# Remembering Paul Cohen (1934-2007) 

Peter Sarnak, Coordinating Editor



Paul Cohen

Paul Joseph Cohen, one of the stars of twentieth-century mathematics, passed away in March 2007 at the age of seventy-two. Blessed with a unique mathematical gift for solving difficult and central problems, he made fundamental breakthroughs in a number of fields, the most spectacular being his resolution of Hilbert's first problem-the continuum hypothesis

Like many of the mathematical giants of the past, Paul did not restrict his attention to any one specialty. To him mathematics was a unified subject that one could master broadly. He had a deep understanding of most areas, and he taught advanced courses in logic, analysis, differential equations, algebra, topology, Lie theory, and number theory on a regular basis. He felt that good mathematics should be easy to understand and that it is always based on simple ideas once you got to the bottom of the issue. This attitude extended to a strong belief that the well-recognized unsolved problems in mathematics are the heart of the subject and have clear and transparent solutions once the right new ideas and viewpoints are found. This belief gave him courage to work on notoriously difficult problems throughout his career.

Paul's mathematical life began early. As a child and a teenager in New York City, he was recognized as a mathematical prodigy. He excelled in mathematics competitions, impressing everyone around him with his rare talent. After finishing high school at a young age and spending two years at Brooklyn College, he went to graduate school in mathematics at the University of Chicago. He arrived there in 1953 with a keen interest in number theory, which he had learned by reading

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some classic texts. There he got his first exposure to modern mathematics, and it molded him as a mathematician. He tried working with André Weil in number theory, but that didn't pan out, and instead he studied with Antoni Zygmund, writing a thesis in Fourier series on the topic of sets of uniqueness. In Chicago he formed many longlasting friendships with some of his fellow students (for example, John Thompson, who remained a lifelong close friend).

The period after he graduated with a Ph.D. was very productive, and he enjoyed a series of successes in his research. He solved a problem of Walter Rudin in group algebras, and soon after that he obtained his first breakthrough on what was considered to be a very difficult prob-lem-the Littlewood conjecture. He gave the first nontrivial lower bound for the $L^{1}$ norm of trigonometric polynomials on the circle whose Fourier coefficients are either 0 or 1 . The British number theorist-analyst Harold Davenport wrote to Paul, saying that if Paul's proof held up, he would have bettered a generation of British analysts who had worked hard on this problem. Paul's proof did hold up; in fact, Davenport was the first to improve on Paul's result. This was followed by work of a number of people, with the complete solution of the Littlewood conjecture being achieved separately by Konyagin and McGehee-Pigno-Smith in 1981. In the same paper, Paul also resolved completely the idempotent problem for measures on a locally compact abelian group. Both of the topics from this paper continue to be very actively researched today, especially in connection with additive combinatorics.

As an instructor at the Massachusetts Institute of Technology (1958-59), Paul was introduced to the problem of uniqueness for the Cauchy problem in linear partial differential equations. Alberto Calderón and others had obtained uniqueness results under some hypotheses, but it was unclear whether the various assumptions were essential. Paul clarified the understanding of
this much-studied problem by constructing examples in which uniqueness failed in the context of smooth functions, showing in particular that the various assumptions that were being made were in fact necessary. He never published this work (other than putting out an Office of Naval Research technical report), but Lars Hörmander incorporated it into his 1963 book on linear partial differential operators, so that it became well known. This was one of many instances showing that Paul's impact on mathematics went far beyond his published papers. He continued to have a keen interest in linear PDEs and taught graduate courses and seminars on Fourier integral operators during the 1970s, 1980s, and 1990s.

After spending two years in Princeton at the Institute for Advanced Study, Paul moved to Stanford in 1961; there he remained for the rest of his life. He said that getting away from the lively but hectic mathematical atmosphere on the East Coast allowed him to sit back and think freely about other fundamental problems. He has described in a number of places (see, for example, [Yandell]) ${ }^{1}$ his turning to work on problems in the foundations of mathematics. By 1963 he had produced his proof of the independence of the continuum hypothesis, as well as of the axiom of choice, from the axioms of set theory. His basic technique to do so, that of "forcing", revolutionized set theory as well as related areas. To quote Hugh Woodin in a recent lecture, "It will remain with us for as long as humans continue to think about mathematics and truth." A few years later, Paul combined his interest in logic and number theory, discovering a new decision procedure for polynomial equations over the $p$-adics and the reals. His very direct and effective solution of the decision problem has proved to be central in many recent developments in the subject [MacIntyre] ${ }^{2}$.

After 1970 Paul published little, but he continued to tackle the hardest problems, to learn and to teach mathematics, and to inspire many generations of mathematicians. I was a member of the new generation who was lucky enough to be around Paul.

