What is Mathematics, Really? Reuben Hersh, 1997

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Intuition/Proof/Certainty

There's an old joke about a theory so perfectly general it had no possible application. Humanist philosophy is applicable.

From the humanist point of view, how would one investigate such knotty problems of the philosophy of mathematics as mathematical proof, mathematical intuition, mathematical certainty? It would be good to compare the humanist method with methods suggested by a Platonist or formalist philosophy, but I am really not aware how either of those views would lead to a method of philosophical investigation. A humanist sees mathematics as a social-cultural-historic activity. In that case it's clear that one can actually look, go to mathematical life and see how proof and intuition and certainty are seen or not seen there.

What is proof? What should it be? Old and difficult questions.

Rather than, in the time-honored way of philosophers, choose a priori the right definitions and axioms for proof, I attempt to *look carefully, with an open mind.* What do mathematicians (including myself) do that we call proving? And what do we mean when we talk about proving? Immediately I observe that "proof" has different meanings in mathematical practice and in logico-philosophical analysis. What's worse, this discrepancy is not acknowledged, especially not in teaching or in textbooks. How can this be? What does it mean? How are the two "proofs" related, in theory and in practice?

The articles on intuition and certainty, and half a dozen more in the next chapter, are in the same spirit.

"Call this philosophy? Isn't it sociology?"

The sociologist studying mathematics comes as an outsider—presumably an objective one. I rely, not on the outsider's objectivity, but on my insider's experience and know-how, as well as on what my fellow mathematicians do, say, and write. I watch what's going on and I understand it as a participant.

This is not sociology. Neither is it introspection, that long discredited way to find truth by looking inside your own head. My experience and know-how aren't isolated or solipsistic. They're part of the web of mutual understanding of the mathematical community.

There used to be a kind of sociology called "Verstehen." That was perhaps close to what I've been talking about. But my looking and listening with understanding isn't an end in itself. It's a preparation for analysis, criticism, and connection with the rest of mathematics, philosophy, and science. That's why I call it philosophy.

Proof

The old, colloquial meaning of "prove" is: *Test, try out, determine the true state of affairs* (as in Aberdeen *Proving* Ground, galley *proof*, "the *proof* of the pudding," "the exception that proves the rule," and so forth.) How is the mathematical "prove" related to the old, colloquial "prove"?

We accuse students of the high crime of "not even knowing what a proof is." Yet we, the math teachers, don't know it either, if "know" means give a coherent, factual explanation. (Of course, we know how to "give a proof" in our own specialty.)

The trouble is, "mathematical proof" has two meanings. In practice, it's one thing. In principle, it's another. We *show* students what proof is in practice. We *tell* them what it is in principle. The two meanings aren't identical. That's O.K. But *we never acknowledge the discrepancy.* How can that be O.K.?

Meaning number 1, the *practical* meaning, is informal, imprecise. *Practical mathematical proof is what we do to make each other believe our theorems.* It's argument that convinces the qualified, skeptical expert. It's done in Euclid and in *The International Archive Journal of Absolutely Pure Homology.* But what is it, *exactly*? No one can say.

Meaning number 2, theoretical mathematical proof, is formal. Aristotle helped make it. So did Boole, Peirce, Frege, Russell, Hilbert, and Gödel. It's transformation of certain symbol sequences (formal sentences) according to certain rules of logic (modus ponens, etc). A sequence of steps, each a strict logical deduction, or readily expanded to a strict logical deduction. This is supposed to be a "formalization, idealization, rational reconstruction of the idea of proof" (P. Ernest, private communication).

Problem A: *What does meaning number 1 have to do with meaning number 2?*
Problem B: *How come so few notice Problem A?* Is it uninteresting? Embarrassing?
Problem C: *Does it matter?*

Problem C is easier than A and B. It matters, morally, psychologically, and philosophically.

When you're a student, professors and books claim to prove things. But they don't say what's meant by "prove." You have to catch on. Watch what the professor does, then do the same thing.

Then you become a professor, and pass on the same "know-how" without "knowing what" that your professor taught you.

There is an official, standard viewpoint. It makes two interesting assertions.

Assertion 1. Logicians don't tell mathematicians what to do. They make a theory out of what mathematicians actually do. Logicians supposedly study us the way fluid dynamicists study water waves. Fluid dynamicists don't tell water how to wave. They just make a mathematical model of it.

A fluid dynamicist studying water waves usually does two things. In advance, seek to derive a model from known principles. After the fact, check the behavior of the model against real-world data.

Is there a published analysis of a sample of practical proofs that derives "rigorous" proof as a model from the properties of that sample? Are there case studies of practical proof in comparison with theoretical proof? I haven't heard of them. The claim that theoretical proof models practical proof is an assertion of belief. It rests on intuition. Gut feeling.

You might say there's no need for such testing of the logic model against practice. If a mathematician doesn't follow the rules of logic, her reasoning is simply wrong. I take the opposite standpoint: What mathematicians at large sanction and accept *is* correct mathematics. Their work is the touchstone of mathematical proof, not *vice versa*. However that may be, anyone who takes a look must acknowledge that formal logic has little visible resemblance to what's done day to day in mathematics.

It commonly happens in mathematics that we believe something, even without possessing a complete proof. It also sometimes happens that we *don't* believe, even in the *presence* of complete proof. Edmund Landau, one of the most powerful number theorists, discovered a remarkable fact about analytic functions called the two constants theorem.** He proved it, but couldn't believe it. He hid it in his desk drawer for years, until his intuition was able to accept it (Epstein and Hahn, p. 396n).

There's a famous result of Banach and Tarski,** which very few can believe, though all agree, it has been proved.

Professor Robert Osserman of Stanford University once startled me by reporting that he had heard a mathematician comment, "After all, Riemann never proved anything." Bernhard Riemann, German (1826—1866), is universally admired as one of the greatest mathematicians. His influence is deeply felt to this day in many parts of mathematics. I asked Osserman to explain. He did:

"Clearly, it's not literally true that Riemann never proved anything, but what is true is that his fame derives less from things that he proved than from his other contributions. I would classify those in a number of categories. The first consists of conjectures, the most famous being the Riemann Hypothesis about the zeros of the zeta function. The second is a number of definitions, such as the Riemann integral, the definition of an analytic function in terms of the Cauchv-Riemann equations (rather than via an 'analytic' expression), the Riemann curvature tensor and sectional curvature in Riemannian geometry. The third, related to the second, but somewhat different, consists of new concepts, such as a Riemann surface and a Riemannian manifold, which involve radical rethinking of fundamental notions, as well as the extended complex plane, or Riemann sphere, and the fundamental notions of algebraic curve. The fourth is the construction of important new examples, such as Riemann's complete periodic embedded minimal surface and what we now call hyperbolic space, via the metric now referred to as the 'Poincaré metric', which Riemann wrote down explicitly. And finally, there is the Riemann mapping theorem, whose generality is simply breathtaking, and again required a depth of insight, but whose proof, as given by Riemann, was defective, and was not fully established until many years later. Perhaps one could combine them by saying that Riemann was a deep mathematical thinker whose vision had a profound impact on the future of mathematics."

