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What happens when hubris meets nemesis
By Gary Taubes

I have committed the sin of falsely proving Poincaré's conjecture. But that was another country; and besides, until now no one has known about it.
-- John Stallings

Conjectures are what mathematicians call their guesses. Once a conjecture is in print, it becomes a challenge, daring them to prove it, which quite a few will try to do. The more they work at it and the more they fail, the more obsessed they become. After a while, it no longer matters whether the conjecture is still in the mainstream of mathematics, only that it's still unproved.

Late last year, after Colin Rourke, an English mathematician, became the latest victim of what's known as the Poincaré conjecture, Dave Gabai, of Caltech, explained the obsession.

"For a mathematician," said Gabai, who has a reputation for cracking seemingly unsolvable problems, "proving the Poincaré conjecture is fame, honor, and prestige." And that's the name of the game. Will Kazez, a Caltech colleague, added, "If you did prove it, maybe some people would say, 'Well, what's it good for?' But they would say it only because they weren't the ones who proved it."

Mathematicians speak of Poincaré's conjecture like Ahab expounding on the White Whale. And once they get involved, and when the conjecture inevitably wins the first round, they become as obsessed with their pursuit as Ahab was with his. "They say, 'I'm going to be the one to get it,' " says Rourke, who knows all too well whereof he speaks. "And they don't do anything else, and a good mathematician goes down the drain."

Considering that the conjecture is only one of many unsolved problems in its field, which is topology, the branch of mathematics concerned with the fundamental properties of structures and spaces, the obsession seems misplaced. The conjecture is so alluring perhaps because it is so fundamental and seems so simple. Tantalizingly so. But it seemed simple 83 years ago when Poincaré first tossed it off -- an incredible teaser, one topologist calls it. Penn State's Steve Armentrout, who has tried without success to prove that the conjecture is false, puts it this way: "The Poincaré conjecture requires not only everything you've got but one hell of a lot more."

Over the years, some very great mathematicians and some not so great ones have thought they had proved the thing, and, except for those who died not knowing any better, have had occasion to regret their presumption. The exact count of misbegotten attempts isn't known. Each year several mathematicians deceive themselves into believing they've proved the conjecture. Maybe half of these arrange to keep their failures private. A slightly lower percentage expound their proofs to a colleague, or worse, air their folly at a seminar. In a very small number of cases, the mistakes aren't found quickly, and these have a way of becoming public, which is what happened to Rourke. By the time he arrived in Berkeley last November to discuss his proof at a week-long seminar that became the gentlemanly equivalent of an inquisition, his purported success had been touted in several newspapers and science magazines.

Rourke, 44, is a large, affable man from the University of Warwick who's also a part-time farmer. After getting his [redacted]. at Cambridge, he was known as an up-and-coming topologist. He was teaching at Warwick in March 1985 when Eduardo Rego, his 35-year-old post-doc, showed him a proof of a theorem that Rourke figured led directly to the conjecture. The two men went after the proof without even taking time to consider, in the words of topologist John Morgan of Columbia University, "that there was too much history on this one."

By February 1986 Rourke and Rego were confident they had a proof and said so. The responses of knowing mathematicians ranged from cynical to cautiously optimistic. Even among those who thought the proof might be valid, Rourke didn't gain any popularity: they objected to the idea that a man working with less than brilliant ideas should have been the one to pull it off.

Perhaps for this reason, many topologists chose not to attend the Berkeley seminar. Among the absentees was Mike Freedman of the University of California at San Diego, who had proved the Poincaré conjecture for a limited case -- that of a four-dimensional universe, as opposed to the conventional three-dimensional one. Freedman, like a number of his colleagues, had decided that if Rourke's proof was wrong, they would let others expose the flaws. Still, even Freedman admits that his first reaction was disappointment: "Well, there goes the best problem around." Berkeley's John Stallings, who had helped prove the conjecture for universes with six or more dimensions, a more abstract, though easier, task, refused to deal with more of what he called "deluded junk." He attended the first session of Rourke's seminar only, and that, he says, was for the sole purpose of making a "psychological diagnosis": whether Rourke "was crazy or not."