I first heard of Paul when I was still an undergraduate in Johannesburg in the 1970s. I was taken

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Paul Cohen as a young man attending Stuyvesant High School.
by the works of Gauss, Dirichlet, and Riemann, but studying with them was not an option. However, Paul, whose work on the continuum problem is recorded in any good introductory text on mathematics, was apparently alive and well and living in California. Moreover, his reputation and stories of his genius had reached all corners of the mathematical world. Kathy Driver (then Kathy Owen and today dean of science at the University of Cape Town) had just returned from Stanford with the following kind of description of Paul, which she emailed me recently.
"Paul was an astonishing man. Impatient, restless, competitive, provocative and brilliant. He was a regular at coffee hour for the graduate students and the faculty. He loved the cut-and-thrust of debate and argument on any topic and was relentless if he found a logical weakness in an opposing point of view. There was simply nowhere to hide! He stood out with his razor-sharp intellect, his fascination for the big questions, his strange interest in 'perfect pitch' (he brought a tuning fork to coffee hour and tested everyone) and his mild irritation for the few who do have perfect pitch. He was a remarkable man, a dear friend who had a big impact on my life. A light with the full spectrum of colours."

I set my goal to study with Paul in the foundations of mathematics and was lucky enough to get this opportunity. Paul lived up to all that I expected. Soon after I met him, he told me that his interests had shifted to number theory and, in particular, the Riemann hypothesis, and so in an instant my interests changed and moved in that direction, too.

Given his stature and challenging style, he was naturally intimidating to students (and faculty!). This bothered some and is probably the reason he had few graduate students over the years. I have always felt that this was a pity, because one could learn so much from him (as I did), and he was eager to pass on the wealth of understanding that he had acquired. Once one got talking to him, he was always very open, and he welcomed with enthusiasm and appreciation others' insights when they were keener than his (which I must confess didn't happen frequently). As a student, I could learn from him results in any mathematical area. Even if he didn't know a particular result, he would eagerly go read the original paper (or, I should say, skim the paper, often filling in his


Group photograph from the 72nd birthday conference for Paul Cohen, taken on October 5, 2006. Cohen is in the front row, third from the left.
own improvised proofs of key lemmas and theorems) and rush back to explain it. His ideas on the Riemann hypothesis (about which I plan to give a brief description in his forthcoming collected works) led him to study much of the work of Atle Selberg and especially the "trace formula". This became my thesis topic. Paul and I spent a couple of years going through this paper of Selberg's and providing detailed proofs of the many theorems that were announced there. We wrote these up as lecture notes that both Paul and I used repeatedly over the years for classes that we taught. Sections of these notes have found their way into print (in some cases with incorrect attribution), but unfortunately we never polished them for publication.

Paul continued to work on the Riemann hypothesis till the end, not for the glory but because he believed in the beauty of the problem and expected that a solution would bring a deep new understanding of the integers. As mentioned above, it was his strong belief that such problems have simple solutions once properly understood. This gave him the courage to continue this lifelong pursuit. When working on such problems, one is out there alone, with nothing to fall back on. Most professional mathematicians simply don't take this kind of risk.

At Stanford, Paul and his wife, Christina, hosted many dinners and parties for students (graduate and undergraduate), faculty, and visitors to the department. I remember many occasions on which Paul would treat a visitor to a personal guided tour of San Francisco and the Bay Area. This opening of their home and their hospitality is remembered fondly by many mathematicians around the world. Paul loved children, captivating their attention with his youthful and positive outlook on life. To my daughters he is still known as "the magician",
because every visit to our home was highlighted by one of his enthusiastic displays of tricks.

The 2006 meeting in Stanford, celebrating Paul's mathematics and also his seventy-second birthday, brought together a mix of world-leading mathematicians from different areas. Logicians, analysts, and number theorists, who normally would barely interact, were unified by Paul's interests and far-reaching impact. None of us (including Paul) appreciated the seriousness of his rare lung condition that had surfaced some months before the meeting. Paul did his best to attend the lectures, offering his usual penetrating insights, always delivered with a touch of humor.

Paul's passing marks the end of an era at Stanford. The world has lost one of its finest mathematicians, and, for the many of us who learned so much from him and spent quality time with him, it is difficult to come to terms with this loss.

## Angus MacIntyre


#### Abstract

When Paul died, Peter Sarnak wrote to let me and John Coates know, describing us as Stanford people. Despite the shock of the news, this phrase brought a consoling sense of community. I spent three years at Stanford, as a graduate student, from 1964-67, and feel profound gratitude to the Stanford people whose teaching example made me a mathematician. There were many such people, including Dana Scott, my enduring model of an inspiring supervisor. Right at the center, interested in everything, was Paul Cohen, an unforgettably powerful presence.