Many readers have not heard of all these contributions of Riemann. But this example is enough to refute the catch-phrases. "A mathematician is someone who proves theorems" and "Mathematics is nothing without proof."

As a matter of principle there can't be a strict demonstration that Meaning 2, formal proof, is what mathematicians do in practice. This is a universal limitation of mathematical models. In principle it's impossible to give a mathematical proof that a mathematical model is faithful to reality. All you can do is test the model against experience.

The second official assertion about proof is:

Assertion 2. Any correct practical proof can be filled in to be a correct theoretical proof.

"If you can do it, then do it!"

"It would take too long. And then it would be so deadly boring, no one would read it." 2

Assertion B is commonly accepted. Yet I've seen no practical or theoretical argument for it, other than absence of counter-examples. It may be true. It's a matter of faith.

Take a mathematically accepted proof and undertake to fill in the gaps, turn it into a formal proof. If you meet no obstacles, very well! And if you do meet an obstacle? That is, a mathematically accepted step that you can't break down into successive *modus ponens*? You've discovered an implicit assumption, a hidden lemma! Join it to the hypotheses of the theorem and go merrily forward.

If this is what's meant by formalization of proof, it can always be done. There's a preassigned way around any obstacle! There remains one little worry. Is your enlarged set of assumptions consistent? This can be hard to answer. You may say, if there's doubt about their consistency, it was wrong to claim the original proof was correct. On the other hand, we don't even know if the hypotheses of our number system are consistent. We presume it's O.K. on pragmatic grounds.

No one has proved that definition 2, the logic model of proof, is wrong. If a counter-example were found, logic would adapt to accommodate it.

By "proof" we mean correct proof, complete proof. By the standards of formal logic, ordinary mathematical proofs are incomplete. When an ordinary mathematical proof is offered to a referee, she may say, "More detail is needed." So more detail is supplied, and then the proof is accepted. But this acceptable version is still incomplete as a formal proof! The original proof was mathematically incomplete, the final proof is mathematically complete, but *both are formally incomplete.* The formal concept of proof is irrelevant at just the point of concern to the mathematician!

We prefer a beautiful proof with a serious gap over a boring hyper-correct one. If the *idea* is beautiful, we think it will attain valid mathematical expression. We even change the meaning of a concept to get a more beautiful theory. Projective geometry is a classic example. Euclidean geometry uses the familiar axiom, "Two points determine a line, and two lines determine a point, *unless the lines are parallel.*" Projective geometry brings in 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." This is "right." The Euclidean axiom by comparison is awkward and clumsy.

When a mathematician submits work to the critical eyes of her colleagues, it's being tested, or "proved" in the old sense. With few exceptions, mathematicians have no other way to test or "prove" their work—invite whoever's interested to have a shot at it. Then the mathematical "Meaning number 1" of "proof" agrees with the old meaning. The *proof* of the pudding is in the eating. The *proof* of the theorem (in that sense of being tested) is in the refereeing.

Philosophers call this social validation "warranting."

Computers and Proof

In the late 1970s or early 1980s, while visiting a respected engineering school, I was told that the dean was vexed with his mathematicians. Other professors used his computer center; mathematicians didn't. Today no one's complaining that mathematicians don't compute. We were just 10 or 15 years late.

The issue about proof today is the impact of computers on mathematical proof. The effects are complex and manifold. I describe 2 trends, 3 examples, 2 critics, and 5 reasons.

First, two trends: (Trend A) Computers are being used as aids in proving theorems; and (Trend B) computers are encroaching on the central role we give proof in mathematics.

Three examples of trend A:

Example 1. In 1933, before general-purpose computers were known, Derrick Henry Lehmer built a computer to study prime numbers. It collected number-theoretic data and examples, from which he formulated conjectures. This was a mechanization of Lagrange and Gauss, who conjectured the prime number theorem from tables of prime numbers.

Example 2. Wolfgang Haken and Kenneth Appel's proof that a plane map needs at most four colors. The conjecture had been in place since 1852. Appel and Haken turned it into a huge computation. Then their computer did the computation. Thus they proved the conjecture.

Example 3. In the Feigenbaum-Lanford discovery of a universal critical point for doubling of bifurcations, computers played two distinct essential roles. Mitchell Feigenbaum discovered the universal critical point by computer exploration, comparable to Lehmer's number-theoretic explorations. Later, rigorous proofs were given by Oscar Lanford III (9, 10, 11) and by M. Campanino, H. Epstein, and D. Ruelle. At that stage the computer played a role like its role in the four-color problem: assistant in completing the proof.

Now the proof wasn't algebra but analysis. A certain derivative had to be estimated. This was accomplished by the computer, using approximation methods from numerical analysis, with a rigorous error estimate.

Example 1, Lehmer's exploratory use, didn't challenge the standard notion of proof. That notion doesn't care how a conjecture is made. I heard a distinguished professor tell his class, "It doesn't matter if you find the answer lying in a mud puddle. If you prove it's the answer, it makes no difference how you found it."

Examples 2 and 3 are different. Now the machine computation is *part of the proof.* For some people, such a proof violates time-honored doctrines about the difference between empirical knowledge and mathematical knowledge. According to Plato, Kant, and many others, common knowledge and scientific knowledge are a posteriori. They come from observation of the material world. Mathematical knowledge is a priori—independent of contingent facts about the material world. Daily experience and physical experiment could conceivably be other than what they are, but mathematical truths will hold in every possible world, or so thought Plato and Kant.

The operation of computers depends on properties of copper and silicon, on electrodynamics and quantum mechanics. Confidence in computers comes from confidence in physical facts and theories. These are not a priori. We learn the laws of physics and the electrical properties of silicon and copper from experience. It seems, then, that while old-fashioned theorems proved "by hand" are a priori, computer-assisted theorems are a posteriori! (Philip Kitcher thoroughly goes into the issue of a priori knowledge in mathematics.)

