Forum

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The Immortality of Proof

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This is a response to the cover story, "The Death of Proof," in the October 1993 issue of *Scientific American*. More generally, it is a commentary on the circle of ideas touched on in that article.

The article suggests that the fashion of mathematicians stating theorems and proving them is passé. Various reasons have been offered for this transition: that mathematics has become so complicated that most mathematicians cannot understand other mathematicians' proofs; moreover, the proofs are too long and complicated. Wiles's proof of Fermat's Last Theorem is 200 pages; it would be 1000 pages if all the details were provided. W. Y. Hsiang's resolution of Kepler's sphere-packing problem is still, after three or more years, in doubt. (In fact some experts have told me recently that they believe Hsiang's proof to be incorrect—but at least these experts understood the material that Hsiang offers as a proof!) This time delay is also offered as tribute to the idea that things are so complicated that we don't know what we are doing.

Some have suggested that the logical foundations of mathematics are little more than a polite agreement. They are riddled with inconsistencies that are virtually insoluble. This is offered, it seems, as evidence that those dinosaurs (such as myself) who cling to mathematical tradition are being unreasonably stodgy. After all, what is the sense of defending something that isn't quite right? Why not allow that there are other ways to do mathematics besides proving theorems? Can we not establish truths by analyzing computer graphics or showing, à la Babai, that certain statements are "probably true"?

In the uproar that has followed hard upon the *Scientific American* article, it has been pretty well established that the theme of the article is a figment of the author's imagination. Nobody can find a mathematician who is willing to state in public that the proof is dead (anyone care to volunteer?). Evidently John Horgan (the author) only partially understood his interviews with a number of mathematicians. He fashioned a chimeral picture of what he thought he heard, weaving pieces of various interviews into a bizarre tapestry with no basis in reality. No matter what the genesis of the article that actually appeared in print, I think that the ideas presented there are dangerous—dangerous to you, to me, and to our profession and our subject. A number of good people have stood up and formulated their own answers to the article. This is mine.

Here is the truth: Andrew Wiles's proof of the Fermat theorem is not an "anachronism", as the article asserts. It is a triumph of the human intellect. Understanding it is not beyond our collective ken, as the article suggests. Gauss claimed to have been the first to discover non-Euclidean geometry. He did not publish because he did not think that anyone would understand what he was talking about. By contrast, students today learn about non-Euclidean geometry in high school. New ideas take time to become part of the infrastructure.

All over the world, in hundreds of seminars, people will go through Wiles's proof of Fermat's Last Theorem. It will finally be validated, or it will not. A good role model here is Louis de Branges's book on Hilbert spaces of holomorphic functions, which (purportedly) gave a proof of the Bieberbach conjecture. After a lot of thought and analysis by many mathematicians, there is now a two-page proof by Lenard Weinstein—based on de Branges's ideas, to be sure, but involving little more than calculus. Lennart Carleson's proof of the Lusin conjecture was also quite obscure. But, after a time, a great deal of study by many mathematicians, and an independent proof by Charles Fefferman, we now know that it is correct. That proofs evolve and are validated in this way is a tribute to the robustness of mathematics and to the process of mathematics. It is quite likely, if Wiles's proof is right, that simplified proofs of Fermat's theorem will evolve.

It is worth developing this last idea. Mathematics is not simply an endeavor carried out by certain individuals or small groups. It is in fact a process. Certain "point men" often come up with the final step—the proof—of a great theorem, but a close analysis shows the workings of the infrastructure to generate many of the ideas in the proof and to validate it afterward. This is part of what is so beautiful about the way that mathematics works—one person puts the final words on paper, but the entire community carries the ball (and makes sure that the ball has air in it).

It is true that Hsiang's proof is still in doubt. This is because very few people have read it or thought hard about it. In the three hundred-odd years since the problem was formulated, mathematics has evolved and dispersed in many directions. Until recently sphere packing has not been considered to be a central issue. (The recent book of Conway and Sloan will probably change that.) But the purported solution of the Kepler problem is just not the sort of problem—today—that will make mathematicians drop what they are doing to read the 150-page solution. Eventually we will know whether Hsiang is right, but most of us have had our attention diverted elsewhere.

Finally, about the foundations of mathematics: everyone knows about Russell's paradox and the problems with constructing the integers. But logicians have determined how to deal with these things. Morris Kline to the contrary, mathematics is not built on a foundation of sand. There is no area of human inquiry that is more robust or more solid than mathematics. One of the reasons that mathematics is of lasting value is that it is logically consistent. New generations do not shoot down old mathematics in favor of the new. For good mathematicians to claim otherwise is counterproductive and irresponsible.