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When I arrived, he was on leave. Naturally I was eager to meet such a legend, but in fact it took quite a long time after his return for us to have any significant interaction. I attended some lectures he gave and quickly got a sense of a very characteristic way of teaching, hands on, avoiding overly elaborate ideas in favor of direct constructions. The style was supremely confident, but never arrogant. I was shy in those days and at first made no effort to talk to him. What might one have spoken about? Set theory, I suppose. In fact, we did not discuss set theory till the last years of his life. Already in 1965 he had moved on mathematically, and I was no longer starryeyed about the foundational significance of set theory (though my admiration is undiminished for the mathematics of set theory that flowed from his work).

At this time, ultraproducts flourished in the theory of models, and striking application had recently been found to $p$-adic fields (and more generally Henselian fields) by Ax and Kochen. Independently Ershov found similar results by methods closer to those of Abraham Robinson. Though the range of the methods was very wide, most interest came from the application to a weak version of a conjecture of Artin that $p$-adic fields are $C_{2}$. The weak version turned out to be optimal. Both methods are memorable, but not very constructive (and ultraproducts were memorably described by Mumford as far out-precisely the kind of technology Paul avoided). When Simon Kochen told Paul, in 1964, of his work with Ax (with its analogy to real closed fields), Paul replied that he had another approach to the real closed case and would think about an extension to the $p$-adic cases. Eventually he gave some talks on this in the Stanford logic seminar, and I was asked to be note taker. My note taking rapidly dwindled to listening and later chatting to Paul about his method and related matters. In the process, I discovered that he had alternative proofs for various results I had found using ultraproducts. His proofs were so direct that I found it hard to understand how he could have found them (mine were a bit flashy and completely nonconstructive). I was lacking confidence, he was full of it, and a heroic figure, but he soon made me feel at ease. It is misleading to call him daunting, as I have sometimes done. Rather,


From the Stuyvesant High School Indicator, January 1949. Stuyvesant Math Team, Fall 1948. Top, left to right: Unknown, Unknown, Elias Stein, Harold Widom, Paul Cohen. Bottom: Unknown, Martin Brilliant, Maurice Silverman.
he had exceptional mathematical authority. What he insisted upon was clarity and genuine understanding. I remember that he was due to be one of the committee who would quiz me in advance of the final submission of my thesis. I greatly feared that he would expose my still insecure understanding. In any event, he fell off his bike that morning (no real harm done, fortunately) and could not attend.

The proof he had found for real closed fields, giving simultaneously an elimination theorem and a primitive-recursive decision procedure, remains for me a marvel of brevity, clear organization, and genuine constructivity. It needed only the sign change property and Rolle. The analogous, but deeper, proof for $Q_{p}$ was destined to be influential over a long period of time. It is again constructive but also geometric in a way that the Ax-Kochen and Ershov proofs were not. It depended only on Hensel's lemma and gave a "cell decomposition" that would be refined by Jan Denef in the 1980s to provide a powerful technique for showing rationality of $p$-adic Poincaré series and that then became (sometimes in nonconstructive versions) one of the main concepts in model theory of valued fields and motivic integration. Paul's proof, based on quite complex inductions, automatically gave, without any model theory, the transfer theorem of Ax-KochenErshov (and with subastronomical bounds). In particular it made certain uniformities in $p$ quite unmysterious. When, in the 1980s, it became necessary to get cell decompositions uniformly in $p$ for uniform rationality results, Pas did this nonconstructively, and I did it by simply looking carefully at the proofs by Paul and Denef. My earlier quantifier elimination for individual $Q_{p}$ was certainly influenced by Paul's paper, though I gave a proof in Ax-Kochen style.

Paul's $p$-adic work impressed me deeply, and still does after forty-five years, for its no-nonsense constructivity, minimal assumptions, and finely balanced inductions. It has, as has the work on forcing, a certain sense of being just right. One could not reasonably call these proofs ingenious. Rather, they could come only from someone with exceptionally penetrating vision. There are no tricks, or twists and turns. These are proofs
destined to last. Of forcing he wrote: "basically it was not really an enormously involved combinatorial problem; it was a philosophical idea."

I left Stanford in 1967. That we met again came about from a suggestion of Michael Atiyah (his fellow Fields Medalist from 1966). Michael and I were involved in organizing a Royal Society Discussion Meeting entitled "The Nature of Proof". The main hope was to bring mathematicians doing conventional proofs and computer scientists working on automated deduction and fully formalized proofs closer together. Michael had recently been a fellow panelist of Paul's at a meeting in the United States, and he suggested that Paul be invited. I confess that I was unsure that Paul was a natural choice for that particular topic, but I felt sure he would be a great attraction, and I personally was thrilled at the chance of seeing him again. Paul accepted the invitation and attended the meeting, held in London on October 18-19, 2004. He gave a talk on "Skolem and Pessimism in Mathematics" and took part in a panel discussion with Michael Atiyah and Jean-Pierre Serre. Paul was initially quite diffident in the discussion, but eventually he came in with some passionate remarks about beauty in proof. The meeting was probably premature, and the two communities remained far apart.