But despite the illusions of idealist philosophy, old-fashioned person-made proofs are also a posteriori. They depend on credence in the world of experience, the material world. We believe our scribbled notes don't change from hour to hour, that our thinking apparatus—our brain—is reliable more often than not, that our books and journals are what they pretend to be. We sometimes believe a proof without checking every line, every reference, and every line of every reference. Why? We depend on the integrity and competence of certain human beings—our colleagues. But human beings are *less* reliable at long computations than a Cray or a Sun! And still less in collaborations by large groups, as in the classification theorem of simple finite groups. Recognizing this demolishes the dream of a priori knowledge in advanced mathematics, and in general.

Most mathematicians see the difference between computer-assisted proofs and traditional proofs as a difference of degree, not of kind. But Paul Halmos does object to the Haken-Appel proof. "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 does Halmos call the computer an Oracle? He can't know every step in the calculation. Indeed, the physical processes that make computers work aren't fully understood. Believing a computer is like believing a successful, well-reputed fortune teller. Should we regard the four-color conjecture as true? Should our confidence in it be increased by the Appel-Haken proof? Halmos doesn't say.

Halmos dislikes the Appel-Haken proof because it uses an Oracle, and because, he thinks, we can't learn anything from it. His criticism isn't for logical defects—incompleteness, incorrectness, or inaccuracy. It's esthetic and epistemological. This is normal in real-life mathematics. It would be senseless in formalized or logicized mathematics.

Views like Halmos were also vented by Daniel Cohen. "Our pursuit is not the accumulation of facts about the world or even facts about mathematical objects. The mission of mathematics is understanding. The Appel and Haken work on the Four Color Problem amounts to a confirmation that a map-maker with only four paint pots will not be driven out of business. This is not really what mathematicians were worried about in the first place. Admitting the computer shenanigans of Appel and Haken to the ranks of mathematics would only leave us intellectually unfulfilled."

The eloquence of Halmos and Cohen won't deter mathematicians who hope a computer will help on their problem. Computers in pure mathematics will increase for at least five reasons.

Reason 1. Access to more powerful computers spreads ever more widely.

Reason 2. As oldsters are replaced by youngsters schooled after the computer revolution, the proportion of us at home with computers rises.

Reason 3. Despite the scolding of Halmos, Cohen, and sympathizers, success like that of Haken-Appel and Lanford inspires emulation.

Reason 4. Scientists and engineers were computerized long ago. You can't imagine a mathematician interacting with a scientist or engineer today without a computer.

Reason 5. Reasons 1, 2, 3, and 4 stimulate those branches of mathematics that use computation, leaving the others at a disadvantage. This reinforces Reasons 1, 2, 3, and 4.

Some will still reject computer proofs. They'll have as much effect as old King Canute. (By royal order, he commanded the tide to turn back.)

In fields like chaos, dynamical systems, and high Reynolds number fluid dynamics, we come to trend B. Here, as in number theory, the computer is an explorer, a scout. It goes much deeper into unknown regions than rigorous analysis can. In these fields we no longer think of computer-obtained knowledge as tentative—pending proper proof. We prove what we can, compute what we can. This part of mathematical reality rests on computation and analysis closely yoked together. The computer finds "such and such." We believe it—not as indubitable, but as believable. Proof of "such and such" then would help us see *why* it's so, though not perhaps increase our conviction it *is* so.

This effect of computers, Trend B, is profound. It erodes the time-honored understanding of what mathematics is. Machine computation as part of proof is radical, but still more radical is machine computation accepted as *empirical evidence of mathematical truth, virtually a weak form of proof.* Such acceptance contradicts the official line that mathematics *is* deductions from axioms. It makes mathematics more like an empirical science.

An interesting proposal to control this disturbing new tendency was made in the *Bulletin of the American Mathematical Society* by the distinguished mathematical physicist Arthur Jaffe and the distinguished topologist Frank Quinn. They want to save rigorous proof, the chief distinction of modern mathematics, from promiscuous contamination with mere machine calculations. Yet they acknowledge that "empirical" or "experimental" computer mathematics will not go away, and may have a place in the world. Their solution is—compulsory rigid distinction between the two. Genuine mathematics stays as it should be. Experimental or numerical or empirical mathematics is labeled appropriately. Analogous to kosher chickens versus non-kosher. I don't have the impression that this proposal met with general acclaim.

A different departure from traditional proof was found by Miller (1976), Rabin (1976), Davis (1977), and Schwartz (1980). There's now 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." If n is really prime, you can make p arbitrarily close to 1. Yet the primality of a given n is *not* a random variable. n either is prime or it isn't. But if n is large, finding its primality by a deterministic method is so laborious that random errors must be expected in the computation. Rabin showed that if n is very large, the probability of error in the deterministic calculation is greater than the probability p in his fast probabilistic method!

How will we adjust to such variation in proof? We can think of proofs as having variable quality. Instead of "proved," label them either "proved by hand" or "proved by machine." Even provide an estimate for the reliability of the machine calculation used in a proof (Swart, 1980).

There are two precedents for this situation. Between the world wars, some mathematicians made it a practice to state explicitly where they used the axiom of choice. And in 1972 Errett Bishop said the clash between constructivists and classicists would end if classicists stated explicitly where they used the law of the excluded middle (L. E. M.). (Constructivists reject the L. E. M. with respect to infinite sets.) These issues didn't involve computing machines. They involved disagreement about proof.

If experience with the axiom of choice and the L. E. M. is indicative, Swart's proposal is unpromising. Nobody worries any more about the axiom of choice, and few worry about the L. E. M. Perhaps we don't care much about distinctions of quality or certainty in proof. Perhaps few care today if proofs are handmade or computer-made.

There's a separate trend in computer proof, that so far has had more resonance in computer science than in main-line mathematics. In this trend, the idea is not to use the computer to supply a missing piece of a mathematician's proof. Rather, the computer is a logic assistant. Give it some axioms, and send it out to find interesting theorems. Of course, you have to give it a way to measure "interesting." Or tell it what you want proved—a conjecture you haven't proved yourself, or a proved theorem, to see what different proof it might find.

Since the computer is expected to follow the rules of logic, the relation to definition 2 is apparent. Since it really does find proofs that convince mathematicians, it conforms to definition 1. People who think mathematicians will become obsolete have this kind of thing in mind—computer proofs of real theorems.

Professor Wos writes movingly, "We have beaten the odds, done the impossible, automated reasoning so effectively that our programs have even answered open questions. Yet skeptics still exist, funding is not abundant, and recognition of our achievements is inappropriately small and not sufficiently widespread."

This work can be regarded as practical justification of the formal logic notion of proof. To the extent that computers following only the rules of formal logic do reproduce discoveries of live mathematicians, they show that formal logic is an adequate model of real live proof. But "technical limitations" restrict automated proof to relatively simple theorems.