Another point of view worth considering is this: while the foundations of mathematics are interesting and important, they have little to do with the everyday workings of mathematics. Waving Gödel's incompleteness theorem and Russell's paradox in my face is not going to stop me from thinking about complex analysis, and it should not stop you from thinking about geometric topology or whatever your chosen field may be. The Hilbert/Bourbaki view of mathematics as growing logically from solid foundations is a bit like Newton's view of physics: philosophically sound, but not the whole picture.

One of the themes of the *Scientific American* article is that proofs will soon be replaced by computer experimentation. Invoking recent ideas of Babai, it is suggested that computers can suggest to us that assertions are "probably true". Computer graphics can show us things that we cannot see unaided. It is important to sort out here the differences among computer simulation, graphical experimentation, numerical experimentation, "computer proof" (whatever that is), and the use of computers to graphically illustrate the meaning of a theorem that has already been proved by classical means (such as the computer graphics movie *Not Knot*). Let me point out that computer experiments could never have informed Yau's proof of the Calabi conjecture, nor the Calderón-Zygmund theorem about singular integrals, nor the work of Nirenberg/Treves and Beals/Fefferman on local solvability of PDE's, nor the work of Kohn on the inhomogeneous Cauchy-Riemann equations, nor Egorov's work on canonical transformations. Computers were used in one of the technical steps of de Branges's proof of the Bieberbach conjecture, but it is ludicrous to think of de Branges using the computer to generate power series of various Schlicht functions and staring at the coefficients to get ideas.

It is my understanding that the movie *Not Knot* has been used to great effect in getting high school students excited about mathematics. Many of those students have gone on to become math majors and then on to careers in mathematics. Any device that will generate talented American mathematicians, that will draw students back to math from business school, law school, computer science, and so forth is a godsend. But let us not, in our enthusiasm (with a pied piper like John Horgan), trick ourselves into thinking that the movie is a "computer proof" of anything. The movie *Not Knot* is a device for popularizing mathematics, something that needs to be done a lot more in this country. Most Americans are not even aware that there is a profession called "mathematician". There is nobody to blame for this but ourselves.

In many respects this is a golden age for mathematics—and I mean classical, rigorous mathematics done in the traditional way. There are fantastic collaborations taking place between geometers and PDE people, between geometers and physicists, between geometers and analysts (to name just a few). The rate at which enormous breakthroughs are being made is incredible. Yet the mathematics that often grabs the headlines is some new form of computer graphics. This is understandable, for the public is much more ready to consume computer graphics than pseudodifferential operators.

Yet this puts the onus on us, the dinosaurs, to figure out how to get the public to appreciate what we are up to. Don't forget that "the public" includes (1) potential graduate students, (2) senators and congressmen, and (3) the American voters. Also, program officers at the National Science Foundation (NSF) and other agencies read *Scientific American.* I wouldn't doubt that their ideas about funding are influenced in part by what they read. If you work in a field, such as I do, that does not lend itself to computer graphics in any obvious way, then you should think about how to let your graduate students, your undergraduates, and (if possible) a broader base of people know what it is that you do and why it is worthwhile.

Doing mathematics is hard. Programming computers (at least at the level of creating videos of a theorem that some smart guy proved ten years ago) is relatively easy. While the latter can be important in popularizing and communicating our subject, I hope that we will not commit the same error as John Horgan and think that it is the same as *doing* mathematics. Looking at this in a different way, let me point out that doing graphic or numerical experimentation to generate ideas for a proof or to provide enough examples to give one the courage to go on is a valuable exercise. But it is not a goal in itself. It has no intrinsic value.

At the risk of beating a dead horse, let me observe that dynamical systems is a vital, well-rooted, vigorous area of modern mathematics. Drawing pictures of fractals, giving them names like "XP-43", and printing them on picture postcards that sell for $1 a throw is not.

I was quoted in the *Scientific American* article to the effect that mathematicians are a bunch of "spineless slobs", unwilling to stand up and defend their subject. I regret this. I don't recall making the statement; in fact it is not expressed in my usual argot. But it is up to us to define what our subject is and to defend it. If I had in fact made this statement, I would now have to withdraw it; I have been pleased to see the great numbers of mathematicians responding to Horgan in defense of what we do. I'd like to think of Horgan's misquotation of me as a catalyst. Now let's consider it used up and dead.