When I first saw him at the Royal Society, I saw no real change. We got chatting warmly right away, after some pleasantries about how thin I had been at Stanford. The enthusiasm and bonhomie shone as before. Yet, in his talk I got a sense of farewell (this is explicit in the text, right at the end). The published version was prepared when he was recovering from the accident sustained soon after the RS meeting. I think he would have liked to revise the paper, but instead he gave me carte blanche to make whatever changes I felt appropriate. I remember saying to an RS editor that the paper was somehow valedictory and that, coming from someone destined to be in the history books, it would be of special interest. The last paragraph, characteristically frank and passionate, voices a fear that the age of proof may come to an end.

We shared a taste for Skolem. From my teenage days, I had known and admired his logical work, and, in Cassels's lectures at Cambridge, I came upon and was fascinated by his $p$-adic analytic method for finiteness theorems in arithmetic. Paul clearly saw himself as having reflected philosophically on the relevance of the Skolem "paradox" to understanding how one might construct models of


Cohen in 1966, the year he received the Fields Medal.
set theory, and he was characteristically generous in his praise of Skolem's vision. In our next meeting, we would return to Skolem and to our mutual interest in effectivity in number theory. We did not once discuss contemporary set theory.

In April 2006 came "Horizons of Truth" in Vienna, to celebrate the Gödel Centenary. I met Paul and Christina on the flight from London. We took a cab from the airport, and Paul and I got right back to Skolem. We had quite a bit of time together, and I heard from him a story of deep interest to me, concerning the putting down of pretension. I had the pleasure of meeting his twin sons and later talking to them about Paul's special mathematical qualities.

His Vienna lecture was highly emotional and powerful, as were his concluding words at the gala dinner. Naturally he was lionized in Vienna. He wore his fame without pretension, and he spoke as passionately as he lived his mathematics. One sensed a genuine humility about his good fortune.

Afterward he wrote, especially happy that Hilary Putnam had described the meeting as the best ever. He referred to his enjoyment in listening to an exchange between me and Harvey Friedman (about proving Fermat's Last Theorem in first-order Peano arithmetic, something on which I subsequently wrote, but without the penetrating advice Paul could have given me). To my great surprise, he also suggested that he and I might think about a project on proof checking.

My last meeting with Paul came at the birthday meeting held in Stanford in fall 2006. Paul was very taken, as far as logic was concerned, with the talks of Alekos Kechris and Saharon Shelah, one relating to Paul's early common interests with Ilya Piatetski-Shapiro, and the other ranging over the seemingly limitless potential of forcing. Paul did not seem at all well, but at the banquet he and his friends rose to the occasion in a most memorable way. The friends made vivid the bonhomie of the Chicago days, the Stanford days, the days in Sweden, and the enduring affection. Paul himself spoke with deep emotion. I was greatly moved by his tender memories of family holidays. I had to leave early the next morning, so after the speeches, I went over to say goodbye to Paul. With a strong handshake, thanks, and his usual smile, he said goodbye. I had a foreboding this would be our last meeting.

His son Charles writes of his wide-ranging enthusiasms, his courage, a life of spirit. This rings very true with me. Cavafy's "Ithaca," read
by Charles at the Memorial Meeting, had intense meaning for Paul. Though the spirit is somewhat different, the last line of Tennyson's "Ulysses" also comes inevitably to my mind:

To strive, to seek, to find, and not to yield.

## John G. Thompson

## An Appreciation of Paul Cohen

My great good fortune was to begin graduate school in 1955 at the University of Chicago. The faculty of Weil, Stone, MacLane, Albert, Chern, Halmos, Kaplansky, Zygmund, Calderón, Browder, Schilling, Spanier, Lashof, and Dyer turned Eckhart Hall into a haven for us graduate students, including Cohen, Towber, Stein, Schanuel, Bass, Posner, Hertzig, and Guido and Mary Weiss.

The respective leaders of the two parties were unmistakably Weil and Cohen, who somehow did not hit it off. This cannot be because of disparate mathematical gifts or interests. For whatever reason, Paul wrote his dissertation under the direction of Zygmund.

My friendship with Paul began during this heady time and in spite of the different paths we followed, continued until Paul's death. His powerful intellect and love of life charm me even now in my sadness at his absence.

