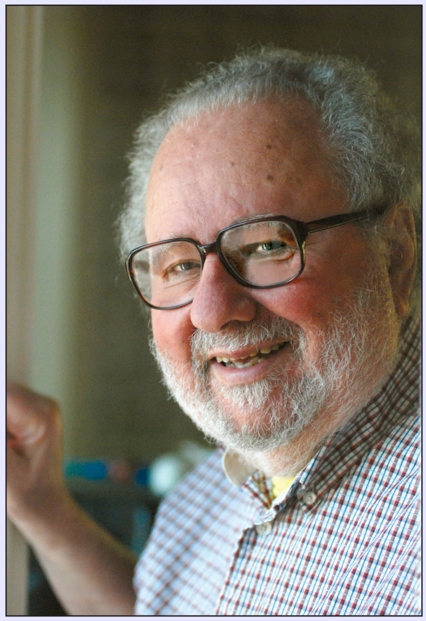
Interview with Martin Davis

Martin Davis is one of the world's outstanding logicians. He was born in 1928 in New York City, where he attended City College and was influenced by Emil L. Post. Early on, Davis came under the spell of Hilbert's Tenth Problem: Does there exist an algorithm that can, given an arbitrary Diophantine equation, decide whether that equation is solvable? Davis's Ph.D. dissertation, written at Princeton University under the direction of Alonzo Church, contained a conjecture that, if true, would imply that Hilbert's Tenth Problem is unsolvable. In rough terms, the conjecture said that any computer can be simulated by a Diophantine equation. The implications of this conjecture struck many as unbelievable, and it was greeted with a good deal of skepticism; for example, the conjecture implies that the primes are the positive part of the range of a Diophantine polynomial. Work during the 1950s and 1960s by Davis, Hilary Putnam, and Julia Robinson made a good deal of headway towards proving the conjecture. The final piece of the puzzle came with work of Yuri Matiyasevich, in 1970. The resulting theorem is usually called either DPRM or MRDP (Matiyasevich favors the former and Davis the latter). The unsolvability of Hilbert's Tenth Problem follows immediately.



Martin Davis

Davis became one of the earliest computer programmers when he began programming on the ORDVAC computer at the University of Illinois in the early 1950s. His book *Computability and Unsolvability* [3] first appeared in 1958 and has become a classic in theoretical computer science. After a peripatetic early career that included stints at Bell Labs in the era of Claude Shannon and at the RAND Corporation, Davis settled at New York University, where he spent thirty years on the faculty and helped to found the computer science department. He retired from NYU in 1996 and moved to Berkeley, California, where he now resides with his wife Virginia, who is a textile artist. Davis has a strong interest in history, and in 2000 he published a popular book *The Universal Computer* [4], which follows a strand in the history of computing from Leibniz to Turing (the book also appeared under the name *Engines of Logic*; it was reviewed by Brian Blank in the May 2001 issue of the *Notices*).

What follows is the edited text of an interview with Martin Davis, conducted in September 2007 by *Notices* senior writer and deputy editor Allyn Jackson.

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Early Years

Notices: *I'd like to start at the beginning of your life. Could you tell me about your family? Were you an only child?*

Davis: I had a younger brother who died in childhood. He was 8 and I was 13.

Notices: *That must have been very hard on your family.*

Davis: It was devastating. My parents grew up in Poland; they were Polish Jews. They knew one another casually in Lodz but met again in New York and married. We were hit very hard by the Great Depression and were really quite poor. For a while we were on what was then called "home relief" and later called welfare. I went to school in the New York public school system, benefiting from the Bronx High School of Science. Later I went to City College where the tuition was free. I was not an athletic boy at all. I was a bookish boy. And I got beat up a lot by more athletic boys! I got interested in science quite early. I wanted to be a paleontologist, then I wanted to be a physicist, and finally I fell in love with mathematics.

Notices: *What were your early influences as a child or teenager in pushing you toward math and science? Was it teachers, or books?*

Davis: It was certainly more books. I read Bell's *Queen of the Sciences*, and was delighted by another book that, as a mature mathematician I thought rather awful, *Mathematics and the Imagination*, by Kasner and Newman. At high school I met a lot of boys who had similar interests, and we bounced off one another. Then at City College, there were two people who had a big influence on me. One was Emil L. Post, who was a great logician and a very direct influence on the direction of my work, and Bennington Gill, who was really a very inspiring teacher, even though his mathematical productivity pretty much ended with his dissertation.

Notices: *Can we go back to the Bronx High School of Science? What was it like to be a student there at that time?*

Davis: It was during the Second World War, and that dominated the atmosphere in many ways. The principal went to Washington and talked to various people in the government and came back with the idea of what were called "pre-induction courses", to modify the curriculum in the direction of subjects that would be useful when we were taken into the army, which, it was assumed, we would be. So for example, light and sound were deleted as topics from the physics curriculum, and what was thrown in to replace them was a lot more work on alternating current circuits and things of that sort. We were told that these were very significant things and that a great deal of attention would be paid once we were in the army due to the fact that we had had these courses—which of course was a lot of nonsense.

The faculty adviser for the Mathematics Club thought that we should do things that were more useful, so suddenly we spent the semester talking about navigation and spherical trigonometry.

Notices: *Did you find navigation interesting?*

Davis: I found it boring! I remember I gave a talk about dead reckoning, which was the technique that Columbus used. I was also president of the Astronomy Club for a while.

Notices: *You were clearly very motivated toward education. How much education did your parents have?*

Davis: They had no formal education. They went to night school courses for immigrants to learn English. My father was really a remarkable man. His life would have been very different under other circumstances. He worked hard to support the family, but he was a very gifted artist. Recently we donated thirteen of his paintings to the YIVO Institute for Jewish Research in New York; they formed a cycle inspired by events in Europe during the Second World War, especially by what was happening to the Jews.

Notices: *So your parents encouraged you to get an education.*

Davis: Oh yes, that was the given in Jewish families in the Bronx. They imagined I would become a doctor or a lawyer, and they were at a loss to know what to make of the direction my interests were taking. They simply didn't know what it was and were worried I would starve in a garret! And of course an academic career for a Jew in America was a very difficult thing before the war. That changed dramatically after the war.