As the *Scientific American* article reported, high school teachers in Berkeley are now minimizing proofs in Euclidean geometry. One argument in support of this change is that a computer can quickly test 5000 cases of an assertion so that proofs are no longer necessary. Well meaning though these changes may be (the teachers may have in mind students from disadvantaged backgrounds or students who have watched too much television and are permanently in the passive mode), they fly in the face of fundamental mathematical values. The article itself claims that students no longer appreciate the value of proofs. So we have to find another way to teach them.

Students do not appreciate the value of reading unless they are taught. They do not appreciate the value of good music unless they are taught. (Is anyone advocating that we replace Beethoven's Fifth Symphony by "da da da dum" and a video because it is too complicated?) And students will not appreciate the value and importance of mathematical thinking unless they are taught. It is our job to teach them, *not* to bend like reeds in the breeze. We should work with high school teachers to inculcate strong intellectual values in students, not to pander to their uninformed whimsy. I fear that the low salaries and lack of respect that high school teachers find to be their lot these days has given us a group of not very well trained high school teachers who are uncomfortable with proofs in Euclidean geometry. I recently asked a group of high school teachers in my city how they treat Euclidean geometry in school, and they didn't know what Euclidean geometry was.

Another interesting aspect of life is that bureaucracies like hardware. When parents come to a university to get an impression of whether they should send little Sally there for her four formative years, the leading lights in the administration do not trot out their rather shabby-looking Nobel Laureates and Fields Medalists. Instead, they show off their genetic engineering labs and supercomputer centers. Likewise, taxpayers understand money that is spent to buy PCs and software. They do not understand money that is spent to increase teachers' salaries so that we can get better teachers. Remember that we, the mathematicians in this country, are the caretakers of mathematical knowledge. It's not the books, and it's not the software; it's us. It is up to us to define what mathematics is and to defend it. Now that NSF grants are going the way of the dodo, maybe we will have more time to do so.

I am happy that the work of Jean Taylor and of David Hoffman and his group were given prominent mention in the article in *Scientific American*. These scientists are working in areas that can be informed by computer experiment (and also more visceral experiment—like dipping wire frames into soap solution). After they have done their experimentation, they then prove theorems. That way we know what is true. One of the triumphs of mathematics is that it transcends anecdotal information. The traditional definition of *theorem* is something like this: "the establishing of immutable quantitative or geometric truths by means of tried and exact reasoning". To replace this definition with "the offering of uninformed speculation after staring at computer graphics" would be both wrong headed and tragic. Tragic because it abrogates, without careful thought, everything that we've learned in the last 3000 years. Wrong headed because the value of traditional mathematics is well established while the value of the new stuff is not.

To use the work of Hoffman and Taylor to justify discarding proofs and replacing them with "probable proofs" and "graphical analysis" is like using the work of John Stuart Mill to justify anarchy. William Jennings Bryan was a great public speaker and so was Adolph Hitler. Therefore what? Again, the *Scientific American* article is using classical propagandistic techniques, such as confusing the converse with the contrapositive, to support the case that mathematical proof is being replaced with computer experiment. We all know that this is fallacious but will the readership of *Scientific American* know this?

I have used computer algebra to inform calculations that had become too complicated to handle by hand. I imagine that someday I will use graphics to help me to see something that I cannot see in my mind or with a pencil and paper. I advocate strongly that other traditionally trained mathematicians consider becoming conversant with these new tools. But the tools are not an end in themselves. Drawing a picture of a simply connected domain in the plane is not the same as proving the Riemann mapping theorem (RMT), even if it is a computer that drew the picture. I've been thinking about the RMT for over twenty years, and I've never used any kind of picture to aid my thoughts. Thurston, Rodin, and Sullivan have given us interesting new ways to think about the RMT that are very geometrical and do lend themselves to nice computer pictures. But the pictures do not prove anything.

And let us not misunderstand each other: pictures are valuable. They are particularly valuable in communicating mathematical ideas, but they are also valuable when you are in private, trying to solve a problem. The availability of wonderful graphics software now makes drawing complex pictures easy and offers us an exciting new tool. But if Gauss, smart as he was, had had a computer available to him, it would not have enabled him to prove the Riemann mapping theorem years before Riemann.

I hope that other mathematicians will discuss these matters and perhaps disagree strongly with what I say. The wolves are in our midst, and it is time for us to decide what we believe and what we value. One of the upshots of the discussions that have taken place since the *Scientific American* article appeared is that it seems unlikely that many of the wolves are mathematicians. But the wolves are still a danger. They have influence in the media and influence with funding agencies. We must be aware of the dangers that lie outside our cloister.

It may or may not be true that in ten or fifteen years we will have abandoned proofs and will be letting computers tell us what is probably true. But in ten or fifteen years it will be too late to decide what we want. We have to decide today.