Berkeley topologist Andrew Casson went to the seminar, but only to play the role of Grand Inquisitor to Rourke's mathematical heretic. A few participants, including Robion Kirby, the seminar's organizer, who created a technique called Kirby calculus that Rourke had used extensively in his proof, were hoping for the best. But then, Kirby also considered Rourke a friend. And Kirby worked predominantly in the fourth dimension, not the third (mathematicians, like foreign-car mechanics, are highly specialized), and so had no great attachment to a three-dimensional conjecture. A few others were also hopeful, thinking that Rourke's proof had a certain elegance and power. But even they came to the inquisition wearing "proof buster" T shirts.

In sum, Hubris was about to meet Nemesis.

Henri Poincaré, the man who left the conjecture like a family curse to his intellectual heirs, was born in 1854. He taught at the University of Paris from 1881 until his death in 1912, dominating the mathematics of his day. Poincaré was a universalist. He worked in everything from pure math to physics and astronomy, and he did it all in his head first, which perhaps accounted for his reputation as an absent-minded soul. His greatest creation was topology, or what he called analysis situs, which was regarded at first as more or less trivial, and has since become a preeminent part of twentieth century math.

Topologists are concerned with those properties of objects and spaces that transcend mere geometry. To the topologist, a sphere, for instance, can be stretched, compressed, or warped in any number of ways and still remain a sphere, so long as it isn't ripped or torn. "Say you're given a ball," says one Harvard mathematician, "which is a topological object. You push it in on one side, pull it out on another. Twist it a bit. Maybe you run over it with your car a couple of times, let your baby play with it. To a topologist, if it's not ripped, it's still a ball."

Topologists, so the joke goes, can't tell the difference between a coffee cup and a doughnut. Both are examples of what they call a torus: a topologist can deform the former into the latter simply by shrinking the depression in the cup and the size of the cup itself, while enlarging the handle.

Of particular interest are manifolds, which are spaces that are finite and that look flat on a small scale but are capable of being curved in any sort of bizarre fashion on larger scales. This isn't as weird as it sounds if you consider that the surface of the earth is a manifold (to be precise, a two-dimensional manifold; topologists are concerned only with surfaces and not interiors). Up close the planet looks flat to us, and it might look flat from our point of view whether it was shaped like a doughnut, a pretzel, or an infinite number of other curved objects.

The universe, to a topologist, is a three-dimensional manifold. Like the surface of the earth, it looks flat from our point of view -- light appears to travel to us in straight lines, whether from galaxies or quasars or the top of the Empire State Building -- but, in fact, could be curved if we could only see it from the right vantage point. Whether the universe does eventually curve into a three-dimensional version of a sphere, or a doughnut, or even some topological version of a sailor's knot, no one knows.

Ideally topologists would like to identify all possible manifolds, including the shape of the universe -- which is what Poincaré's conjecture is all about. This is a relatively easy task in two dimensions, and had been achieved by the end of the nineteenth century. But it has been less easy in three. The simplest possible manifold in two dimensions is the sphere -- the two-sphere to cognoscenti -- and there's only one type. There are no fake or exotic spheres that have all the by-the-book properties of a sphere while in some utterly unimaginable way are not the two-sphere.

To topologists, the test of a true two-sphere, since it can take on almost any shape at all, is whether or not it's what they call simply connected (see diagrams, page 73), which is easier to understand than it sounds. Imagine a kind of magical rubber band placed randomly on the surface of a basketball, say. If the rubber band can be shrunk down to a point without ever leaving the surface and if that can be done anywhere on the surface, the basketball is simply connected. And that means, to the topologist, that the basketball must be a sphere, even if it's deflated. However, if an object has a hole in it -- a coffee mug, say -- the rubber band could pass around the handle, and it would be impossible to shrink it to a point without cutting through the handle. Therefore, it isn't simply connected and it isn't a sphere.

In 1904 Poincaré conjectured that what was true for two dimensions was also true for three: that any three-dimensional manifold -- the universe, for instance -- that was simply connected had to be the three-sphere. (In three dimensions, topologists use a magic balloon instead of a magic rubber band, and the now three-dimensional surface is simply connected if the balloon placed on it at random can be shrunk to a point. If there were some kind of hole in space, the universe would no longer be a sphere and the balloon would get caught up on the hole in the same way the rubber band could get caught on the handle of the mug.)

The conjecture is the simplest question a topologist can ask if he wants to begin to classify all possible three-dimensional manifolds.* And it sounds, as academic types like to say, intuitively obvious. Except that nobody has ever shown that there aren't some kinds of fake three-spheres, which means the conjecture has never been proved true. If such counter-examples were ever found, then the conjecture would be false, but that hasn't happened either.

