The Millennium Problems: The Seven Greatest Unsolved Mathematical Puzzles of Our Time

Reviewed by Brian E. Blank

The Millennium Problems: The Seven Greatest Unsolved Mathematical Puzzles of Our Time
Keith J. Devlin
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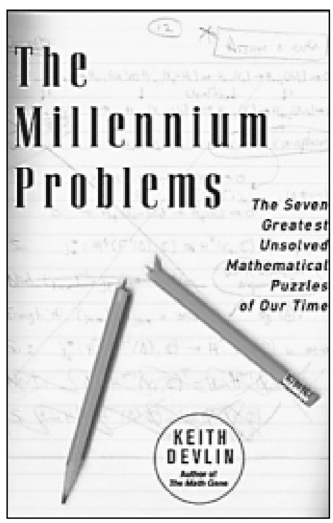
On May 24, 2000, the Clay Mathematics Institute (CMI), inspired by the centenary of the Hilbert Challenge and seeking to put its own stamp on the new century and perhaps beyond, announced awards of one million dollars each for solutions to seven "Millennium Prize Problems" [6]. The problems selected by the CMI were neither new nor concerned with pressing practical matters. Ordinarily the popular press would not have found them newsworthy. But, as the CMI correctly reckoned, seven million dollars concentrates the mind wonderfully. Journalists worldwide reported the story, stirring up so great an interest that the CMI website was quickly overwhelmed. It is reasonable to suppose that most of the fuss was concerned not with the zeros of the Riemann zeta function but with the zeros of the prize fund.

The custom of prize problems in mathematics is an old one. The funds have variously come from governments, societies, interested amateurs, and mathematicians themselves. Among the latter, Paul Erdős is no doubt most associated with the practice of placing bounties on obstinate problems. It



is reported that $10,000 was his greatest offer, $1,000 the greatest claim on his funds. Several other mathematicians have announced prizes of a comparable size. The £1,500 offered by Thwaites for the solution of the $3x+1$ problem is possibly the best known of these. In 1997 Andrew Beal, a Dallas banker with an interest in number theory, upped the stakes considerably by announcing the Beal Conjecture and Prize, worth $100,000 at the time of this writing. During a relatively brief publicity stunt, the Goldbach Conjecture carried a one million dollar price tag. There have been even more lucrative prize funds. At the time of its endowment, the purchasing power of the most famous of all mathematical prizes, the Wolfskehl Prize for the solution of Fermat's Last Theorem, came to about 1.7 million dollars (as measured in 1997 currency) [1]. These sums for abstract problems stack up very well against the £20,000 reward (worth more than three million dollars in today's currency) that the Parliament of England established in 1714 for the invention of the marine chronometer, an instrument

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of incalculable commercial and lifesaving importance.

Using the Wolfskehl Prize as an example, Barner has convincingly argued that a conspicuous prize can play an important role in registering mathematics on the public's radar screen [1]. The question is, now that the initial buzz has died down, will the Millennium Prize Problems retain an interest outside the professional community? With his new book, *The Millennium Problems: The Seven Greatest Unsolved Mathematical Puzzles of our Time*, Keith Devlin joins a group of authors who are betting that the answer is "yes". Indeed, there does appear to be a ready market for books of this sort. The enduring popularity of elementary (albeit arduous) classics such as Dörrie [3], Khinchin [7], and Klein [8] compellingly illustrates the lure of the difficult problem. Recently there have appeared popular accounts of the Kepler Conjecture and the Four-Color Problem, three books on the Riemann Hypothesis (of which I have read only [9]), and two books on the entire set of Hilbert problems [5], [11].