There are centers of this research in Austin, Texas, and at Argonne National Laboratory. There is a *Journal of Automated Reasoning*. Bledsoe and Loveland is an instructive and readable review.

I've said nothing about applied mathematics. In applied mathematics, infiltration and domination by computers has long been a fait accompli. Under the influence of computers, pure mathematics is becoming more like applied mathematics.

Fallibility

Philosophical discussions of mathematical proof usually talk about it only as it's seen 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 indeed it is a proof.

How does formal proof differ from real live proof?

Real-life proof is informal, in whole or in part. A piece of formal argument—a calculation—is meaningful only to complete or verify some informal reasoning. The formal-logic picture of proof is a topic for study in logic rather than a truthful picture of real-life mathematics.

Formal proof exists only in a formalized theory, cleaned and purged of all associations and connotations. It uses a formal vocabulary, formal axioms, and formal inference rules.

The passage from informal to formalized theory must entail loss of meaning or change of meaning. The informal has connotations and alternative interpretations not in the formalized theory. Consequently, anything proved formally can be challenged: "How faithful are this statement and proof to the informal concept we're actually interested in?"

For some investigations, formalization and complete formal proof would take time and persistence beyond human capability, beyond any foreseeable computer.

Mathematicians say that any "Theorem and Proof" in a pure mathematics journal must be formalizable *in principle*. A glance into any mathematical *Archive* or *Bulletin* or *Journale* or *Zeitschrift* shows a great proportion of the text in natural language. Even pages of solid calculation turn out, on inspection, not to be formalized. In presenting calculations for publication, we include steps that we consider nonroutine, which should be explained to fellow specialists. And in every calculation there are routine steps that needn't be explained to fellow experts. These, naturally, we leave out. (If we include them, the editor throws them out.)

So the published proof is incomplete. The reader accepts the result on faith, or fills in the steps herself.

Even in a graduate math class proof isn't completely formalized. The professor leaves out as a matter of course what she considers routine or trivial. A non-routine step may be assigned as homework.

Practically none of the mathematical literature is formalized (except for computer programs, which are another story.) Yet these far-from-formalized proofs are accepted by mathematicians as "formalizable in principle."

Why are they accepted? Because they're convincing to the experts, who'd be only too happy to find a serious error or gap.

We accept incomplete, natural-language proofs on the basis of our experience, our know-how in looking for the weakest link. And also on the author's reputation. A known bungler, or just an unknown, or an authority of proved accomplishment?

Peano hoped that his formal language would guarantee proofs to be correct. In today's language, that proofs could be checked by computer. But trying to check proofs by computer may introduce new errors. There is random error by physical fluctuations of the machine, and there is human error in designing and producing hardware and software (logic and programming.) For most nontrivial mathematics, the vision of formal proof is still visionary. And when a machine does part of a proof, as in the four-color theorem of Appel and Haken, some mathematicians reject it because the details of machine computation are inevitably hidden. More than *whether* a conjecture is correct, we ask *why* it is correct. We want to understand the proof, not just be told it exists.

The issue of machine error is more than just electrical engineering. It's the difference between computation in principle (infallible) and computation in practice (fallible). A simple calculation shows that no matter how small the chance of error in one step, if the calculation or formal proof is long enough, it's almost sure to contain errors. But we can have other grounds for believing the conclusion of a proof, apart from the claimed certainty of step-by-step reasoning. Examples and special cases, analogy with other results, expected symmetry, unexpected elegance, even an inexplicable feeling of rightness. These illogical logics may say, "It's true!" If you have something you hope is a proof, such non-rigorous reasons can make you sure of the conclusion, even while you know the "proof" is sure to contain uncorrected errors. Such intuition is fallible in principle. Attempted rigorous proof is fallible in practice.

Beyond the certainty of error in long calculations, you face the fact that calculation is finite, mathematics infinite. There's a limit to the biggest computer, how tightly it can be packed, how fast it can run, how long it will operate. The life of the human race is a limit. Put these limits together, and you have a bound on how much anyone will ever compute. If you can't *know* anything in mathematics except by formal proof (a particular kind of computation), you've set a bound on how much mathematics you'll ever know. The physical bound on computation implies a bound on the number and length of theorems that will ever be proved. The only way out would be a faster, less certain way to mathematical knowledge (Knuth, 1976).

Meyer (1974, p. 481) quotes a theorem, proved 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 $6 \times 616 = 3{,}696$ binary digits, then any logical network with 3,696 inputs which decides truth of these sentences contains 10^{123} operations." (WSIS is much weaker than ordinary arithmetic. The conclusion applies a fortiori to sentences longer than 616 digits.)

"We remind the reader," Meyer writes, "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": One says that a decision procedure "exists" for sentences of any length.

Our notion of rigorous proof isn't carved in granite. We'll modify it. We'll allow machine computation, numerical evidence, probabilistic algorithms, if we find them advantageous. We mislead our pupils if we make "rigorous proof" a shibboleth in class.

In Class

The role of proof in class isn't the same as in research. In research, it's to *convince*. In class, students are all too easily convinced! Two special cases do it. In a first course in abstract algebra, proof of the fundamental theorem of algebra is often omitted. Students believe it anyway.

The student needs proof to *explain*, to give insight why a theorem's true. Not proof in the sense of formal logic. As the graduate student said to the Ideal Mathematician in *The Mathematical Experience*, she never saw such a proof in class. In class informal or semiformal proofs are presented in natural language. They include calculations, which are formal subproofs inside the overall informal proof.

Some instructors think, "If it's a math class, you prove. If you don't prove anything, it isn't math." That makes a kind of sense. If proof is math and math is proof, then in math class you're duty bound to prove. The more you prove, the more honest and rigorous you feel your class is.

Exposure to proof can be more emotional than intellectual. If the instructor gives no better reason for proof than "That's math!," the student knows she saw a proof, but not wherefore, except: "That's math!"

I call this view "absolutist," despite that word's unfortunate associations (absolute monarchy, absolute zero, etc.). If mathematics is a system of absolute truths, independent of human construction or knowledge—then mathematical proofs are external and eternal. They're to admire. The absolutist teacher wants to tell only what he intends to prove (or order the students to prove). He'll usually try for the shortest proof or the most general one. The main purpose of proof isn't explanation. The purpose is certification: admission into the catalog of absolute truths.

The view I favor is humanism. To the humanist, mathematics is *ours*—our tool, our plaything.

Proof is complete explanation. Give it when complete explanation is appropriate, rather than incomplete explanation or no explanation.