In 83 years the only progress, so to speak, has been negative. No one has even been able to imagine what properties a fake three-sphere might have that would make it recognizably different from the real one. As one topologist says, "Even if you had fake three-spheres up to your eyeballs, how would you know they were fake?" A Soviet mathematician had suggested that a particular aspect of three-dimensional spaces (known as the Rochlin invariant) might somehow be used to distinguish a fake three-sphere from a real one. In 1985 Casson proved it couldn't. End of progress.

What's ironic in this Poincaré business is that the higher-dimensional versions of the conjecture have turned out to be easier than the three-dimensional ones. In the spring of 1960 Stephen Smale, a Berkeley topologist who had tried proving the three-dimensional conjecture often enough to know better, announced that he could prove it was true for any number of dimensions higher than five. At the time he was spending a year in Rio, and some of his colleagues insinuated that he had developed the proof while lying on the beach, which Smale doesn't deny. Stallings heard of Smale's proof back in Oxford, "after a winter of playing Monopoly," and came up with another one for dimensions greater than six. Then Christopher Zeeman, at Cambridge, proved the conjecture in five and six dimensions, and Smale provided one proof for all dimensions higher than four. In 1981 Freedman announced his proof in four dimensions, which Kirby called "the best piece of mathematics around." Freedman, who was then 30 and had been working on the problem since becoming an instructor at Berkeley in 1974, attributed his success to what he calls the arrogance of youth.

In retrospect, this success in higher dimensions wasn't surprising. Topologically, the universe becomes roomier as you add dimensions to it, imaginary though they may be. In effect, Smale and Freedman went about their proofs by showing that they could take any strange and tangled manifold and simplify it so they could compare it with various spheres to see if it was equivalent. To do this they used two-dimensional discs as if they were marlinspikes to unknot a tangled manifold wherever it might cross over itself.

The more dimensions they postulated, the more space they had to play with. But in three dimensions (and to a lesser degree in four) space becomes so tight that the discs often cross themselves, which creates more problems than it solves. The proof of the conjecture in higher dimensions has turned out to be the breakthrough that led to a theory of how to classify all the manifolds in those dimensions, which is what topologists are really after.

As early as 1939 topologists learned how to live with the uncertainty of the conjecture by dodging it: if the conjecture was true, they said, then this was true, and if the conjecture was false, then that was true. Three-dimensional topology moved on and left the conjecture in the boondocks. The principal reason anyone might go out of his way to prove it was because it was, well, there.

Rourke's predecessors in the assault on the Poincaré conjecture make up a long and illustrious list. The great English mathematician J. H. C. Whitehead took the first public shot at it after Poincaré himself, in 1934. Whitehead was renowned for his brilliant topology, his left-wing politics, and his ability to drink colleagues under the table. (As his students tell it, Whitehead died at eight o'clock in the morning while walking back from an all-night undergraduate poker game at Princeton.) A year after he revealed his proof, he found his own mistake, published a correction, and then the next year a retraction.

The conjecture went pretty much uncontested until the 1950s, when the action was resumed at the Institute for Advanced Studies in Princeton, in particular by a young Greek mathematician named Christos Papakyriakopoulos. Papa, as he was known, had arrived in the U.S. in the late 1940s, and on a single day in 1956 announced proofs for three of topology's most important theorems: Dehn's lemma,* the sphere theorem, and the loop theorem.

Most topologists assumed that they had finally been given, courtesy of Papa, the analytical technology to tackle three-dimensional manifolds and the Poincaré conjecture. They were mistaken. The conjecture went through good topologists the way Marciano went through heavyweights.

First to fall was Papakyriakopoulos himself, who had pursued the conjecture with monomaniacal dedication, refusing to teach classes, for notable instance -- which meant rejecting a tenured position -- because it took time away from his quest. He turned down dinner invitations, and lived unmarried and unencumbered in a one-room apartment. When he died, in 1976, his colleagues found among his possessions a neatly handwritten 160-page manuscript. It was a program to prove the conjecture and classify all three-manifolds. At the top of one of the pages, according to Stallings, was written "Lemma 14." The remainder of the page was blank. "So he was going to come back and fill in this proof," says Stallings, "and he never did."