Several of the cited authors liken unsolved mathematical problems to challenging mountain peaks. The analogy is a useful one, because it highlights differences as well as similarities. You might be surprised to know that climbers make the same comparison. In his account of the recent expedition that found George Mallory's body on Mount Everest, Doug Bell wrote, "Climbing a truly high mountain is not unlike the academic quest for truth. Both require rigid discipline and yet a flexibility of approach—and to reach either goal a person needs both the ability to slog along hour after hour and the finesse to find a way past tricky narrows." To Devlin "the seven Millennium Problems are the current Mount Everests of mathematics." They are, in the order of appearance in his book: the Riemann Hypothesis, Yang-Mills existence and the mass gap, the *P* versus *NP* problem, the Navier-Stokes existence and smoothness problem, the Poincaré Conjecture, the Birch and Swinnerton-Dyer Conjecture, and the Hodge Conjecture. The number of pages devoted to each ranges downward from 44 to 16 in what would have been a monotone decreasing sequence but for the graphics in the Poincaré Conjecture chapter. Addressing a reader who is assumed to have no more than a good high-school knowledge of mathematics, Devlin sets himself four tasks: "to provide the background to each problem, to describe how it arose, explain what makes it particularly difficult, and give you some sense of why mathematicians regard it as important."

Now if Devlin's audience consisted of well-prepared second-year graduate students with eclectic tastes, then his agenda would seem ambitious but worthwhile. Devlin, however, is serious about reaching a much less sophisticated audience. When his readers encounter "10!" he feels obliged to tell them, "You don't read this aloud as 'ten' in an excited or startled voice." Surely it is pointless trying to convey to such readers just how difficult the millennium problems are. Experience is required for such a judgment. Those of us who have never climbed even a moderately difficult mountain cannot appreciate how hard it is to summit Eiger via its north face. But Reinhold Messner tells us that it is a scary ascent, and we are prepared to take his word for it. That is pretty much how Devlin achieves his goal of explaining the particular difficulties of the millennium problems. He shows us no failed routes to the top, no lines of attack that might get us there if only we could see them through.

Establishing the importance of the millennium problems is another task that is not easily accomplished. Devlin is correct to assume that for his audience "We must know! We shall know!" will not suffice. I suspect that of the seven millennium problems, only *P versus NP* has the type of significance that will resonate with the majority of Devlin's readers. He explains it to them well. Not so with the other problems. To affirm the importance of the Mass Gap Hypothesis, Devlin quotes Witten, who tells us that "the proof would shed light on a fundamental aspect of nature." That is as deep and convincing as it gets. The reader is told that "A proof of the Hodge Conjecture would establish a fundamental link among the three disciplines of algebraic geometry, analysis, and topology," "A solution to [the Birch and Swinnerton-Dyer Conjecture] will add to our overall understanding of the prime numbers," "A solution of the Navier-Stokes equations would almost certainly lead to advances in nautical and aeronautical engineering." A proof of the Riemann Hypothesis, Devlin writes, "might well lead to a major breakthrough in factoring techniques…with Internet security…hanging in the balance." I suppose it is a sign of the times that internet commerce is being used to validate interest in the Riemann Hypothesis.

The last two of Devlin's goals, explaining the genesis of each problem and developing its background, can be grouped together. These tasks are the heart of the book, and Devlin scores some successes amid the expected failures. The Hodge Conjecture, for example, completely defies elementary discussion. Devlin does not impart a good sense of this problem, and he admits it. But it is the concept of the book that is at fault, not the effort. The Birch and Swinnerton-Dyer Conjecture might also seem to be out of the reach of a popular book, but in this case I think that Devlin's discussion is surprisingly effective. The chapter on Yang-Mills Theory and the Mass Gap Hypothesis is essentially devoid of mathematics, but it is fun, interesting, and a painless summary of a good deal of physics. An introduction to topology makes up most of the chapter that is devoted to the Poincaré Conjecture. Half the chapter on the Navier-Stokes equation is given over to a crash course on calculus and partial derivatives. Maybe it is the case of a glass-half-full author meeting a glass-half-empty reviewer, but I am not convinced that it was wise to target this book at an audience with such minimal background.

I come now to the two problems that I think are best suited for Devlin's concept. The well-known *P versus NP* problem was actually a prize problem long before the CMI put up its one million dollars. In January 1974 Donald Knuth offered one live turkey to the first person who proves that $P = NP$. This problem is quite different from the others. Few undergraduate texts in mathematics focus on the other six millennium problems, but P versus NP has become a mainstay of the computer science literature. Most books about algorithms, complexity theory, or theoretical computer science introduce the classes P, NP, and NP-complete (as well as others such as *co-NP*). The Traveling Salesman Problem (TSP) is almost always chosen as an example. By now most computer scientists anticipate that many of their readers will already be familiar with TSP. As a rule they supplement it with a few less hackneyed examples. Shasha and Lazere [10] find another way to avoid the commonplace by interviewing Leonid Levin, overlooked by Devlin, and Stephen Cook. (As an aside, let me note that when scientists write in a popular vein, they often consult colleagues about facts, but they rarely *interview* fellow scientists. The books of Yandell [11] and Sabbagh [9], like that of Shasha and Lazere, demonstrate how effective the technique can be.)