## Saharon Shelah

## Forcing Is Great

Unlike the other writers, I unfortunately had little personal contact with Paul Cohen. Before the conference at Stanford in October 2006, I had met Paul briefly on two occasions, at Stanford in 1974 and at the Mittag-Leffler Institute in 2000. What follows are some excerpts from my lecture at the 2006 Stanford conference, including some of Paul's comments during and after the lecture. The lecture was concerned with the far-reaching implications of forcing in mathematics, and as a starting point I put on the board the equation
"Paul Cohen intersect myself" = "Forcing"
to which Paul suggested jokingly during the lecture that perhaps the equality sign might be replaced by a greater than or equal to sign. As is well known, forcing was introduced in connection with proofs

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Christina and Paul Cohen.
of independence. Gödel has given statements of independence, but they normally have the form "this theory is consistent", so it says that the axioms do not exhaust our intuition, because it seems to me that it is hard to believe a theory but not its consistency. There is much to be said for Gödel's method and its many illustrious descendants, but not here and now.

Given a problem which you despair in proving or disproving, Gödel may by luck help in establishing independence; however, Paul's forcing is by contrast a robust method that one can try to use and, if the problem has set theoretic aspects (so not speaking only on natural numbers), it often succeeds.

This is the news, but is it good news? Many think it is not. For example, you can interpret much of Hugh Woodin's work as an attempt to remedy this bad news and AlexanderKechris's work as trying to avoid it. I think that the news is not only good but exhilarating. Is it not hubris to think that you can write down all the axioms? Of course, no formal answer to the good news/bad news questions can be given, but one can still argue one way or the other.

Below are some reasons for my "good news" point of view. The examples mentioned supporting these are naturally heavily biased toward my own experience and interests.

1) Forcing saves mathematicians from futile efforts. Many mathematicians feel that independence is very nice, interesting, fascinating, or rather "fishy", but in any case not really relevant to the problems that are in their backyard. For many mathematicians there were times when they were trying to resolve what cannot be resolved, and I believe this will occur many more times in the future.
2) Better we know the truth even if it is bad news.
3) Even if we are not interested in independence, in several directions after forcing has cleared the "trash/noise", the problems that remain draw attention and eventually are solved once the
camouflage of independent statements has been removed.
4) Encountering independence can direct us to the right definitions.
5) There are universes (or, if you prefer, additional axioms) that are interesting, illuminating for some sets of problems but almost by definition not for all of them. With forcing we can discover such universes.
6) Not only is forcing a good way to prove the consistency of additional axioms, it is an excellent way to phrase them-prototypical is the Martin axiom.
7) Forcing has helped to prove results in ZFC.

In the lecture, examples illustrating these various points were given. Here I limit myself to a couple.

As far as (1) goes, a good example is the problem in abelian groups concerning the set of all group extensions $\operatorname{Ext}(G, K)$ of $G$ and $K$ modulo the relevant isomorphisms and, in particular, whether $\operatorname{Ext}(G, Z)=0$ is equivalent to $G$ being free, this being the Whitehead problem; recall that $\operatorname{Ext}(G, Z)=0$ means that if $Z \subseteq H$ and $H / Z$ is isomorphic to $G$, then $Z$ is a direct summand of $H$. See [Nun77] for why it is interesting. ${ }^{3}$ Not only are there now detailed independence results for these (see [EM02] on the subject), but this also gives a good example for reasons (5) and (6). Moreover, the understanding of the Whitehead problem using forcing is an example of (7) since, along the way, the following theorem is proven:
"If an abelian group is of so-called singular cardinality $\mu$ and every subgroup of smaller cardinality is free, then so is the group itself."

This has no direct connection to forcing, but one would not have been led to believe it or to prove it had the previous independence results not indicated that for other cardinals, the so-called regular cardinals, the situation is different, hence an induction on cardinals works for them.

A second example concerns measure. After Lebesgue's introduction of his measurable sets, the question arose of whether all "reasonable" sets of reals, that is, ones not definable by using a well ordering of the reals, are measurable. This has the following natural interpretations: can we prove the consistency of
(a) ZF + weak choice + every set is Lebesgue measurable
(b) every set of reals defined simply in a descriptive set-theoretical sense is Lebesgue measurable.
The consistency of (a) and (b) were proved by Solovay [So70] (to be precise provided that ZFC + there is a strongly inaccessible cardinal is

[^3]consistent; and there are strong reasons to believe so) not too long after Cohen introduced forcing. In extensions of related questions concerning dimensions of Maharam spaces of measures defined on Borel subsets [Fre89], the question arises of whether the dimension of such a space can be $\aleph_{0}$, and the answer is no [GiSh: 89]. The proof of this uses forcing, and we single it out as it exemplifies our point (7). An earlier brilliant, different illustration of (7) occurs in infinite combinatorics, where there are theorems for which the first proofs used forcing and for which later proofs were found that were more direct (though more complicated) and that avoided forcing (see Baumgartner-Hajnal [BaHa73]).