Next came two transplanted Europeans, Wolfgang Haken and Valentin Poenaru, both of whom had taken up residence at the Institute. Haken was the product of an Old World German education. He had begun his doctoral thesis on a problem related to the conjecture, before veering off into knot theory and proving how to recognize whether a knot was really knotted or not. But his proof went unnoticed; not only did it run to several hundred unreadable pages, but his non-algebraic approach to knot theory was no longer in vogue. "I didn't know it had gone out of style," he says. ("The more you know, the more prejudices you have," he adds. "If you know too much, you're no good any more.") In 1963 his wife, also a mathematician, suggested that if it was attention he craved, he should get out of knots and into Poincaré. Having seen Papakyriakopoulos's work, Haken figured the conjecture "should be immediately possible to prove."

Poenaru, his rival, defected from Rumania in 1962 while attending a conference in Sweden, and sought asylum in France. He had made his name while still in Bucharest, solving a major problem concerning four-dimensional balls. In 1964 he moved from Harvard to the institute, where he set out after Poincaré.

Neither Poenaru nor Haken was aware that the other was working on a proof. Poenaru announced first, in 1964, while Haken was lecturing in West Germany. When he heard he had been beaten, Haken locked himself in a room, wrote up his proof, and went public as well. Later they had time to talk and found that their proofs were nearly identical and that both had succumbed to the same error.

Then they both fell victim to a disorder that Haken calls Poincaré-itis -- a morbid and obsessive belief that the conjecture can be proved, complicated by an utter inability to admit failure. The cause lies once again with the fact that the conjecture seems so simple. When mistakes are made, they too seem simple. Everything about it seems eminently solvable if one would only spend enough time working out the details.

As late as 1970 Haken still believed that he could get the proof. "The first ten years were fun," he says, "because I always thought I should soon have it. Then it became depressing. Whenever I had a good idea I felt I had an obligation to go after it. But it would take me another year to see why it wouldn't work. Finally, I lost confidence, and said, 'This is no life.' " In 1973 Haken cured himself of his addiction with the help of an equally famous conjecture, the four-color problem,* which he solved with colleague Ken Appel four years later. "I shall not go back," he vows, then adds, in a disconcerting way, "unless it's to look for counter-examples." As for Poenaru, when he resumed work on the conjecture recently, one of his colleagues' wives was said to have commented, "Poor Valentin, he's got the bug again."

(Perhaps the classic case of Poincaré-itis was that of Auburn University mathematician Andrew Conner, who took up the conjecture in the late 1960s and worked on it unrelentingly.

Many times he thought he had proofs. Just weeks before his death from cancer at age 43 in 1984, Conner again announced a proof. Haken and four other mathematicians were summoned to his deathbed to hear the details. The proof was in three steps. Haken verified the first two, but the third seemed flawed. By then Conner was too weak to discuss it. "I don't think it would have worked," says Haken.)

Successful topologists tend to have a highly developed faculty for mental imagery. This is especially true of geometric topologists, as opposed to the algebraic types, for whom the Poincaré conjecture is of less interest. The difference between the two, says Freedman, "is that with the former the shapes are running around in their heads rather than the formulas." As an aftertought, Freedman says, "It's amazing that if your talent is spacing out and daydreaming and picturing things in your head, there's somewhere to go in our society so that you're happy."

Blessed with a vision that Freedman finds "almost incredible,"Bill Thurston, 40, of Princeton effectively changed the game of three-dimensional topology in the mid-1970s. The legacy of Papakyriakopoulos had been spent, and Thurston replaced it with what's now known as the geometric representation of manifolds. Thurston, who got tenure at Princeton at 27, is an iconoclast even for a mathematician. "He has a genius for coming up with perfectly off-the-wall stuff that miraculously seems to mean something," says Morgan.

Thurston himself put it this way: "I think of mathematics almost as observations. There is somehow a mathematical world which is really there. Certain patterns in it fit together in certain ways. It's just a question of learning to see what they are."

In 1976 Thurston saw three-dimensional manifolds as purely geometrical, which isn't the way his fellow topologists see them. ("A crystal is a geometric thing," says Freedman. "A ball of putty is sort of a topological thing.") In essence, Thurston was saying that each of the malleable manifolds of the topologists had a preferred geometry, like a drapery hanging over a crystalline framework that had an exact geometric structure. Thurston used this insight in putting forth a conjecture of his own, which surmised that every three-manifold can be cut into pieces and each piece will be one of eight types of crystal. Says Larry Siebenmann, of the University of Paris, "It came out of the blue, like a great sledgehammer."