Devlin's treatment of *P versus NP* is mundane but effective. He refers briefly to another *NP* problem but otherwise sticks closely to TSP. Generally speaking, his discussion is a scaled-down version of the treatment that he gave in *The New Golden Age* [2]. The *Time-complexity* table that appears in both books is but one illustration of how closely Devlin sometimes follows his earlier writings. A jaded reviewer who is apt to regard the new discussion as merely a superfluous rehashing must concede that many of Devlin's readers will be learning these ideas for the first time. Those readers will understand the discussion in Devlin's book and come away with a good idea of what this millennium problem is about. It is therefore a successful chapter.

We reach the Riemann Hypothesis (RH) at last. Because it is a great problem that can be accessible when approached in a thoughtful way, it should be ideal for Devlin's type of book. Like Fermat's Last Theorem, RH originates with a fascinating story that encourages speculation. Many interesting, easily stated facts about the ζ function are known. Several curious, seemingly unrelated conjectures are equivalent to RH. Many of the greatest mathematical characters of the twentieth century were drawn to it. There are anecdotes, false proofs by famous mathematicians, mammoth computer calculations, unimaginably large counterexamples to related conjectures—in short, a treasure trove from which any expositor can profitably plunder if he is so inclined. On the evidence, Devlin was only lukewarm to the idea.

As mentioned earlier, the chapter on RH, Devlin's longest, comes to 44 pages. That includes a lot of standard padding such as Euclid's proof of the infinitude of primes. Devlin makes the connection between the zeta function and the sequence of primes via the Euler Product Formula, but after that everything becomes needlessly imprecise. The Prime Number Theorem is discussed, there is a lot of *vague* talk about its relationship to the equation $\zeta(s) = 0$, but neither the fact that $\zeta(1 + it) \neq 0$ nor its import is mentioned. Even the *critical strip* does not appear. A plot of $z = \sin(xy)$ finds its way into the chapter, but $z = |\zeta(\sigma + it)|$ is not graphed. Although it will not bother the typical reader, Devlin's speculation about Riemann's insight (pp. 50–51) is at odds with Edwards's exposition of the Riemann-Siegel Formula [4, pp. 136–170]. In the final analysis, much better treatments of RH abound. Indeed, Devlin wrote one of them [2, pp. 193–221].

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The job of the popularizer is admittedly difficult. These authors are constantly faced with optimization problems that are not present in more advanced monographs. I confess that I am often baffled by the solutions that they find. For example, Devlin records the symmetric form of the analytic continuation formula for $\zeta(s)$ using an unspecified expression, $\gamma(s)$, for $\Gamma(s/2)$ or $\Pi(s/2 - 1)$. So far as I can see, little is gained by using nonstandard notation—the symmetry is a bit more transparent—but there is a real loss: the reader who knows the gamma function but who is learning the zeta function from Devlin will be deprived of a beautiful formula. Twice Devlin tells us that RH is the most important unsolved problem in mathematics. There is something to be gained by replacing tangled, indecisive discourse with simple, clear-cut assertions. The loss is that such pronouncements invite questions that are not answered. Why should a topologist consider RH more important than the Poincaré Conjecture? The $3x + 1$

problem, to cite but one example, does not *appear* to have the *gravitas* of RH, but until we get to the bottom of it and any number of other unsolved problems, is it not premature to bestow highest honors?