Notices: You mentioned the influence on you of Emil Post. Can you tell me about him and the changes that were going on in logic at that time, with the work of Church and Turing?

Davis: Goodness, that's a lot! You know, I edited Post's collected works [6] and wrote an introductory article on his life and work. I gave a talk about him here at the Logic Colloquium just a couple of weeks ago. We could spend the whole interview talking about that! Briefly, Post was a bit older than Church and certainly older than Turing, and so he came into these ideas well before them, in the early 1920s. From one point of view, he really discovered all the main results well before them. From another point of view, he never got his formulations to the point where they would have been acceptable for publication. Post's own comment on this I find very poignant. He was always writing postcards to people and sent one to Gödel shortly after they met. He apologized for what he thought was his overexuberant behavior with Gödel and then said about his own earlier contributions, "The best I can say is that in 1921 I would have proved Gödel's theorem if I had been Gödel."

Notices: *So he knew of Gödel's result but wasn't able to carry it through himself.*

Davis: The problem was that, in effect, Post bumped up against what later was called Church's Thesis, or the Church-Turing Thesis, and didn't see how to justify it properly. His formulation was fundamentally based on the assumption of the adequacy of Russell and Whitehead's *Principia Mathematica*, for anything that could be done mathematically, whereas Gödel's theorem itself partly contradicted that. So Post decided that what was needed was something he called "psychological fidelity", which would somehow encompass any processes that the human mind could develop. And then he just went totally off the track, in my opinion, in how to develop such a thing. In effect, what he was looking for is what Turing did to analyze the notion of a computation, but he didn't find it. Post had other difficulties.

One was that he suffered from bipolar disease. He was a manic-depressive and had episodes that were totally debilitating. Also, he faced a climate of mathematical opinion that was somewhat hostile to the whole enterprise of mathematical logic. For example, some of the important results in his dissertation were not accepted for publication until they finally appeared a decade later, as an *Annals* study.

I had done calculus somewhat on my own before coming to City College, so I started with elective advanced courses right at the beginning. In my sophomore year, there was a course that Post gave in real variable theory, which was quite famous among City College students. But because of the war it wasn't going to be given. So five of us went to him and asked whether he would be willing to give it as what was called an honors course, in effect a reading course. He agreed, and we met once a week for a two-hour period. Every one of the five of us would tell you that this was an emotionally wringing experience!

Notices: *Why?*

Davis: The pedagogical method was the following. We had a textbook, which was awful; it was just full of mistakes. So Post had prepared about forty pages of material correcting and supplementing the text. Each week we were given an assignment for the next week in which there was a certain amount of text with accompanying notes that we had to read, absorb, and learn. Then what would happen during the two-hour period is that he would randomly call on us to go to the blackboard, with no notes, and expound parts of the text. This was very hard! But it was great training.

Notices: *Did some of these five other than you go on in mathematics?*

Davis: All of us.

Notices: *So maybe he did something right!*

Davis: Well, at that time, the mathematical talent at City College was just incredible. Everywhere you go, you find mathematicians who were graduates of City College.

Notices: *Who do you remember in particular?*

Davis: In the group in that real variable class, there was Murray Rosenblatt, who is a probabilist at La Jolla. Also Gerry Freilich, who wrote a fine dissertation and was on the faculty at Queens College; Julie Dwork, who was at Burlington; Seymour Ginsburg, who became a computer scientist and an expert on context-free languages. People who arrived a little later included Donald Newman, Jack Schwartz, Leon Ehrenpreis, and Bob Aumann, 2005 Nobel Laureate in economics. I should also mention John Stachel, who is now an Einstein scholar at Boston University. John was a broadly educated physics major with strong mathematical interests. I learned a lot from him about various things. His father was an important member of the American Communist Party, which in those years was a tricky business. During the McCarthy years, which of course came later, John was quite isolated. One of our fellow mathematics students, Herman Zabronsky, wrote a dissertation at Penn, and later got a job at a national lab, Oak Ridge or Los Alamos. Around Christmas week he came back to New York, and a bunch of us were going to get together somewhere, and he stipulated that John Stachel shouldn't be there. He said, "If John Stachel is there I'm going to flunk the lie detector test"—to give you an idea of the atmosphere!

At City College, there was a required course for science majors in logic and scientific method, given in the Philosophy Department. I took that course in my freshman year, and it turned out to be a beginning course in symbolic mathematical logic. So I learned the basics of propositional calculus and quantification theory as a freshman in that philosophy course. There wasn't any meta-theory, but still it meant I knew the basic material very early. I read in Bell's *Development of Mathematics* that Post in his dissertation had developed a many-valued logic. So I knew that he worked in the area of logic. His real variable theory course certainly touched on topics close to logic. Also, I had heard about Gödel, and there was a copy in the City College library—undoubtedly because Post put it there—of the mimeographed notes that Kleene and Rosser had taken of Gödel's 1934 lectures at the Institute for Advanced Study. The notes were published in reasonable form only much later, first in my anthology *The Undecidable* [5], and then in Gödel's *Collected Works*. So I took it out of the library and tried to make sense of it quite early, and I associated all that with Post—I don't exactly know why or how, but somewhere along the line, I started talking with Post regularly. He gave me a batch of his reprints. John Stachel and I asked Post if we could do a reading course with him in mathematical logic. That was in my junior year. We didn't get very far in the course, because Post had one of his breakdowns after a few weeks. He had just made an important discovery regarding incomparable degrees of unsolvability, and the excitement was too much for him and pitched him over into the manic phase. We didn't see him again for some months.

Notices: *You are mentioning Gödel and Post, who had mental problems. And there are others,* *for example, Cantor. Do you think there is any association between math and mental illness?*

Davis: Probably. Particularly logicians seem to be prone to it! In fact, I had a joke with John Stachel. Post had lost an arm in a childhood accident. Hans Reichenbach, who was sort of a logician and a philosopher, came to City College for a semester to give some courses, and he was essentially stone deaf. Church had vision problems—he had bad cataracts, which in those days was much more of a problem than it is today. So the joke was that, if I'm going to be a logician, maybe I should give up a finger now, instead of something worse!