Thurston's work, like Papakyriakopoulos's twenty years before, had the theoretical power that topologists figured was needed to conquer the Poincaré conjecture. Not only was it elegant but it also opened up topology to geometry, and geometry had what mathematicians call structure. "Some areas of mathematics are very rich in structure," says Peter Shalen, a topologist at the University of Illinois. "You can do mental leaps all the time from one kind of problem to another." By linking it with geometry, Shalen says, "Thurston made three-dimensional topology a field where you could do the kind of mental gymnastics that had never been possible before. That in turn made it possible to introduce all kinds of techniques."

Thurston's vision also looked like a sure route to understanding and classifying all three-manifolds, which he says is all he wanted to do. "I think too many people have wasted too much time in trying to prove the Poincaré conjecture," he says. "Even when it's proved it will only tell the answer about one three-manifold, which is the three-sphere."

Actually, the proved portion of Thurston's geometrization conjecture, called the geometrization theorem, didn't address all the types of manifolds considered in Poincaré's conjecture. "It's on the other side of the fence," says Siebenmann. Other parts of the geometrization conjecture do, and if they could be proved, mathematicians figured, Thurston would do it and the Poincaré conjecture would fall. "I'm not at the end of my rope yet," says Thurston.

By 1980 most topologists felt that any further traditional frontal assaults on the conjecture would be fruitless. Some, like Freedman and Casson, would have liked to be working on the problem, but knew that they didn't have any ideas powerful enough to give it a go. "You can't just sit down to work on it," says Freedman. "You realize your head's blank, so what do you do next? I'd drop other things in a moment if I thought I had an idea." Most topologists were betting that if the conjecture was going to be proved soon, the solution would come from either Thurston or an unknown mathematician too young to have been brainwashed by the conservative approaches.

The latter is what happened in 1983, when Freedman used the work of an Oxford grad student named Simon Donaldson to prove that, mathematically speaking, there had to be at least two four-dimensional universes, which was as surprising a piece of mathematics as had come along in a while. Donaldson had made it possible by borrowing techniques from theoretical physics, which, as Freedman says, "seemed like a kind of a weird thing to associate to a manifold."

It was this kind of novelty that Colin Rourke was hoping for when he developed his proof of the conjecture without reading up on any of the previous attempts so as not to bias his creativity. His colleagues, however, thought that in ignoring the history he had come up with nothing more than a revised edition of Haken's approach, with a couple of added attractions that gave it, in Rourke's words, "extra strength." The Kirby calculus, for example, provided Rourke with a language with which he could compare two apparently different manifolds. He was certain that where Haken's approach ran into a dead end, his did not.

To his colleagues Rourke's proof was disappointing. It wasn't elegant. "It didn't seem to spin out interesting small observations as you go along," says Freedman. "Thurston's work, in contrast, is littered with small, beautiful facts that you can put in your pocket." Thurston himself states it a little more brusquely: "I don't think I would have wanted to do that proof myself."

When Rourke arrived in Berkeley in November, his proof had already been through two iterations -- he had caught one mistake himself, prompted by Haken's discomfort over some of the logic; Haken had caught another. And then there was all that unseemly publicity.

Rourke had started the whole thing with a press release written with Ian Stewart, a Warwick mathematician and a science writer. Stewart also published articles in the Guardian and Nature. Subsequently the two co-authored a long, colorful article in the New Scientist. (Says Stallings, who isn't known for equivocation, "I imagine the main motive Stewart has is to make noise about mathematics in the hope that the government will give money. Math is basically a cheap, if esoteric, art and the government ought to give it money.") On top of all this, Rourke was quoted in The New York Times as saying that he considered the proof "cut and dried," and that if many mathematicians hadn't read his paper yet, it was because they were "sort of lazy." In retrospect, he says, "I was so convinced about the proof that I fished for an invitation to go lecture on it" -- which Berkeley took him up on.

"He was really sticking his neck out," says Kirby. "I like the guy and would have liked to see him succeed. But since he stuck his neck out, we thought we should just get this business cleaned up. Mathematicians were looking a little foolish."