At the end of my lecture, Paul was rather complimentary and said that he expected his method of forcing to be good in set theory but not for problems in other fields. He said that he felt like a father whose sons have taken things far further than he could have hoped and, moreover, that using forcing to prove theorems in ZFC thrilled him.

Paul said that he was interested in trying to eliminate the inaccessible in Solovay's proof and would like an explanation of the later history. A detailed answer, including all the wrong turns, mistakes, and reinterpretation involved, was given in [Sh: 84].

It is a tribute to the so-called "dark ages" of set theory, from Gödel to Cohen, that almost all problems on the axiom of choice that were not solved turned out to be independent. Concerning forcing, it is and will continue to be an important piece of mathematics, an indispensable part of set theory, a central powerful and profound method oblique in its applications.

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## Harold Diamond

## Recollections of Paul Cohen

I knew Paul from my time as a graduate student at Stanford in the early 1960s. My first recollection of him was sitting in the Union building, drinking coffee and talking with students about their mathematical problems.

While taking a harmonic analysis class from Paul, I came to his attention in a somewhat unfortunate way. Paul's teaching style was to stress the ideas underlying proofs, as if he were doing research on the topic. After he gave what I considered a drawn-out conceptual proof of the RiemannLebesgue lemma, I offered a quick formal proof. From that moment, I was a marked man, often the target of Paul's questions and remarks. On one memorable occasion, his question was prefaced by the words "Diamond, as the product of a great eastern educational institution, would you tell the class..."

When, in spite of such occurrences, I became Paul's doctoral student, we each conceived a project for me in the area of analytic number theory. My proposal was to improve the error term in the elementary proof of the prime number theorem (PNT), and Paul's was to work on A. Beurling's theory of generalized prime numbers. Both were good topics, and, interestingly, each was connected with a Scandinavian acquaintance of Paul's at the Institute for Advanced Study.

Elementary proofs of the PNT were still quite new then, and their error terms differed from the main term by a fractional power of a logarithm. It seemed likely that one might do quite a bit better than that. The basis of known proofs was a formula of A. Selberg. From a generating function analogy, Paul discovered a family of formulas that had main terms of order $x \log ^{2 k+1} x$ and an error term of order $x \log ^{k} x$ for $k=0,1,2, \ldots$, of which Selberg's formula was the case $k=0$.

Paul, who was exceptionally quick, soon decided that he could not use this formula to establish the Riemann hypothesis, so I was free to apply it to my project. While I was working to extract the PNT from the formula, I learned that two (!) papers had appeared in the preceding year, by E. Wirsing and by E. Bombieri, that achieved PNT error terms of the type I sought. This was deflating news, and I put aside the PNT project to take up the Beurling topic Paul originally had proposed.

[^4]About this time, Paul won a fellowship that he used for a leave in Torremolinos, Spain. "When I get results," I asked Paul, "should I send them to you?" "Just the statements," replied Paul. "If I agree with you, I will assume you can get the proof right, and if I don't believe an assertion, I am going to work on a counterexample rather than read what you wrote." This period was pre-Internet, and each exchange took nearly three weeks. Paul was a good correspondent and helpful advisor, even if removed from the scene, and we exchanged many letters while I worked on my thesis.

After Stanford, we corresponded rarely; once he wrote telling of his ideas on the Riemann hypothesis. I next saw Paul at my class's twentyfifth reunion and for the last time at his birthday conference in the fall of 2006.

Six students earned Ph.D.s under Paul's direction. He probably scared away potential advisees by his quickness of mind and tongue. This was unfortunate, for Paul was bursting with ideas and had much to teach on how to attack problems.

## Dennis Hejhal

## Remembering Paul Cohen

I first met Paul in the fall of 1970, when, as a graduate student at Stanford, I was given an office directly across the corridor from his. At the time, my main interests lay in complex function theory. Paul noticed that I tended to work long hours in my office, and he gradually began coming in to "razz" me a little over my "un-California-like" behavior. Paul's demeanor was very open and informal, and, in short order, we began chatting about all kinds of things.

Paul was intrigued by the fact that I had the good fortune to live in a small apartment attached to the home of George and Stella Pólya. He clearly liked the Pólyas very much. He was also intrigued by my interest in "dusty, old-fashioned" functiontheoretic books and papers-once calling me an anachronism. It was in connection with some reading that I was doing on Riemann theta functions that I first came to witness Paul's uncanny ability to rapidly penetrate to the heart (or "beef") of virtually any mathematical matter. To a beginning graduate student, Paul's quick and perspicacious style could-and sometimes did-come across as more than just a bit unnerving. Paul certainly had a way of making you think.