By the time I graduated City College, I knew I wanted to be a logician. I had written a term paper for an advanced logic course in the Philosophy Department, which in a way was a first draft of part of what was later my dissertation. I went to Princeton to work with Church, but I was really much more influenced by Post than by Church.

Culture Clash in Princeton

Notices: *How was it for you as a graduate student at Princeton?*

Davis: I was pretty unhappy there, all in all. Let's say there was a very heavy culture clash. I grew up in a working class Jewish family in the Bronx. At the City College cafeteria we had our "mathematician's table" where we argued and learned from each other. In that noisy atmosphere we had to speak loudly just to be heard. Besides in our culture, a loud voice simply indicated excitement. I spoke at the Logic Seminar in Princeton shortly after I arrived. Leon Henkin's comment on my talk was that it was too loud. Let me say without going into great detail that I certainly felt a culture clash. I finished my degree in two years and was very glad to leave when I was done.

Notices: *Leon Henkin was a student there at the same time?*

Davis: Earlier. He was a postdoc my first year.

Notices: *What kind of person was Alonzo Church?*

Davis: Alonzo Church was a shy, retiring man, extremely pedantic, very compulsive in his habits. A famous, quite true story about him is the thorough way he would clean the blackboards every day before his lecture. One day some of us students cleaned the board before he came in. This had utterly no effect on his behavior; he cleaned it in exactly the same way. I have often thought that, when he was younger, when Kleene and Rosser were in his classes and he was developing lambda calculus, he must have had a much more spirited lecture style. His lecture style was slow, tediously slow.

Notices: *Who else was there among your student colleagues when you were at Princeton?*

Davis: My good friend from New York and roommate later, Melvin Hausner, who was a student of Bochner's there and was later a colleague at NYU. Washnitzer, who later was on the faculty at Princeton, was an older student, and to some extent acted as a mentor. Leo Goodman, who is in the National Academy, is a sociologist and a statistician who was part of my class. And of course John Nash was a fellow student when I was there. We didn't get along at all. If you look at Sylvia Nasar's biography of Nash [7], I make a brief entrance.

Notices: *I read the book, but I don't remember what you had to say about Nash.*

Davis: She quotes me as saying that Nash once asked me whether I grew up in a slum. Serge Lang was also a fellow student in Princeton.

Notices: *What was he like in those days? Was he as intense and committed as he was later on?*

Davis: I knew him as being very eager to have Emil Artin take him on as a student and very worried that that might not happen. I always had friendly relations with him.

Notices: *Was it when you were a graduate student that you got interested in Hilbert's Tenth Problem? Or was that earlier?*

Davis: Well, it was Post's fault. An important paper of Post's that was published in 1944 mentioned Hilbert's Tenth Problem and said that it begged for an unsolvability proof. Most of my mathematical career I have had an ambivalent relationship with the problem. On the one hand, it fascinated me and pulled me in, seduced me, and on the other hand I felt that it was to a very large extent a number theory problem, and I was no number theorist. When I was a graduate student I kept thinking I should stay away from it, because I needed to write a dissertation! I had one topic that I knew was going to be easy because it was completely untouched territory—what was later called the hyperarithmetic hierarchy—and that topic was one part of my dissertation. But the dissertation also included my first contribution to Hilbert's Tenth Problem—what later was called the Davis normal form.

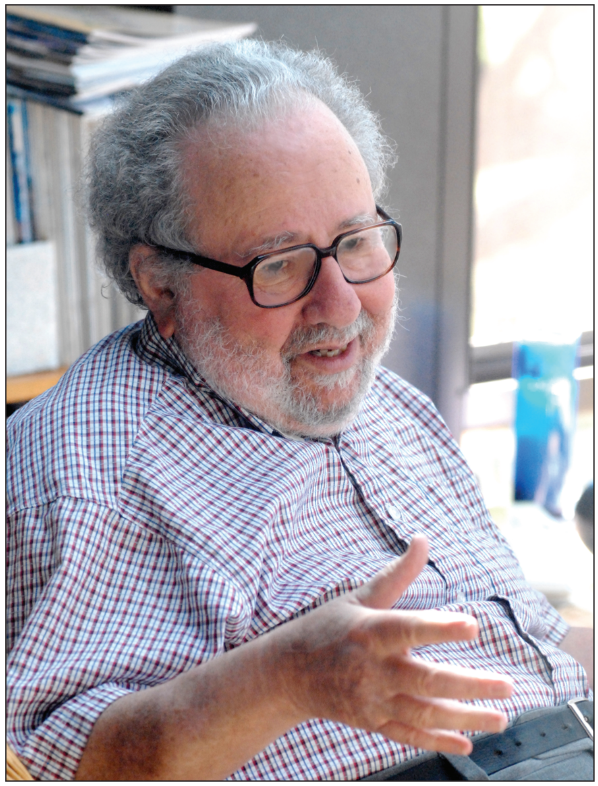
Notices: *Post said that it needed an unsolvability proof.*

Davis: It wasn't hard to see.

Notices: *Why was that?*

Davis: Well, people have been working on Diophantine equations since Diophantus, and what is there to show for it? A lot of special cases. The idea that there should be a general algorithm that would tell you whether any Diophantine equation had a solution or not? That's super-utopian. Once it becomes possible to consider that problems of that kind could be solved negatively, Hilbert's Tenth Problem was a natural candidate. When Post said it begged for an unsolvability proof, he was exactly right, but that didn't mean one knew how to construct such a proof.

