down an “explicit” solution formula for this system of ordinary differential equations. We then must apply an “inverse Fourier transform” to this formula, to solve the original problem. But this method works only for a problem defined in all of unbounded space. Realistic problems usually involve regions with boundaries and boundary conditions. Such problems were well understood for simple, traditional boundary conditions. Exactly which boundary conditions define a unique solution? I solved this problem by combining a Fourier transform in all the unbounded spatial variables with a Laplace transform applied to the time variable for the simplest possible region with boundary—namely, a half-space. One is left again with just a single differential operator, but now the differentiation is not in the time variable, but in the space variable perpendicular to the boundary. One again has a system of ordinary differential equations, with an explicit solution formula, but no longer an initial-value problem. It is a pure boundary-value problem, which becomes well-defined by appropriate restriction “at infinity,” (far to the right, if we situate the boundary plane on the left). The crucial question again is to perform the inverse transformations, both Fourier and Laplace inversions. The “correct” or “admissible” boundary conditions are the ones that define an invertible solution to this transform problem. This was good enough to get me my Ph.D.

The problem I had solved was for a first-order hyperbolic system. It was immediately clear to me that the assumption of “first-order” could be dropped. But I was startled to realize that the “hyperbolic” hypothesis is equally redundant! It was well known that initial value problems for “parabolic” equations, like the diffusion or heat equation, are well posed, along with hyperbolic ones like the wave equation. My method worked even more generally. For any system of equations for which the pure initial value problem is “well posed”—systems bearing the cumbersome name of “correct in the sense of Petrowsky”—I identified all correct boundary conditions and gave a solution formula for such mixed initial-boundary value problems. Then
I found out that this problem had been stated as an interesting open question, by the great and famous Israel Moiseyevich Gel’fand.

I worked out the details for physically interesting examples, discovering a distinction, among correct boundary conditions, between “hyperbolic” ones (preserving the finite-speed-of-propagation property) and other correct but non-hyperbolic ones (PDE3). I found and solved the correct “transmission problems” between two different well-posed systems in complementary half-spaces (another problem of Gelfand.) (PDE4) At Peter Lax’s suggestion, I used the same technique for the stability-convergence problem for finite difference schemes associated to well-posed initial value problems (PDE6). I wrote an accessible exposition (PDE7) of these results. Much later, I returned to the subject of finite difference approximation. Jointly with Tosio Kato (LO4), I presented finite difference approximations of arbitrarily high accuracy, for any well-posed initial-value problem, using an abstract version of the Padé table of rational approximations to the exponential function.

I collaborated with Y.W. Chen to give solvability and unsolvability results for the Goursat problem (characteristic quadrant) for the wave equation in 2 and 3 dimensions. (PDE5). For a Festschrift honoring Chen, I contributed a paper (PDE10) which generalizes the elementary method of reflection, from the one-dimensional wave equation to general well-posed evolution equations, in a slab bounded by two parallel half-spaces. An additional algebraic condition, besides well-posedness, is shown to be necessary and sufficient for convergence of the series of reflected “waves”.

In PDE8 and, with J. Donaldson, PDE10, I applied operator-theoretic techniques from papers in the LO sequence to give formulas for variable-coefficient parabolic equations. PDE11 is an accessible introduction to the classification of higher order PDE’s. PDE13 is an exposition of theorems about hyperbolic systems obtained by the method of random evolutions. PDE12 systematizes and generalizes, by means of operator-valued integral transforms, a collection of previously unrelated and classical “transmutation” formulas, reducing complex or singular initial value problems to simpler, regular ones. PDE14, with Stan Steinberg, connects my previous LO results to Lie groups and algebras to solve certain classes of variable-coefficient hyperbolic equations.

Stimulated by the challenge of teaching a probability course, I came up with a problem unrelated to my thesis, or to any of the interests of my mentor Peter Lax. Noticing that the famous “law of large numbers” and “central limit theorem” can be stated in terms of a parameter, which takes on the value 1 for the law of large numbers, and 2 for the central limit theorem, I found ways to let that parameter be any positive whole number n. Then, as long as n is not a multiple of 4, there is a limit theorem generalizing the two famous classical ones! (PR1) I felt good to state and solve my own problem, like a grown-up mathematician, no longer a mere student.

The random evolution idea I developed with Richard Griego and then with other probabilists was inspired by an example of Mark Kac. A particle moving at fixed speed on a line, and suffering reversals of direction according to a Poisson process, has an expected position satisfying the telegraph equation! This was remarkable, as an example of a hyperbolic equation with a stochastic solution. We generalized this model to an abstract evolution governed by several different generators, and switching from one to the other stochastically. If the switching process is
Markov, the expected outcome satisfies a deterministic differential equation, generalizing the telegraph equation. By suitable scaling with small parameters, we were able to use operator versions of the central limit theorem to prove singular perturbation results for PDE’s. In particular, we proved that an abstract diffusion equation is the limit of general or abstract transport equations (RE1 and RE2). Working with Mark Pinsky, I proved (RE3) analogous singular perturbation results for n-component hyperbolic systems. With George Papanicolaou (RE4) I proved an abstract central limit theorem for evolutions switching by a Markov process between different generators, without assuming commutativity. We used a recurrence argument. Then, with Bob Cogburn (RE5), this was extended from a Markov chain to a general “mixing condition” choosing among an infinite set of generators. We obtained both first-order (“law of large numbers”) and second-order (“central limit theorem”) types of results, in this most general possible setting. Later on, Richard Griego and I (RE7) used random evolutions to get theorems about the eigenvalue spectra of certain Hilbert space operators. I wrote two expository papers (RE6 and RE8) on this whole development. RE8, in the Mathematical Intelligencer, contains a long list of papers applying the random evolution model, and it includes the work of the Kiev school, who studied random evolutions governed by a semi-Markov process. The PDE applications were presented in PDE13.