The humanist math teacher looks for enlightening proofs, not necessarily the most general or the shortest. Some proofs don't explain much. They're called "tricky," "pulling a rabbit out of a hat." Give that kind of proof when you want your students to see a rabbit pulled out of a hat. But in general, give proofs that explain. And if the only proof you can find is unmotivated and tricky, if your students won't learn much from it, must you do it "to stay honest"? That "honesty" is a figment, a self-imposed burden. Better try to be clear, well-motivated, even inspiring.

This attitude disturbs people who think proof is the be-all and end-all of mathematics—who say "a mathematician is someone who proves theorems" and "without proof, there's no mathematics." From that viewpoint, a mathematics in which proof is less than absolute is heresy.

For the humanist, the purpose of proof, as of all teaching, is understanding. Whether to give a proof as is, elaborate it, or abbreviate it, depends on what he thinks will increase the student's understanding of concepts, methods, and applications.

This policy uses the notion of "understanding," which isn't precise or likely to be made precise. Do we understand what it means "to understand"? No. Can we teach to foster understanding? Yes. We recognize understanding, though we can't say precisely what it is.

In a stimulating article, Uri Leron (1983) borrowed an idea from computing—"structured proof." A structured proof is like a structured program. Instead of starting with little lemmas whose significance appears at the end, start by breaking the proof-task into chunks. Then break each chunk into subchunks. The little lemmas come at the end, where you see why you need them.

In the general classroom, the motto is: "Proof is a tool in service of teacher and class, not a shackle to restrain them."

In teaching future mathematicians, "Proof is a tool in service of research, not a shackle on the mathematician's imagination."

Proof can convince, and it can explain. In research, convincing is primary. In high-school or undergraduate class, explaining is primary.

Intuition

If we look at mathematical practice, the intuitive is everywhere. We consider intuition in the mathematical literature and in mathematical discovery.

A famous example was the letter from Ramanujan to Hardy, containing astonishing formulas for infinite sums, products, fractions, and roots. The letter had gone to Baker and to Hobson. They ignored it. Hardy didn't ignore it.

Ramanujan's formulas prove there is mathematical intuition, for they're correct, even though Ramanujan didn't prove them, and in some cases had hardly an idea what a proof would be. But what about Hardy? He also made a correct judgment without proof—the judgment that Ramanujan's formulas were true, and that Ramanujan was a genius. How did he do that? Not by checking his formulas with complete proofs. By some mental faculty associated with mathematics, Hardy made a sound judgment of Ramanujan's formulas, without proofs. Was Hardy's judgment of Ramanujan's letter a mathematical judgment? Of course it was, in any reasonable understanding of the word mathematics. It was an exceptional event, yet not essentially different from mathematical judgments made every day by reviewers and referees, by teachers and paper-graders, by search committees and admission committees. The faculty called on in these judgments is mathematical intuition. It's reliable mathematical belief without the slightest dream of being formalized.

Since intuition is an essential part of mathematics, no adequate philosophy of mathematics can ignore intuition.

The word intuition, as mathematicians use it, carries a heavy load of mystery and ambiguity. Sometimes it's a dangerous, illegitimate substitute for rigorous proof. Sometimes it's a flash of insight that tells the happy few what others learn with great effort. As a first step to explore this slippery concept, consider this list of the meanings and uses we give this word.

1. Intuitive is the opposite of rigorous. This usage is not completely clear, for the meaning of "rigorous" is never given precisely. We might say that in this usage intuitive means lacking in rigor, yet the concept of rigor is defined intuitively, not rigorously.

2. Intuitive means visual. Intuitive topology or geometry differs from rigorous topology or geometry in two ways. On one hand, the intuitive version has a meaning, a referent in the domain of visualized curves and surfaces, which is absent from the rigorous formal or abstract version. In this the intuitive is superior; it has a valuable quality the rigorous version lacks. On the other hand, visualization may mislead us to think obvious or self-evident statements that are

dubious or false. The article by Hahn, "The Crises in Intuition" is a beautiful collection of such statements.

3. Intuitive means plausible, or convincing in the absence of proof. A related meaning is, "what you might expect to be true in this kind of situation, on the basis of experience with similar situations." "Intuitively plausible" means reasonable as a conjecture, i.e., as a candidate for proof.

4. Intuitive means incomplete. If you take a limit under the integral sign without using Lebesgue's theorem, if you expand a function in a power series without checking that it's analytic, you acknowledge the logical gap by calling the argument intuitive.

5. Intuitive means based on a physical model or on some special examples. This is close to "heuristic."

6. Intuitive means holistic or integrative as opposed to detailed or analytic. When we think of a theory in the large, when we're sure of something because it fits everything else we know, we're thinking intuitively. Rigor requires a chain of reasoning where the first step is known and the last step is the conjecture. If the chain is very long, rigorous proof may leave doubt and misgiving. It may actually be less convincing than an intuitive argument that you grasp as a whole, which uses your faith that mathematics is coherent.

In all these usages intuition is vague. It changes from one usage to another. One author takes pride in avoiding the "merely" intuitive—the use of figures and diagrams as aids to proof. Another takes pride in emphasizing the intuitive—showing visual and physical significance of a theory, or giving heuristic derivations, not just formal post hoc verification.

With any of these interpretations, the intuitive is to some degree extraneous. It has desirable and undesirable aspects. It's optional, like seasoning on a salad. It's possible to teach mathematics or to write papers without thinking about intuition.

However, if you're not doing mathematics, but watching people do mathematics and trying to understand what they're doing, dealing with intuition becomes unavoidable.

I maintain that:

1. All the standard philosophical viewpoints rely on some notion of intuition.
2. None of them explain the nature of the intuition that they postulate.
3. Consideration of intuition as actually experienced leads to a notion that is difficult and complex, but not inexplicable.
4. A realistic analysis of mathematical intuition should be a central goal of the philosophy of mathematics.

Let's elaborate these points. By the main philosophies, I mean as usual constructivism, Platonism, and formalism. For the present we don't need refined distinctions among versions of the three. It's sufficient to characterize each crudely with one sentence.

The constructivist regards the natural numbers as the fundamental datum of mathematics, which neither requires nor is capable of reduction to a more basic notion, and from which all meaningful mathematics must be constructed.

The Platonist regards mathematical objects as already existing, once and for all, in some ideal and timeless (or tenseless) sense. We don't create, we discover what's already there, including infinites of a complexity yet to be conceived by mind of mathematician.

The formalist rejects both the restrictions of the constructivist and the theology of the Platonist. All that matters are inference rules by which he transforms one formula to another. Any meaning such formulas have is nonmathematical and beside the point.