What I proposed in my dissertation, and went a small way towards proving, was a much stronger statement from which the unsolvability would follow: That anything that can be done by an algorithmic process could also be defined by a specific Diophantine equation. Yuri Matiyasevich later referred to this as "Davis's daring hypothesis". I had a reason for thinking this might be true, even though the general opinion for years was that it wasn't, until it was proved finally by Yuri giving the last step. The experts all thought it was false. It has to do with recursively enumerable sets, which are sets that can be listed by an algorithm. The basic result from which unsolvability results follow is that there are recursively enumerable sets that are not computable—for which there is no deciding algorithm. My conjecture was that every recursively enumerable set has a Diophantine definition. The class of recursively enumerable sets has the key property that while it is closed under union and intersection, it is not closed under complementation: There is a recursively enumerable set whose complement is not recursively enumerable. I could prove in a nonconstructive way, and it was easy, that the Diophantine sets, the sets definable by Diophantine equations, have the same properties: They are closed under union and intersection but not under complementation. That made me think that they might be the very same class—which turned out to be true.



Notices: *But there was skepticism that this was true. What was the reason for the skepticism?*

Davis: It seemed like a stretch that something as simple as polynomial equations could capture the full gamut of things that are algorithmic. Ten years after my dissertation there was a theorem of me and Hilary Putnam and Julia Robinson [1] that proved the analogue of that conjecture for exponential Diophantine equations—that is, we proved the conjecture under the assumption that you allow the equations to have variable exponents. It followed from earlier work of Julia Robinson that the full conjecture would be a consequence of what Hilary and I called "Julia Robinson's Hypothesis", that there is a polynomial Diophantine equation whose solutions grow exponentially as a function. Yuri finally proved this hypothesis ten years later by giving an explicit example. Georg Kreisel reviewed our paper for *Mathematical Reviews*. In his review, he didn't think it was worthwhile noting that we had shown that my conjecture, and consequently the unsolvability of Hilbert's Tenth Problem, would follow from the Julia Robinson Hypothesis, but he did express his opinion that these results were likely not to have any connection with Hilbert's Tenth Problem. You can imagine why I like quoting that!

Notices: *Why did your conjecture seem hard to believe?*

Davis: I can tell you what Kreisel's reason was. One of the things that follows from my conjecture-become-a-theorem is that not only is every algorithmic process definable by a Diophantine equation, but it's also definable by a Diophantine equation with a bounded number of variables. In fact, later work of Julia and Yuri brought the number of variables down to 9. And such a bound was thought to be totally implausible.

Notices: *It does seem implausible, doesn't it? It's really surprising.*

Davis: Yes! The point is that people talked about polynomial equations, but what they were thinking of was equations of degree maybe 3 or 4, and with maybe four or five unknowns. The idea of equations of arbitrary degree and arbitrary number of unknowns—people had no intuition or experience with those. It was too hard.

Hilary Putnam had a cute trick that turns any Diophantine set into the positive part of the range of a polynomial. When I was talking to number theorists before Yuri's work, I would say, "Do you think the prime numbers could be the positive part of the range of a polynomial?" And often I would get the following answer: "No, that couldn't be. Give me a half hour and I'll prove it."

Notices: *A half hour! That's all they required?*

Davis: Yeah!

Notices: *But it is counterintuitive, isn't it?*

Davis: Yes, sure it is!

Notices: *What gives Diophantine equations this power? What is the richness there?*

Davis: There are two pieces of richness. There is the richness that reaches to exponentiation, that the set of triples a, b, and c, such that $a = b^c$, has a Diophantine definition. Logically speaking, that should have been the first thing proved, but it was the last. Way back in the 1950s, Julia started working on this and showed it would follow from what Hilary Putnam and I later called the Julia Robinson Hypothesis, which as I said wasn't proved until Yuri did it in 1970. The second piece of richness is, once you have exponentiation, going all the way. In the case of that first result, the richness comes from the power of second-degree Diophantine equations—Pell equations, or Fibonacci numbers. They have the power to move up to exponentiation.

The proof in the paper the three of us wrote [1] goes back to the Chinese remainder theorem. It was Gödel who first used the Chinese remainder theorem as a device to code finite sequences. That's what I used in my dissertation result, but it didn't go all the way. Technically, a bounded universal quantifier stood in the way of the definition that one wanted to be entirely existential. Using the Chinese remainder theorem to code the effect of the bounded universal quantifier needed some clever tricks, and that's what the three of us developed. When Hilary and I first tried to prove this, our proof had the major flaw that we needed to assume that there are arbitrarily long progressions consisting entirely of prime numbers—something that was only proved two years ago by Ben Green and Terence Tao. If we had had that theorem, our proof would have been a perfectly good proof. But at the time it was Julia who showed how to do without that then unproved assumption.

Notices: *So ultimately Hilbert's Tenth Problem was resolved by work of you, Hilary Putnam, Julia Robinson, and Yuri Matiyasevich, and your conjecture became the DPRM theorem. So often in mathematics there are priority disputes. But that did not happen with you four.*

Davis: It's just the opposite. We all like and respect one another. I guess we are nice people! Some people want to call the result Matiyasevich's Theorem, and Yuri insists no, it's DPRM. Other people say MRDP. A story I like to tell is about the later collaboration of Julia and Yuri, in which the number of unknowns was knocked down to nine. As I said, once you had the main result, it was clear that there was a bound. If you just took the crude proof, you would get an estimate of forty or so unknowns. Julia and Yuri took on the task of trying to get a better bound. They published a very nice paper in which they got it down to thirteen. Then Yuri, basically using the same methods that they had developed but refining them, managed to get it down from thirteen to nine and proposed to Julia that they publish a joint paper. She said, "No, I didn't have anything to do with getting it down to nine, that's your result, you publish it." He said, "No, it uses our methods, I'm not going to publish it unless you will be a joint author." What finally happened is James Jones got permission from Yuri to include the proof in a paper that he wrote, and that's how the proof was finally published [2].