The crack about laziness had struck a particularly responsive chord, since most of those who had tried to read Rourke's 100-page proof couldn't get through it. A rigorous mathematical proof usually begins with known theorems, then introduces new techniques, explains what they are, indicates what they can do in specific settings, and then shows one after another, how they nail the proof down for all possible cases. But, says Kazez, "in Rourke's proof the last fifty pages didn't have a single clearly stated lemma, proposition, or theorem. It just went to hell."

Rourke defended his proof by claiming it was written as one enormous construct. He describes the form: "You do this to this; you do this to this; you do that to that; you do that to that; and then you end up checking a whole list of things." His peers, he said, only wanted something that could be broken down into small, comprehensible bites, and his proof couldn't. Few bought this line of reasoning. "We were afraid," said one mathematician, "that his proof wasn't sufficiently clear to be wrong."

The mathematicians at Berkeley were also worried about the seminar's ground rules. The way it was shaping up, either they would find an error in Rourke's proof in the five days allotted or he would go home triumphant. "This attitude is exactly wrong," says Stallings. "The burden of proof of a mathematical result is on the prover."

Although Kirby had organized the seminar, the de facto master of ceremonies was Casson, an Englishman who invented Casson handles, a mathematical device upon which Freedman had relied for his four-dimensional proof. Casson had left Cambridge, so it was said, because the university was unhappy over his reluctance to publish his work. Berkeley was more amenable. "This field is so small," says one former Berkeley topologist, "that everyone knows he's brilliant and that he's come up with brilliant ideas all the time. He doesn't write them down, but he explains them to everybody in a way that's so clear that everyone gets it." Casson had tried the conjecture several times himself and had come out of it, he says, with an understanding of why his attempts didn't work -- which he could then use to pinpoint the flaws in somebody else's proof.

Collaborating closely with Casson was Gabai, who had been in on the proof from the beginning. Gabai was visiting Warwick in the spring, when Rourke was giving seminars on his proof. As the only three-manifold expert in the neighborhood, Gabai felt a responsibility to look into the proof, and came away impressed. "It was extremely seductive," he says.

They were joined by Kazez, as well as Hamish Short, a student of Rourke's, and two graduate students studying under Kirby. Gabai had tried to enlist Haken as well, but Haken declined, although he did suggest points at which Gabai should direct his suspicions. When Gabai and Kazez arrived from Caltech, they briefed Casson and the stage was set.

The seminar began smoothly, with Rourke outlining his proof to give his audience an idea of where he would be going and discussing the details of how to get there. Then he worked back and forth between the two, taking questions as they arose. "People would mention all sorts of minor objections," says Kazez. "Some of them [redacted] explain away. Others [redacted] say, 'Well, you're right, but I didn't mean quite that.' [redacted] state a modification, and it would go on."

By the third day, says Gabai, "it was clear that what Rourke had written down in his manuscript and what he was telling us the proof was were just completely different." Rourke's proof continued to evolve with each objection. As Casson directed the prosecution, he seemed to have in mind a particular plan of attack, although he blames himself for not having seen Rourke's mistake earlier. "I knew what kind of snag I was looking for," he says. "I just didn't know exactly where it would be found."

On the fourth day Gabai and Casson found it. In a discussion the night before, they had narrowed down the problem area. To get around Haken's dead end, Rourke had added what he called a tag-breaking cascade, which might have been considered his ace in the hole. Neither Gabai nor Casson trusted it. The next afternoon, Casson questioned a diagram in the dubious part of the proof. Rourke gave what seemed a satisfactory answer. A short while later, as Rourke was working through a related theorem, Gabai innocently asked him to backtrack on another minor point. "If it wasn't for that it was O.K.," says Gabai. "I just wanted him to explain it again. And he looked at the detail, and he said, 'But we just break that cascade.' And we went, 'Whoa! You can't break it, because you're only able to break tags, and this is a pseudo tag, or whatever.' And that was it."

The mistake turned out to be a reincarnation of the one Haken had exposed six months earlier and that Rourke thought he had resolved. On the last morning Rourke came in suggesting he could patch it up. The front line of Kirby, Gabai, Kazez, and Casson sat back and sent in the second team of Kirby's two graduate students, Mike Hirsch and Kevin Walker, to clean up. "These guys were great," says Gabai. "They were like hounds after the stag, right on the jugular of poor Rourke. They just chewed him up." Says Kazez, "By the end, Rourke was admitting it was a disaster. He said that he's sure the manuscript is incomplete, which isn't the appropriate sort of statement to make when you're defending one of the most famous problems in mathematics." Next day Gabai wired Thurston an update via computer. Its title: "PC Nuked!"