What does each of these three philosophies need from the intuition? The most obvious difficulty is that besetting the Platonist. If mathematical objects constitute an ideal nonmaterial world, how does the human mind/brain establish contact with this world? Consider the continuum hypothesis. Gödel and Cohen proved that it can neither be proved nor disproved from the set axioms of contemporary mathematics. The Platonist believes this is a sign of ignorance. The continuum is a definite thing, independent of the human mind. It either does or doesn't contain an infinite subset equivalent neither to the set of integers, nor to the set of real numbers. Our *intuition* must be developed to tell us which is the case. The Platonist needs intuition to connect human awareness and mathematical reality. But his intuition is elusive. He doesn't describe it, let alone analyze it. How is it acquired? It varies from person to person, from one mathematical genius to another mathematical genius. It has to be developed and refined. By whom, by what criteria, does one develop it? Does it directly perceive an ideal reality, as our eyes perceive visible reality? Then intuition would be a second ideal entity, the subjective counterpart of Platonic mathematical reality. We have traded one mystery for two: first, the mysterious relation between timeless, immaterial ideas and the mundane reality of change and flux; and second, the mysterious relation between the flesh and blood mathematician and his intuition, which directly perceives the timeless and eternal. These difficulties make Platonism hard for a scientifically oriented person to defend.

Mathematical Platonists simply disregard them. For them, intuition is something unanalyzable but indispensable. Like the soul in modern Protestantism, the intuition is there but no questions can be asked about it.

The constructivist, as a conscious descendant of Kant, knows he relies on intuition. The natural numbers are given intuitively. This doesn't seem problematical. Yet Brouwer's followers have disagreed on how to be a constructivist. Of course, every philosophical school has that experience. But it creates a difficulty for a school that claims to base itself on a universal intuition.

The dogma that the intuition of the natural numbers is universal violates historical, pedagogical, and anthropological experience. The natural number system seems an innate intuition to mathematicians so sophisticated they can't remember or imagine before they acquired it; and so isolated they never meet people who haven't internalized arithmetic and made it intuitive (the majority of the human race!).

What about the formalist? Does intuition vanish along with meaning and truth? You can avoid intuition as long as you consider mathematics to be no more than formal deductions from formal axioms. A. Lichnerowicz wrote, "Our demands on ourselves have become infinitely larger; the demonstrations of our predecessors no longer satisfy us but the mathematical facts that they discovered remain and we prove them by methods that are infinitely more rigorous and precise, methods from which geometric intuition with its character of badly analyzed evidence has been totally banned."

Geometry was pronounced dead as an autonomous subject; it was no more than the study of certain particular algebraic-topological structures. The formalist eliminates intuition by concentrating on refinement of proof and dreaming of an irrefutable final presentation. To the natural question, Why should we be interested in these superprecise, superreliable theorems?, formalism turns a deaf ear. Obviously, their interest derives from their meaning. But the all-out formalist throws out meaning as nonmathematical. Then how did our predecessors find correct theorems by incorrect reasoning? He has no answer but "Intuition."

Surely Cauchy knew Cauchy's integral theorem, even though (in the formalist's sense of knowing the formal set-theoretic definition) he didn't know the meaning of any term in the theorem. He didn't know what is a complex number, what is an integral, what is a curve; yet he found the complex number represented by the integral over this curve! How can this be? Cauchy had great intuition.

"But during my last night, the 22–23 of March, 1882—which I spent sitting on the sofa because of asthma—at about 3:30 there suddenly arose before me the Central Theorem, as it has been prefigured by me through the figure of the 14-gon in (Ges. Abh., vol. 3, p. 126). The next afternoon, in the mail-coach (which then ran from Norden to Emden) I thought through what I had found, in all its details. Then I knew I had a great theorem. . . . The proof was in fact very difficult. I never doubted that the method of proof was correct, but everywhere I ran into gaps in my knowledge of function theory or in function theory itself. I could only postulate the resolution of these difficulties, which were in fact completely resolved only 30 years later (in 1921) by Koebe" (F. Klein).

But what *is* this intuition? The Platonist believes in real objects (ideal, to be sure), which we "intuit." The formalist believes no such things exist. So what is there to intuit? The only answer is, unconscious formalizing. Cauchy subconsciously knew a correct proof of his theorem, which means knowing the correct definitions of all the terms in the theorem.

This answer is interesting to the many mathematicians who've made correct conjectures they couldn't prove. If their intuitive conjecture was the result of unconscious reasoning, then: (a) Either the unconscious has a secret method of reasoning that is better than any known method; or (b) the proof is there in my head, I just can't get it out!

Formalists willing to consider the problem of discovery and the historical development of mathematics need intuition to account for the gap between their account of mathematics (a game played by the rules) and the real experience of mathematics, where more is sometimes accomplished by breaking rules than obeying them.

Accounting for intuitive "knowledge" in mathematics is the basic problem of mathematical epistemology. What do we believe, and why do we believe it? To answer this question we ask another question: what do we teach, and how do we teach it? Or what do we try to teach, and how do we find it necessary to teach it? We try to teach mathematical concepts, not formally (memorizing definitions) but intuitively—by examples, problems, developing an ability to think, which is the expression of having successfully internalized something. What? An intuitive mathematical idea. The fundamental intuition of the natural numbers is a shared concept, an idea held in common after manipulating coins, bricks, buttons, pebbles. We can tell by the student's answers to our questions that he gets the idea of a huge bin of buttons that never runs out.

Intuition isn't direct perception of something external. It's the effect in the mind/brain of manipulating concrete objects—at a later stage, of making marks on paper, and still later, manipulating mental images. This experience leaves a trace, an effect, in the mind/brain. That trace of manipulative experience is your representation of the natural numbers. Your representation is equivalent to mine in the sense that we both give the same answer to any question you ask. Or if we get different answers, we compare notes and figure out who's right. We can do this, not because we have been explicitly taught a set of algebraic rules, but because our mental pictures match. If they don't, since I'm the teacher and my mental picture matches the one all the other teachers have, you get a bad mark.

We have intuition because we have mental representations of mathematical objects. We acquire these representations, not mainly by memorizing formulas, but by repeated experiences (on the elementary level, experience of manipulating physical objects; on the advanced level, experiences of doing problems and discovering things for ourselves). These mental representations are checked for veracity by our teachers and fellow students. If we don't get the right answer, we flunk the course. Different people's representations are always being rubbed against each other to make sure they're congruent. We don't know how these representations are held in the mind/brain. We don't know how *any* thought or knowledge is held in the mind/brain. The point is that as shared concepts, as mutually congruent mental representations, they're real objects whose existence is just as "objective" as mother love and race prejudice, as the price of tea or the fear of God.