Notices: *Can you tell me your memories of Julia Robinson, what she was like as a person?*

Davis: Very nice, very straightforward. Broad in her interests, mathematical and otherwise. And great power—there is no question in my mind that she was a much more powerful mathematician than I. We worked together on a problem on which we didn't get anywhere. We were trying to prove the unsolvability of the decision problem for word equations. It turned out that we wouldn't have been able to do that because the problem is solvable. Makanin solved it positively. That had a curious relationship to Hilbert's Tenth Problem, because some of the Russians were interested in proving it unsolvable because its unsolvability would have been a way to get the unsolvability of Hilbert's Tenth Problem, without proving my conjecture, which they thought was likely false. But in fact, it turned out to be on the other side of the line.

Notices: *What made you think that it was unsolvable?*

Davis: I don't know that we thought it was unsolvable. We thought it might be unsolvable. When we were working on the problem, Julia and I would stand at a blackboard on the campus here in Berkeley, and you could just feel the power. You could feel the power in her papers too, particularly her dissertation on the definition of the integers in the theory of the rational numbers—it's really a powerful piece of number theory. It was also uncharted territory. It was the kind of number theory nobody was doing.

Notices: *Do you have a sense that any outstanding mathematical problems out there nowadays might be unsolvable?*

Davis: In the sense of nonexistence of an algorithm? Not really. The easy stuff has all been scooped up, I would say. One thing that is clear from many cases is that the boundary is tricky, as is often the case with sharp mathematical boundaries. Particularly in the case of Hilbert's *Entscheidungsproblem*, the boundary between cases that are solvable and the ones that are unsolvable boils down to the question, Are there two quantifiers, or three? I don't think anyone would have guessed to start with that three is unsolvable and two is solvable, but that's how it turned out.

Programming the ORDVAC

Notices: *After Princeton, in 1950, you got a job at Illinois, and that was when you started programming. You were one of the world's first programmers, weren't you?*

Davis: Well, let's say an early one, anyway. I had a research instructorship at Urbana-Champaign, which was basically a kind of postdoc. I taught a logic course, and the second semester was about computability. I wrote a book that was published years later, in 1958, called *Computability and Unsolvability*, and the second semester was sort of a first draft of that book. Part of what I was doing was writing Turing machine programs on the blackboard. Edward Moore, later known for his work on sequential machines, was an auditor of the course. He had just finished a Ph.D. and had joined the computer project at Urbana-Champaign. They were building an early computer, called the ORDVAC, one of a family of early computers called "johniacs" after John von Neumann. Moore came up after one of the classes to tell me that a program I had written on the board could be improved and showed me how to do it better. He said, "You really ought to come across the street, we've got one of those there!"

Notices: *Did you know of the existence of the computer there?*

Davis: No, I was not aware of it. I should have followed it up, but I didn't. What happened instead was that, as a result of the Korean War, I was in danger of being drafted. A military project started up, the Control Systems Lab, and I was given the opportunity to join it, which seemed like a good way to avoid being in the army. What they set me doing was writing programs for the ORDVAC. In fact, I wrote a program that was supposed to navigate 100 airplanes in real time! Of course it was a preposterous thing with the technology available. But I did produce the program. My course in computer science consisted of a five-minute lecture by Abe Taub, who said, "This is how you program." I was given the von Neumann-Goldstine reports, which had a lot of sample programs.

Notices: *What was it like to program on this computer? What did you have to do?*

Davis: What I had to do was to write code on a piece of paper with a pencil. A secretary would type it on a teletype machine, which would produce a piece of punched paper tape that would have the code on it, and that was what was fed into the computer.

The memory consisted of forty cathode ray tubes, little TV screens. On each one, there would be a grid of 32 by 32 bits. A 0 was two dots, and a 1 was one dot. So the data was stored as electrostatic charge on the surface of the tube. This had the serious defect that charge doesn't stay put, it decays. So they had what was called "read-around", a process by which the memory was constantly being read and rewritten. The programmers had to be aware of this, because if they wrote a program in such a way that the read-around didn't get a chance to correct the memory, the data would become unreliable. We had to avoid tight inductive loops in our code.

Notices: *Did working with computers at this time affect your thinking about the purely mathematical problems you were working on?*

Davis: It affected the way I thought about computability. It certainly affected my book *Computability and Unsolvability* [4]. A number of people who became computer scientists at a time when there was no computer science told me that they learned programming from that book, even though it's not a practical book at all, but a theoretical one.

The Mind and the Brain

Notices: *In 1951 you went to a lecture by Gödel in Providence. Can you tell me about this lecture, and how it influenced you?*

Davis: You are not getting my pure memory of the lecture, because I have since read the text several times, after it was published in his collected works. The lecture was altogether remarkable. It was the Gibbs Lecture. Gibbs of course was an applied mathematician par excellence, and here was Gödel, coming to lecture the mathematicians on philosophy! He said that, if you look at his incompleteness theorem and what it implies about mathematics and about the human mind, you are faced with a pair of alternatives. One is that thinking is entirely mechanistic, and it's all done by the brain, in which case you have to think that the brain is just a Turing machine. Then our mathematical doings would be subject to the incompleteness theorem, and there would be number-theoretic truths that we will never be able to prove. The other alternative is that the human mind surpasses any mechanism, and that's of course what Gödel really believed. He was a Cartesian dualist. He really thought the mind has an existence quite separate from the physical brain. Those were the two alternatives he provided. I came out with my head spinning.

Notices: *Which alternative do you think is true?*

Davis: Oh, the first one. I'm a mechanist.

Notices: *Why is that so clear to you?*

Davis: I think the more research is done about the human brain, particularly about people who suffer brain damage of one kind or another, the more we see that various aspects of what we think of as mind really sit in the brain. I recently read an article in the *New Yorker* by Oliver Sacks, about a man who had total amnesia. He simply could not form a memory of anything happening to him but still was perfectly capable of sitting down at the piano and playing at a high professional level of skill. People in whom the corpus calossum, the connection between the two halves of the brain, has been severed, behave as though there were two separate people inhabiting their skull. Also, there is just the general historical fact that vitalism as a philosophy has been in retreat.

When I was at City College, a biology professor said that he didn't think biology would at bottom end up being physics and chemistry. But now we know about DNA and genes. And if you think the human mind is separate from the brain, what about the mind of a chimpanzee? Is that really qualitatively different? Or is it just that there are some extra little subroutines that enable us to use language?