Those early days also showed me something of Paul's playful side. Once, for instance, I noticed Paul coming in to his office more regularly than normal, seemingly in the midst of some project or

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other. One day, the pattern changed. Up and about quite a bit, Paul spotted me hard at work under my usual pile of papers. He came in and, after teasing me a bit, remarked that he was envious-that I was getting something done that afternoon and he was not. Continuing, he then jokingly declared that he wanted "a brain transfusion" from me to reinvigorate his concentration abilities! We both had a good laugh. (Evidently, there were certain advantages to having an office by oneself with no window, especially in sunny California.)

Though my thesis work with Max Schiffer centered on classical function theory and automorphic forms, Paul's ongoing enthusiasm for hard problems coupled with Pólya's wealth of stories basically made it inevitable that the Riemann zeta-function $\zeta(s)$ would come to entice me more and more. I can still remember Paul pulling down his well-used copy of Titchmarsh's book [on zeta] from a shelf near his desk to show me something about an old identity during one of our earliest chats in his office. When I systematically read Titchmarsh about a year later, it was clear that Paul did not quite approve. Already then Paul was telling me that one needed to focus more on the "harmonic-analytic" aspects of the zeta-function to make serious progress ... and that it would behoove me to look at Atle Selberg's work for an example of real depth.

As I look back, I realize that the influence Paul had on me mathematically really only began to gel properly a couple of years after I left Stanford. (I needed to learn more first.) The push that Paul gave me, however, was a solid one that did a lot to help me get moving in a very good direction.

There was another way-less immediate perhaps, but no less important-in which Paul had an influence on me in the early 1970s. During my Stanford days and several subsequent summertime visits to campus, I had the pleasure of enjoying Paul and Christina's warm hospitality at home on any number of occasions. Paul's frequent laudatory comments about Sweden during these get-togethers played a key role in sparking my interest in that part of the world. That interest ultimately morphed-"after a few zigzags"-into my current affiliation with Uppsala University.

Paul visited Sweden numerous times over the years. In 1993, for instance, he visited the department in Uppsala for two months while on sabbatical, giving a much-appreciated lecture series on set theory. Two years later, in 1995, he received an honorary doctorate from Uppsala.

I began my Uppsala position in 1994. Our former department head recently alerted me to a very curious fact about Paul's visit the preceding year that I can't resist mentioning here. It turns out that, technically, Paul was in Uppsala to study something; i.e., his financial support from the department took the form of a stipend. The
stipend, funded by the G. Gustafsson Foundation (and having the advantage of being tax free), was intended for a researcher with a non-Swedish undergraduate degree who showed "special interest in and aptitude for" mathematical logic. An ad was posted and Paul, who was already in Uppsala, was "encouraged" to apply. He did so and got the award. Fortunately for the department, Paul was the only applicant.

During his stay, Paul not only lectured but also worked on developing a new proof of Gentzen's 1936 theorem on the consistency of arithmetic. Describing Paul's honorary doctorate as an award that was both well deserved and well earned is thus a statement having truth in more ways than one!

The first time I overlapped with Paul in Sweden was in May-June 1978. The program at the MittagLeffler Institute that year (1977-78) focused on analytic number theory and harmonic analysis. Paul was on sabbatical and chose to spend the full year there with his family. The program was an intensive one, with many noted participants. Earlier in the fall, the program's activities had created something of a stir-particularly abroad-thanks to a rumor that Paul had found a proof of the Riemann hypothesis!

Paul was, in fact, working on the R.H., but there was no proof; the rumor soon faded. Lo and behold, after three to four months, the rumor started up again with even greater vigor. (This was before the days of worldwide 24/7 email.) Atle Selberg, who was now back from Sweden, told me that, in the fall at least, Paul had been very much focused on trying to develop a new kind of trace formula over the adeles based on the Poisson summation formula in which the zeros of the zeta-function acquired a natural spectral interpretation. Beyond that, he hadn't heard anything new. (Selberg noted that with Paul, however, one could never be surePaul was a very sharp guy.)

In late May, when I arrived at the Mittag-Leffler Institute, I was naturally looking forward to hearing for myself what was going on. Besides Paul, several other key people from the spring semester were still around, including Enrico Bombieri. To make a long story short, it turned out that both Paul and Enrico were actively thinking about the R.H.-albeit along much different lines.

I have fond recollections of the various discussions that I had during this period. While Enrico sought to use multivariate "Chebyshev" methods to directly estimate the number of primes in suitable intervals, Paul continued to work along trace-theoretic lines [over the adeles] based on Poisson summation and the Riemann-Weil explicit formula. Paul told me more than once that he was convinced that bringing in more of the integers' ring-theoretic structure was crucial; he was experi-
menting with certain "Hecke-like" operators to try to achieve this.