How do we distinguish mathematics from other humanistic studies? There's a fundamental difference between mathematics and literary criticism. While mathematics is a humanistic study with respect to its subject matter—human ideas—it's science-like in its objectivity. Those results about the physical world that are reproducible—which come out the same way every time you ask—are called scientific. Those subjects that have reproducible results are called natural sciences. In the realm of ideas, of mental objects, those ideas whose properties are reproducible are called mathematical objects, and *the study of mental objects with reproducible properties is called mathematics.* Intuition is the faculty by which we consider or examine these internal, mental objects.

There's always some discrepancy between my intuition and yours. Mutual adjustment to keep agreement is going on all the time. As new questions are asked, new parts of the structure come into sight. Sometimes a question has no answer. The continuum hypothesis doesn't have to be true or false.

We know that with physical objects we may ask questions that are inappropriate, which have no answer. What are the exact velocity and position of an electron? How many trees are there growing at this moment in Minnesota? For mental objects as for physical ones, what seems at first an appropriate question is sometimes discovered, perhaps with great difficulty, to be inappropriate. This doesn't contradict the existence of the particular mental or physical object. There are questions that *are* appropriate, to which reliable answers can be given.

The difficulty in seeing what intuition is arises because of the expectation that mathematics is infallible. Both formalism and Platonism want a superhuman mathematics. To get it, each of them falsifies the nature of mathematics in human life and in history, creating needless confusion and mystery.

Certainty

Even if it's granted that the need for certainty is inherited from the ancient past, and is religiously motivated, its validity is independent of its history and its motivation. The question remains: is mathematical knowledge indubitable?

Set aside history and motivation. Look at samples of mathematical knowledge and ask: Is this indubitable?

We take three examples. First, good old

$2 + 2 = 4$.**

Second, familiar to all former high-school students,

"The angle sum of any triangle equals two right angles." Finally, a more sophisticated example: a convergent infinite series.

Label the first example

Formula A: $2 + 2 = 4$.

Everyone knows Formula A is a mathematical truth. Everyone knows it's indubitable. *Ergo*, at least one mathematical truth is indubitable.

There's Russell's cavil: indubitable, but no great truth.

$2 + 2$ by definition means $(1 + 1) + (1 + 1)$
4 by definition means $1 + (1 + (1 + 1))$

By the associative law of addition, Formula A then is:

$$1 + 1 + 1 + 1 = 1 + 1 + 1 + 1.$$

Indubitable but unimpressive! As Frege said, it is an analytic a priori truth, not a synthetic one. (See Chapters 7 and 8 about Kant and Frege.)

Bertrand Russell thought *every* mathematical truth is a tautology like $2 + 2 = 4$—trivially indubitable. He said that a mathematical theorem says no more than "the great truth that there are three feet in a yard." This view is regarded by mathematicians as absurd and without merit.

To probe into Formula A, we must ask, what is 2? What is 4? What is +? What is =? Trivial as these questions may seem, they serve to distinguish the different schools (logicist, formalist, intuitionist, empiricist, conventionalist).

The most elementary answer is the empiricist one. "$2 + 2 = 4$" means "Put two buttons in a jar, put in two more, and you have four buttons in the jar." John Stuart Mill is the classic advocate of this interpretation. For him, formula A is *not* indubitable. It's about buttons. Buttons are material objects, which never can be known with certainty (Heraclitus et al., as expounded, for instance, in Russell's *History of Western Philosophy*, 1945). Who knows if some exotic chemical reaction might give

two buttons + two buttons = zero buttons

or

two buttons + two buttons = five buttons.

For indubitability, forget buttons.

Another answer, along formalist or logicist lines, might be given by a graduate student of mathematics. "1, +, = are symbols defined by the Dedekind-Peano axioms.**

"2 is short for $1 + 1$,
"3 is short for $2 + 1$,
"4 is short for $3 + 1$.
"Now Formula A can be proved."

(Our reduction above of Formula A to

$$1 + 1 + 1 + 1 = 1 + 1 + 1 + 1$$

is a sketch of the formal proof in the Mathematical Notes and Comments.)

So then is Formula A doubtable?

Before I worry about doubtability, how sure am I that the proof is even correct? It *seems* to me that it is correct. Could I be mistaken? Overlooked something staring me in the face? I make mistakes in math. I think I'm sure the proof is O.K., but am I *really* sure that I'm *totally* certain?

A second worry is more substantial. How do I know Peano's axioms produce the same number system 1,2,3, . . . that I had in mind (or Dedekind had in mind) in the first place? They seem to work. How certain can I be that they'll always work? The numbers 1,2. . . . with which we start (before anybody gives us axioms) are an informal, "intuitively given " system. For that reason, it's impossible to prove formally that they correspond to any formal model. Such a proof is possible only between two formal models. Does the formal model correspond to the original intuitive idea? That question can never by answered by a rigorous, formal proof! It must rest on informal, intuitive reasoning that has no claim to be rigorous, let alone indubitable.

I have no doubt that Peano's axioms actually do describe the intuitive natural numbers 1,2,. . . . But I can't claim this belief as indubitable. Consider my knowledge that I'm now writing on a yellow pad with a blue pen sitting at a round table, and so forth. I can barely conceive that this might be false. But by the standards of Heraclitus, Plato, and others, such knowledge is doubtable or dubitable. If so, I can more readily believe Peano's seemingly convincing axioms might be doubtable. If they are, then every theorem in Peano arithmetic is doubtable. Even Formula A,

$$2 + 2 = 4 \text{ !!}$$

Another often-cited distinction between sensory knowledge and mathematical knowledge is that sensory knowledge *could conceivably be* other than it is. It's *conceivable* that my blue pen is really yellow and my yellow pad really blue.

Mathematical knowledge, on the other hand, such as Formula A, is supposed to be not only indubitably true, it's supposed to be inconceivable that it could be false. We supposedly can't imagine

$$2 + 2 = 3$$

as we supposedly can imagine a blue pad that looks yellow. This observation has been held to demonstrate that Formula A is indubitable.

But whether the denial of Formula A is inconceivable can't be judged until the import and significance of Formula A is explicated. It has one meaning as a *report of a property of buttons*, coins, or other such discrete objects. It has a second meaning as *a theorem in an axiom system* with +, =, and 1 as undefined terms. But we've just seen that as a formula about buttons, Formula A is doubtable. And as a theorem in an abstract axiom system, it's doubtable still. Whether in terms of buttons or of Peano arithmetic, Formula A is doubtable. Its negation is conceivable.