One of the strange things about this argument about mechanism and mind is that people on one side of the divide don't even seem to be able to understand what it is that people on the other side could be thinking. My good old friend Raymond Smullyan can't understand how a sensible person can believe what I believe! And I find it hard to imagine what he thinks.

***Notices:** There have been books looking into this question in recent years, for example, the book by Roger Penrose [8].*

Davis: I was involved in a polemic with him in print, in the publication *Behavioral and Brain Science*.

***Notices:** I see. Penrose used Gödel's ideas to argue that the mind must be more than the brain.*

Davis: Yes. He went a lot further than Gödel would have been willing to go. Gödel at least had an alternative. Penrose is a remarkable mathematician and physicist, but on this subject he is simply foolish. He won't listen to what we logicians tell him. It's simply not true, as he asserts, that we can see a truth that no machine can see. All we ever see is that, if a particular system is consistent, then that statement is true. But your favorite machine can see the very same implication. The tricky part is knowing which formal systems are consistent. You can find by the wayside formal systems proposed by first-rate logicians that have turned out to be inconsistent, starting with Frege and continuing with Church, Quine, Rosser, and others. So the assumption that we can somehow really tell whether a formal system is consistent is unjustified. Turing wondered about this way back, in his famous article on whether computers can think [11]. The way he put it is that a machine has to be allowed to make mistakes just as human mathematicians make mistakes. That was his way of saying that a formal system might be inconsistent, and he asked for "fair play" for computers!

***Notices:** You spent 1952–1954 at the Institute for Advanced Study in Princeton. Did you have much contact with Gödel during that time?*

Davis: I spoke to him twice. Once by myself, to tell him about something I was working on, in which he showed not the least interest. The second time was with John Shepherdson, an English logician who was at the Institute at that time as well. We had heard a rumor that Gödel had a proof of the independence of the axiom of choice from the axioms of set theory and decided we should make an appointment to ask him about that. I can't remember the details of the meeting, but it was awkward, and we came away without any information.

Martin Davis and his wife Virginia.

One person we were very friendly with at the Institute was Julian Bigelow, the engineer who, one could almost say, built the Institute johniac computer with his own soldering iron. He was a very interesting person. He wanted to move a house from point A to point B in Princeton. Some utility poles with wires were in the way. When he contacted the utility company about temporarily moving the poles so he could move the house, the cost was prohibitive. So what he did was take a hand-saw and horizontally saw the house in half, move the two pieces separately, and then screw them together with great big flat brackets!

***Notices:** Didn't that just ruin the house?*

Davis: Apparently not!

***Notices:** Did you get to know von Neumann?*

Davis: I met von Neumann back in Urbana, and that was the only time I had a conversation with him. I attended the inauguration of the Control Systems Laboratory, and there were many guests, and von Neumann was one of them. I wanted to meet him, but I was much too shy to walk up to him. My wife Virginia was there—we weren't married yet—and took care of the problem. She made me a scotch and soda that was more scotch than soda, and after that von Neumann and I had a wonderful chat about sun spots and ice ages and heaven knows what! But I never talked to him at the Institute.

Computer Science Culture

***Notices:** After the Institute you went to the University of California at Davis. You had various jobs* *for a while and then eventually you ended up at New York University, where the computer science department was just getting started.*

Davis: Well, I came to NYU in 1965, and the Computer Science Department was started in 1969, and I was there to join it at that time.

Notices: *I've heard people say it's unfortunate that computer science departments split off from math departments at this time. You witnessed this happening at NYU. Do you think they should have stayed together?*

Davis: No, it wouldn't have worked. It's really a different culture. There are things about the culture I don't like, but it is different. The way the theoretical and the pragmatic parts of the subject interact is very different from how applied mathematics of the classical sort interacts with pure mathematics. At NYU, we were involved in finding our way as a separate department. Of course we knew very well that the mathematical power of the people at the Courant Institute—people like Nirenberg and Lax and Varadhan and so on—was of a very different caliber than the mostly young people who we had in the computer science department. But still, we had our own culture and needs, and we had to convince them, for example, that it wasn't appropriate that a Ph.D. student in computer science be required to pass an exam in complex variable theory.

Notices: *What are the aspects of the computer science culture that you don't like?*

Davis: I am thinking of theoretical computer science particularly, where the field moves very fast, leaving unsolved problems behind, under the assumption that, because they haven't been able to deal with the problems in a year or two, the problems are intractable. And they go on to another subject. Following the habits in the more applied parts of computer science, the intellectual center is not finished public articles, but rather conferences. A conference program committee—I have been on one of them—gets extended abstracts in which theorems are stated but rarely proved, and somehow judgments are made. But the things that are claimed to be proved are not always in fact proved. And the stakes are high. People's subsequent careers could depend on getting papers accepted in these conferences. That's what I didn't like.

Notices: *The outstanding problem in computer science now seems to be the P versus NP problem. Do you care to speculate on whether or how that will be resolved?*

Davis: I have very unconventional views about that. It is taken for granted in the field that P and NP are different but that it's just much too hard for people to prove it. I think it's 50–50! I wouldn't be in the least astonished to find that P equals NP. I think the heuristic evidence that is given, when you look at it carefully, is just circular. I certainly agree that it's very unlikely that there are really good algorithms for NP-complete problems like satisfiability. But the equating of "good" with polynomial-time computability seems to me to lack evidence. People say "polynomial", but they mean with an exponent no higher than 3. I sort of see it as a reprise of the situation with Hilbert's Tenth Problem, where people didn't have any real imagination about what a polynomial with high degree could do. I was an invited speaker last summer at a meeting in Lisbon devoted to the satisfiability problem. In my talk I said that if I were a young person I would try to find a polynomial-time algorithm for satisfiability, not expecting it to be a particularly good algorithm!

Notices: *Really!*

Davis: Why not? I don't see any compelling reason there shouldn't be one.

Notices: *But people aren't working on it from that point of view. People seem to think that P and NP are different.*

Davis: Well, there is a million-dollar prize!