Like journalists, mathematical popularizers are well served by the precept "the truth, nothing but the truth, but never the whole truth." Devlin lives by the third part of that saying but often runs afoul of the second. When an author does not bother to check his facts, no matter how unimportant, he puts his credibility at risk. It is not true that the Riemann Hypothesis "is the only problem that remains unsolved from Hilbert's list" (p. 4). Not all the seventy-two savants memorialized on friezes of the Eiffel Tower are "nineteenth-century French scientists" (p. 131). Devlin writes that "the term 'imaginary' for the square root of a negative quantity seems to have been first used by Euler" (p. 37). He is confusing the term "imaginary" with the notation *i*. Descartes, Wallis, and Leibniz all used "imaginary" prior to Euler. A footnote on page 54 refers to "... the (false) assumption that there is no largest prime." Finally, it is foolish and inappropriate to suggest that mathematicians are "the seekers of the only 100% certain, eternal truth there is." Surely there will be curious scientists from other fields who will pick up this book. Do we need to pick fights with them?

What I find most lacking is a real sense of what these problems are all about. What drives our passion for these and similar problems?

Devlin is a professional author, and anything he writes will reflect that. However, there is no way to disguise a suspect idea. Several editors, he tells us, approached him with the concept of this book. It shows. Writing that does not burst forth from an author is bound to be halfhearted. What I find most lacking is a real sense of what these problems are all about. What drives our passion for these and similar problems? At the press conference that accompanied the prize announcements, Landon Clay, founder of the CMI, explained that "It is the desire for truth and the response to the beauty and elegance of mathematics that drives mathematicians." Put aside the incongruity of such thoughts being expressed by a man who has just put up seven million dollars of incentives. More to the point, his list of driving forces is seriously incomplete. To get the tacky subject of financial gain out of the way, tangible benefits *are* expected to ensue, one way or another, from the cracking of a tough nut. I am reminded that many years ago when Quillen solved Serre's Problem, the assistant professor who taught my algebra class did not speak of truth, beauty, or elegance; he lamented that a perfectly good problem had been squandered needlessly on a mathematician who already had tenure.

There are other powerful driving forces that Clay and Devlin are too discreet to mention. As an undergraduate I attended a lecture that touched upon the solution of a famous problem that dominated its field for half a century. A twenty-year-old can be startled to learn that ten years of a mathematician's life might be expended on the solution of one problem. How can anyone persevere so long without admitting defeat? When a fellow student phrased that thought as a question, my professor, himself a leader in the field, laughed and said "You have to know Professor ___ . He thinks he can solve anything." It was a funny answer that only momentarily concealed his sincere admiration for the confidence that can be so essential for success. Ego, competition, one-upmanship—not noble forces, perhaps, but mathematics is the richer for them. In a recent book devoted entirely to RH, Karl Sabbagh elicits unusually candid remarks from many of the top mathematicians in the chase [9]. Several mathematicians confront the controversial practice of hoarding partial results. One admits, "I'm afraid I see all of mathematics as a competition. If someone has a theorem, I always want to prove a better one." Another expresses relief that a highly publicized proof of RH fell apart, sending the unfortunate aspirant back "to oblivion where he came from."

All of this leads one to conclude that Devlin is being a tad genteel when he gives high-minded reasons or even practical reasons that attempt to explain why mathematicians are drawn to unsolved problems. In the preliminary chapter, appropriately titled "The Gauntlet Is Thrown", he comes closer to the truth when he writes "Ultimately, mathematicians pursue these problems for the same reason the famous British mountaineer George Mallory gave in answer to the newspaper reporter's question, 'Why do you want to climb Mount Everest?': 'Because it is there'." Although the words ascribed to Mallory may have been composed by an unnamed *New York Times* reporter as a response to a putative query, alpinist Robert

Jasper *was* asked why he pioneered a new route up the Matterhorn. He cited "the challenge of the line, which I've always regarded as an end in itself." Indeed! As soon as climbers have conquered a peak they strive to find new, more taxing routes to the top. And when those routes have been traversed, they relax the hypotheses and try to be the first to summit in winter. And so on. It sounds just like mathematics. To paraphrase a credit card commercial: CMI Prize Problems—$1,000,000; succeeding where everybody else has failed—priceless.

As I write this review, purported proofs of both the Riemann Hypothesis and the Poincaré Conjecture are circulating. However those claims turn out, we are sure to reap a fine harvest of books and papers inspired by the Millennium Prize Problems. We may all look forward to the day when a fellow mathematician brings news from the summit and echoes the first words of Edmund Hillary on his descent: "We've knocked the bastard off!"

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