Let's consider the angle-sum theorem in Euclidean plane geometry.** It was Baruch Spinoza's favorite example of an absolutely certain statement. We'll follow proper terminology and call it

Formula B: Angle A + Angle B + Angle C = 2 right angles.

The angles A, B, and C on the left side of the equation are the internal angles of a triangle, any triangle at all.

Trite as this example seems to us today, it was already the twentieth century when Gottlob Frege berated David Hilbert for not understanding that there can be only one real, true geometry (Euclid's, of course).

We have theorem and proof. They have withstood scrutiny for 2,000 years. Isn't this indubitable?

Consider what is meant by the term "angle." Some say the theorem tells what will happen if you find a triangle lying somewhere and measure its angles. The sum should be 180 degrees.

If you actually do it, you find the sum not exactly 180 degrees. You retreat. It *would* be 180 degrees if you could draw perfect straight lines and measure perfectly. But you can't. You claim only that the sum will be *close* to 180 degrees, and even closer if you remeasure more accurately. That's *not* what Euclid said. It's a modern reinterpretation based on our post-Newtonian idea of "limit" or "approximation." Euclid's theorem is about the angles themselves, not about measurements or approximations.

A more sophisticated interpretation says Euclid's talking about ideal triangles, not triangles you actually draw or measure—an idealization that our mind/brain creates or discovers after seeing lots of real triangles. The indubitability of the theorem flows from the indubitability of the argument and the indubitability of the axioms with respect to the ideal points and lines.

The most important of the axioms about lines is the parallel postulate, "Euclid's Fifth." It can be stated in many equivalent forms. Most popular is Playfair's: "Through any given point, parallel to any given line, can be drawn exactly one line." For thousands of years the fifth axiom was accepted as intuitively obvious. Using it, we obtain Euclidean geometry, including the "indubitable" theorem about the angle sum of a triangle. But it was too complicated for an axiom, some thought. It ought to be a theorem.

In the nineteenth century Carl Friedrich Gauss, Janos Bolyai, a Hungarian army officer, and Nicolai Ivanovich Lobachevsky, professor and later rector at the University of Kazan, all without knowing of each other's work, had the same idea: Suppose the fifth postulate is *false*, and see what happens. Others had made the same supposition, in search of a contradiction that would prove the fifth postulate. Gauss, Bolyai and Lobatchevsky recognized that they got, not a contradiction, but a new geometry!

There is more information about non-Euclidean geometry in the article about Kant and in the mathematical notes and comments. Here we see how this discovery affects the certitude of the angle sum theorem. Suppose we replace Euclid's fifth by the non-Euclidean "anti-Playfair" postulate: "Through any point there pass more than one line parallel to any given line." From this strange axiom it follows that the angle sum of any triangle is *less* than 180 degrees. And if we try a different non-Euclidean fifth postulate—assume there are *no* parallel lines—then the angle sums of triangles are *greater* than 180 degrees.

We can't claim the angle-sum theorem is indubitable unless Euclid's parallel postulate is indubitable, for if we alter that postulate, the angle sum theorem is falsified. It's tempting just to declare that obviously or intuitively Euclid is correct. This wasn't believed by Gauss. There's a story that he tried to settle the question by measuring angles of a triangle whose vertices were three mountain tops. The larger the triangle, the likelier would be a measurable deviation from Euclideanness. According to the legend, the measurement was indecisive.

Some hundred years later, Einstein's general relativity depended on Riemannian geometry, a far-reaching generalization of non-Euclidean geometry. In relativity texts non-Euclidean geometry represents relativistic velocity vectors. So physics gives no license to favor Euclid over non-Euclid. Our prescientific, intuitive notions of space are learned on a small scale, relative to the universe at large. Locally, "in the small," Euclidean and non-Euclidean geometries are indistinguishable. Local intuition can't tell which is true in the large. Belief in the Euclidean angle sum theorem as indubitable was based on belief in an infallible spatial intuition. That belief has been refuted by non-Euclidean geometry and the following development of Riemannian geometry with its application in relativity.

Then where are we? Surely the angle-sum theorem is true in some sense? Of course. It's true as a theorem in Euclidean geometry. It follows from the axioms—not from Euclid's axioms alone, however. They need correction and completion. Hilbert took care of that, so we finally have a correct proof of the angle sum theorem, from a corrected and completed Euclidean axiom set.

We're in the same position as in the interpretation of

$$2 + 2 = 4$$

by Peano's axioms. We can't be completely sure which axioms describe the triangle we had in mind. By the nature of the case, such a thing can't be proved. Geometry is worse off than arithmetic, for its axioms are more subtle and elaborate. No one says Hilbert's axioms for Euclidean plane geometry are self-evident or indubitable.

Formula C is our last:

$$1 + \frac{1}{4} + \frac{1}{9} + \frac{1}{16} + \frac{1}{25} + \ldots = \frac{\pi^2}{6}$$

This formula was first written down by Euler. On the left is an infinite series. It says, "Add all terms of the form $1/n^2$, where n is any positive whole number." It asserts that the result of such an addition is the same as if you take π (the ratio of the circumference of a circle to its radius), square it, and then divide by 6.

The novice should be staggered. How can he ever add infinitely many numbers? And why would the answer have anything to do with a circle? The proof is easy when you know how to expand functions in series of sines and cosines (Fourier series).**

Formula C is abstruse and remote compared to Formulas A an B. You check

$$2 + 2 = 4$$

on your fingers. You check the angle sum theorem by measuring angles. What can you do here? Well, if you have a computer, or a calculator plus patience, you add up the series—as much as you have patience for. Then press the π button on your calculator, square and divide by 6.

A reader in Johannesburg sent these numbers (found after 30 hours on a hand calculator):

$$\sum \frac{1}{n^2} = 1.644914943$$

$$\frac{\pi^2}{6} = 1.644934067$$

What should you conclude from this?

If you're an optimist, and the error after 30 hours of calculation is only 0.000019124, you expect it would be even less after 40 hours. The formula is probably right. This is hope, not certainty.

No amount of addition could yield certainty, because the formula is about the *limit*—infinitely many terms! Any computation is finite.

If you want indubitable truth for this formula, you need *proof.* Fortunately, from the calculation of Euler or Fourier, which yields Formula C, it takes only a few more steps to such a proof.

The proof relies on the real number system. That has served long and well, but Gödel told us we can't prove it's consistent. Intuitionist and constructivist mathematicians don't rely on it without radical trimming. For practical purposes, we confidently write

$$\sum \frac{1}{n^2} = \frac{\pi^2}{6}$$

At the same time, if the angle-sum theorem and the formula "2 + 2 = 4" are subject to possible doubt, then this formula, involving limits, irrationals, and infinites, is more doubtable.