Notices: *Is the question of whether there is a polynomial-time algorithm the correct way of measuring the difficulty of solving problems?*

Davis: Well, that's really the question. Certainly theoretically the class of things for which there are polynomial-time algorithms has nice closure properties. So it's a mathematically attractive class. But the idea of identifying them with what's computationally feasible is I think the result of looking at an analogy with Church-Turing computability, which is a very successful formalization of the intuitive notion of what is calculable in principle when you don't think about resources. But it's just not a compelling analogy in my opinion.

At the January meeting [New Orleans Joint Mathematics Meetings, January 2007] Margaret Wright gave a talk about linear programming. Linear programming was also thought to be an intractable problem for which there were no polynomial-time algorithms—until the polynomial-time algorithms popped up. First there was the ellipsoid method found by the mathematician Karmarkar, then the interior-point method. Also, one of the things Margaret Wright showed was that, in very specific cases where you're doing serious computation and trying to deal with hundreds of thousands of linear constraints, the old exponential-time algorithm of Dantzig often does better than the polynomial-time algorithms! So I just don't see on what basis people measure feasibility in this way. It's not that I bet that P equals NP. I just can't see any compelling evidence either way.

Steve Cook once pointed out to me that the one separation in the field is between log-space at the bottom end and polynomial-space at the top end. In between there are classes that theoretical computer scientists study: P and NP, and the whole polynomial-time hierarchy, and then PSPACE sits on top of all of that. But the only separation theorem that has been proved is between the very bottom and the very top. All the other layers could collapse, for all anybody knows. But people will publish papers that say, "If such and such is so, then the polynomial-time hierarchy will collapse at level 2" and think that, since that's not going to happen, they have essentially proved their result.

Notices: *Do you think that maybe the problem is just not cast in the correct way, with the right viewpoint?*

Davis: I've thought about that and tried to think what the right viewpoint might be, but I haven't come up with anything! Maybe there is no real notion of feasibility, or maybe there is a notion, and it hasn't been found yet.

A Continuing Mystery: The Continuum Hypothesis

Notices: *Do you think the Continuum Hypothesis will ever be resolved?*

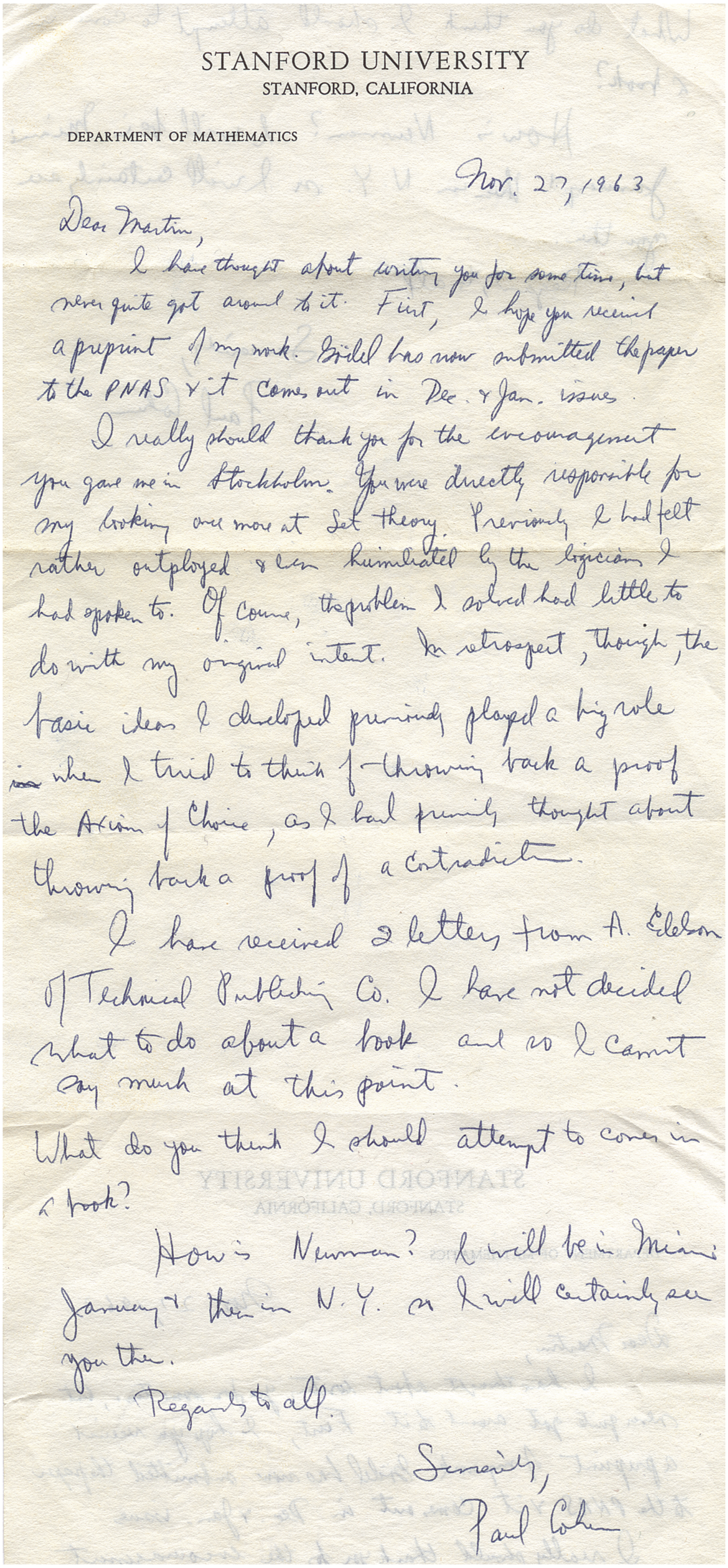
Davis: I think it has a truth value, meaning that it's a coherent statement that is either true or false. Whether the human race will ever be able to resolve it or not, I have no idea. I don't think we can do everything. But I don't think it's ill-posed, in the way Sol Feferman does. He wrote an article "Does mathematics need new axioms?" [9], in which he suggested that the final fate of the continuum problem will be that it's just regarded as incoherent and ill-posed. If you think that the universe of sets is a human creation and that there is no objective truth about it, then the way Sol thinks makes sense. If the world of sets is a human creation in the way the play *Hamlet* is, then the question, "Was Hamlet a virgin?" might not have any answer! But if you think that there is something objective about the universe of sets, then in that universe, regardless of what we are able to do, it will have a definite answer.