The random evolution limit results were expressed in the form of operator-valued integral transforms. These are interesting for their own sake. The following striking result is actually very simple to prove (LO1): A concrete initial value problem for a partial differential equation with constant coefficients in one space variable can be generalized to an abstract initial-value problem by replacing the spatial differential operator by an arbitrary linear operator. If this linear operator is the generator of a group and the concrete initial-value problem was well-posed (including but not limited to the hyperbolic or parabolic types), then the resulting abstract initial-value problem is also well posed, and an explicit solution formula is presented. For a concrete initial-value problem in several space variables, the same is true if the several spatial differential operators are replaced by several mutually commuting abstract group generators. The commutativity condition can be weakened; it is sufficient that the several abstract operators together generate a Lie group (PDE14). This was accomplished in collaboration with Stan Steinberg. With Archie Gibson and Jim Donaldson (LO3), I proved a kind of “invariance principle” of interest in mathematical physics.

There are several other papers in probability. PR3 shows that the classical “capacity coefficients” or “Maxwell coefficients” in potential theory equal the probabilities that a Brownian particle escaping from the j′th boundary component is captured at the k′th. PR5 (with Priscilla Greenwood) and PR6 use non-standard analysis to study Brownian motion as an infinitesimal random walk.

Poems

In the last few months, I have started writing poetry again, after a gap of many years. I am producing short, mostly unrhymed pieces, with an iambic beat. Chandler Davis, one of my oldest friends and the long-time editor of The Mathematical Intelligencer, accepted seven of them. Here is one that he chose:

George Polya was blind his last few years.
Kolmogorov too.
Lie down, lie down, old plowman!
Life is cruel, death is kind.
And deaf and dumb.
And blind.

Here is another, hitherto unpublished:

FROM EUCLID OF ALEXANDRIA, TO EDNA ST. VINCENT MILLAY, OF NEW YORK
I alone have looked on beauty bare, you say?
Nice compliment!
But pardon me, what is your Result?
And where’s your Proof?

I take this opportunity to announce a forthcoming book, a biography of my mentor Peter Lax. His life illuminates a methodological issue—the partnership and rivalry between pure and applied mathematics.
Curriculum Vitae

Born New York City, 1927
Lloyd McKim Garrison Prize for poetry by an undergraduate, 1944 and 1945
B.A. English Literature, Harvard, 1946
Ph.D., Mathematics, NYU (Peter Lax), 1962
National Book Award (with Philip J. Davis), The Mathematical Experience
[Birkhäuser, 1983]
MAA Chauvenet Prize (with Martin Davis), “Hilbert’s tenth problem” [Scientific American, November 1973, pp. 84–91]
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DOCTORAL STUDENTS: Larry Bobisud, Maria del Carmen Jorge, Crepin Mahop, Susan Nett, Walter Roth, Andy Schoene, Steve Wollman

MENTORS: Peter Lax, Einar Hille, Gian-Carlo Rota, Hao Wang, Mark Kac


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The question “What am I doing?” haunts many creative people, researchers, and teachers. Mathematics, poetry, and philosophy can look from the outside sometimes as ballet en pointe, and at other times as the flight of the bumblebee. Reuben Hersh looks at mathematics from the inside; he collects his papers written over several decades, their edited versions, and new chapters in his book Experiencing Mathematics, which is practical, philosophical, and in some places as intensely personal as Swann’s madeleine.

—Yuri Manin, Max Planck Institute, Bonn, Germany

Most mathematicians, when asked about the nature and meaning of mathematics, vacillate between the two unrealistic poles of Platonism and formalism. By looking carefully at what mathematicians really do when they are doing mathematics, Reuben Hersh offers an escape from this trap. This book of selected articles and essays provides an honest, coherent, and clearly understandable account of mathematicians’ proof as it really is, and of the existence and reality of mathematical entities. It follows in the footsteps of Poincaré, Hadamard, and Polya. The pragmatism of John Dewey is a better fit for mathematical practice than the dominant “analytic philosophy”. Dialogue, satire, and fantasy enliven the philosophical and methodological analysis.

Reuben Hersh has written extensively on mathematics, often from the point of view of a philosopher of science. His book with Philip Davis, *The Mathematical Experience*, won the National Book Award in science. Hersh is emeritus professor of mathematics at the University of New Mexico.