With his proof and his credibility both shot, Rourke took time off to recover. He was, he admitted, "a bit shattered and depressed." Kirby summed it up: "The conjecture is an all-or-nothing proposition. Either you get the whole thing or you don't have anything. Let me put it this way: if Rourke were a mountaineer, [redacted] be dead."

As late as December Rourke felt that he could still pull it off. "It's very tantalizing, actually," he said. "The proof is down to technical details that look so ridiculously tacklable. Kirby's right, though. Until the last detail is written down you don't have a thing." When it was suggested that he was beginning to show symptoms of incipient Poincaré-itis, and that he could be spending his remaining years tilting at the conjecture, he replied glumly, "I hope I don't."

And so it goes. Poenaru, now living in Paris, has been circulating an outline of a proof that weighs in at 120 pages and which he claims is a 20-to-1 condensation, which means that the proof itself would be 2,400 pages. Even in this abbreviated version, it hasn't elicited any response. "No journal will publish such a paper at this length," Poenaru says, "without knowing that it actually proves the Poincaré conjecture." Arthur Everett Pitcher, a mathematician at Lehigh, has also been circulating a prospective proof. His latest version was shot down by Stallings, who says, "It was roughly the same thing Poincaré had messed up on in 1904." A few years back, Armentrout put out a 300-page paper that proved the conjecture was false. It was promptly knocked down by Haken, which, as Armentrout says, "makes one naturally rather cautious." He's still trying to disprove it.

How the next proof of the Poincaré conjecture will be received seems to depend as much on whose it is as how it goes about its mathematical business. As Kazez says, "If Thurston were to just say the Poincaré conjecture is proved and write down a one-page outline, everyone would believe it. And they'd scramble to get the first copy of the manuscript." Then he adds, laughing, about his friend Gabai, "If Dave said it, people might dread haing to read what he writes, but they'd believe it. It's a question of credibility. If you've been knocking off famous problems, then people would read it. And if you're just some ordinary person, if the first twenty pages are interesting and you've done something new that will suck in readers, they'll read it. Otherwise . . ."

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Box: How you might prove the universe is a three-dimensional sphere

In the eyes of topologists, Magellan didn't really prove that the earth is a sphere. What he showed was that there was at least one long path on its surface that could get him back to where he began. The earth could have been shaped like a doughnut or a pretzel and Magellan would never have been the wiser.

But a very rich monarch, says Princeton's Bill Thurston, the topologist who's the odds-on favorite to prove the Poincaré conjecture, can establish that the earth is a sphere without ever leaving home. Let's imagine that this mathematically inclined potentate lived at the South Pole. He could send out simultaneously in all directions thousands upon thousands of surveyors. "As you head north," he would tell them, "maintain eye contact with your colleagues on both sides of you."

These surveyors would fan out and travel north, forming a circle that would get larger and larger as they got closer to the Equator. As they passed into the Northern Hemisphere the circle would shrink, finally contracting to a point when all the surveyors arrived at the North Pole. By meeting there, they would have demonstrated that, topologically speaking, the world was a sphere.

This is what topologists call sweeping a one-dimensional circle (the surveyors' circles) through a two-dimensional manifold (the earth's surface). If the world hadn't been a sphere but a doughnut, for instance, some of the surveyors would have taken the long way around the doughnut and others the short way, and they would have met in small groups at various points on the doughnut, but not all together.

The same test, with an added dimension, could prove that the universe is a three-dimensional sphere. In this case, the monarch sends out surveyors in millions and millions of spacecraft. The ships are launched simultaneously from the earth's surface -- once again with orders to maintain contact with neighboring rocket ships -- and head straight out at the same speed through the universe, in an ever expanding sphere. Now instead of the circle, it's the two-dimensional surface of the sphere that gets larger and larger until, at some unimaginable point, the ships pass over the equator of the universe. From then on, the sphere of vessels contracts until, as Thurston whimsically puts it, "all finally arrive together at the Garden of Eden on the opposite side of the universe." The space ships, the topologists would say, have just swept a two-dimensional sphere through a three-dimensional manifold and, in so doing, have proved the latter to be a sphere.

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