Notices: *Do you think that the work of people like Hugh Woodin and John Steel might eventually bring a decision about the Continuum Hypothesis?*

Davis: Yes, I am hopeful. I have to say first of all that I can't claim I really understand in any but the vaguest way what those people are doing. I admire it tremendously, but I find it very hard to follow in detail. But as an outsider, what comes to my mind is this. Before Paul Cohen, people thought of the Zermelo-Fraenkel axioms as pretty much determining the world of sets. They knew about the Skolem-Löwenheim theorem, that there had to be countable models—but it seemed as though those would just be weird and peculiar things. Then Paul Cohen invented the method of forcing, and suddenly the Zermelo-Fraenkel axioms, instead of being like the axioms of, say, Euclidean geometry, became like the axioms of group theory. Suddenly you could make models of the Zermelo-Fraenkel axioms every which way, with whatever properties you want. It had tremendous flexibility, and that was a big obstacle in trying to settle a problem like the continuum problem, because you can force the continuum to be $\aleph_{17}$ or whatever you want. But even before Woodin's work, there was the so-called Martin's Maximum, which is a certain axiom added to the axioms of set theory that in effect prevents forcing from working. When Martin's Maximum is added to the Zermelo Fraenkel axioms, it turns out that the cardinality of the continuum is $\aleph_2$. I wouldn't be a bit surprised if the true value really turns out to be $\aleph_2$. That's certainly what some of Woodin's work suggests, and I can be rash enough to say it, because, as a bystander trying to understand what's going on, my opinion isn't worth much!

One thing you have not asked me about but that I think is very important is the following. We know as a theoretical matter that there are mathematical propositions that, formally speaking, have a very simple form involving solvability of specific Diophantine equations and that require set-theoretic methods for their resolution. Harvey Friedman has found examples of this kind with a combinatorial flavor. To me a really interesting question is: Do any important unsolved problems that are really significant to mathematicians fall into that category? One of the things Gödel himself conjectured in that remarkable Gibbs Lecture was that the Riemann Hypothesis might be of that character. And that wouldn't surprise me in the least.

Notices: *So you are saying the Riemann Hypothesis can be reduced to a question about solvability of a specific Diophantine equation?*


STANFORD UNIVERSITY
STANFORD, CALIFORNIA

DEPARTMENT OF MATHEMATICS

Nov. 27, 1963

Dear Martin,

I have thought about writing you for some time, but never quite got around to it. First, I hope you received a preprint of my work. Gödel has now submitted the paper to the PNAS & it comes out in Dec. & Jan. issues.

I really should thank you for the encouragement you gave me in Stockholm. You were directly responsible for my looking once more at Set theory. Previously I had felt rather outplayed & even humiliated by the logicians I had spoken to. Of course, the problem I solved had little to do with my original intent. In retrospect, though, the basic ideas I developed previously played a big role when I tried to think of throwing back a proof the Axiom of Choice, as I had previously thought about throwing back a proof of a contradiction.

I have received 2 letters from A. Edelson of Technical Publishing Co. I have not decided what to do about a book and so I cannot say much at this point.

What do you think I should attempt to cover in a book?

How is Newman? I will be in Miami January & then in N.Y. so I will certainly see you then.

Regards to all.

Sincerely,

Paul Cohen

Davis: Yes. The technical notion is what logicians call a Π_1^0 proposition. These are statements that a certain decidable property of natural numbers is true of all natural numbers. Decidable, in the sense that there is an algorithm to tell whether a given number has the property or not. By using the MRDP theorem, one easily shows that every Π_1^0 proposition is equivalent to a statement asserting about a particular polynomial equation with integer coefficients that that equation has no natural number solutions. That's entirely equivalent to the first

Text of letter from Paul Cohen to Martin Davis (shown at left), dated November 27, 1963:

Dear Martin,

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version that I stated. And the Riemann Hypothesis certainly is of that character. We worked it out in our paper [10] quite explicitly. I had the help of a number theorist at NYU, Harold Shapiro, who pointed me in the right direction. But it was clear to everybody who thought about it that the Riemann Hypothesis had the character of being a Π_1^0 proposition, by thinking about the behavior of a Cauchy integral on a path around zeros and approximating the integral in some way or other.

I am certainly no analyst, but the reason I think the Riemann Hypothesis is a good candidate for undecidability by elementary methods is that it is sitting right in the middle of classical analysis, and it has been attacked by brilliant mathematicians—Paul Cohen spent a lot of time on it—and the existing methods just don't seem to resolve it. It's hard to believe it isn't true. And why shouldn't it be one of those propositions that require set-theoretic methods? That would be great!

Suppose someone proves that the existence of a measurable cardinal implies the Riemann Hypothesis. Would mathematicians accept that as a proof of the Riemann Hypothesis? Whether there exist measurable cardinals is something that can't be proved from the Zermelo-Fraenkel axioms. Evidence for it is like the kind of evidence that physicists come up with, not the kind of evidence that mathematicians typically want. I hope it's clear I am presenting all of this as a wild speculation, not something that I believe is true.

***Notices:** The Diophantine equation that you can reduce the Riemann Hypothesis to—what does that thing look like? Is it horribly complicated?*

Davis: Sure.

***Notices:** So you can't just look at it and get any information.*

Davis: No. What I say is, This is an equation that only its mother could love.

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American Mathematical Society

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Fourth Annual Einstein Lecture

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October 17–19, 2008 (Friday–Sunday)
Western Michigan University, Kalamazoo, MI
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University of Alabama, Huntsville, Huntsville, AL
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