Unfortunately for Paul, the R.H. rumor was still showing sporadic signs of life-e.g., in telephone calls of the "have you proved it yet?" genre. Paul lamented the negative effect this had on both his mood and focus. I also sensed that he was not accustomed to having a "fellow traveler", as it were, hard at work in a lane nearby. ${ }^{4}$

With the approach of Swedish Midsommar, things were naturally coming to a close, and they managed to do so in a very good way. Lennart Carleson, the Institute director, invited a group of us out to dinner at the Operakällaren, one of Stockholm's finest restaurants, located quite close to the Royal Palace. We had a wonderful meal there. Upon returning to the Institute housing area around 10:30 p.m., it was Paul, as I recall, who said: "It's still so light out, let's take a walk down by the [picturesque Djursholm] waterfront." After saying a few good-nights, the three of us-Paul, Enrico, and myself-decided to do just that. As we walked along and talked, still properly attired in our suits, there was much laughter as Paul intermittently imitated Selberg with near perfection (even down to his accent). We soon came to a kind of clearing where one could get closer to the water's edge. Somehow, a small stone got flung and skipped across the surface of the water. This was promptly followed by another, leading to a brief contest to see [what else!] who could get "the most zeros on the line". Paul and Enrico were quite good. I made a few good-natured attempts in honor of, e.g., C. L. Siegel, but my method had a decided propensity for getting them under the line. It was a memorable evening-and a warm-hearted way to close out our respective stays at Mittag-Leffler.

When it came to the Riemann hypothesis, one of George Pólya's favorite lines was a quote that he said originated with Torsten Carleman: ${ }^{5}$ "R.H. is so difficult because its proof will require not one, but two new ideas."

I think it would be a very fine tribute, indeed, if-in the future, when the proof is finally foundit emerged that Paul's early efforts actually had substantial overlap with one, or even both, of Carleman's "two new ideas." Seeing that would give me great pleasure and be completely consistent with my long-time image of Paul.

[^5]
## Thomas C. Hales

## My Teacher Paul J. Cohen

A few other students and I met weekly with Paul Cohen throughout my undergraduate years at Stanford. During the Putnam season, these meetings became coaching sessions for the competition. In the off season, he taught us a vast amount of mathematics. To name just a few topics, Paul Cohen gave me my first significant lessons in sheaf theory, Lie theory, Riemann surfaces, Hilbert spaces, homotopy, and homology.

Galois theory, which he had read from the original works as a teenager at Stuyvesant, was one of his favorite subjects. He even carried the analogy of field extensions into model theory: the adjunction of a generic set to a model of set theory is "akin to a variable adjunction to a field"; and he considered a set in the resulting extension as a function of the adjoined set, just as a rational function is a function of the adjoined variable in the theory of fields. If an axiom is not satisfied, adjoin a solution! This is the man who guided me through basic Galois theory, adjoined $\sqrt{17}$ to the rationals to compute for me the Gauss sum $\sum_{i=1}^{8} \zeta^{n^{2}}$ (with $\zeta^{17}=1$ ) in the construction of the 17gon, and derived for me Cardano's formula for the


Paul Cohen with Eric (left) and Steven (right). cubic.

He had no patience for proofs that failed to dazzle. Once he barged in on James MacGregor's lecture after peering through the window at a proof of Hölder's inequality that was not to his liking. He took control of the blackboard, gave his own proof, then went on his way.

He sparred with me on almost any topic. If I conjugated German verbs, he coached my accent with his Yiddish. If I studied the Schrödinger equation, he countered with the Dirac equation. If I read John Stuart Mill's Political Economy, he argued the logical flaws of Das Kapital.

His influence on my life has been profound. My admiration for him bordered on worship (and still does). I was constantly aware of being in the presence of genius and hung onto every word of his, thinking for days about proofs that came to him in

[^6]

Paul Cohen with Lyndon Johnson receiving the National Medal of Science. At a White House ceremony on February 13, 1968, President Johnson presented Cohen with the 1967 National Medal of Science for "epoch-making results in mathematical logic which have enlivened and broadened investigations in the foundation of mathematics." Calling Cohen "one of the most brilliant of mathematical logicians," Johnson said: "His work has greatly influenced the foundation and development of mathematics."
a flash. When I first arrived at Stanford, I was the kid from Utah with a rather limited background. When I left, I imagined myself Paul Cohen's youngest protégé. Cohen's interests in the early 1980s, especially his graduate courses on Lie theory and the trace formula, helped to shape my decision to seek Langlands as my graduate advisor. More than any others, I have him and Bob Langlands to thank for my mathematical education.

As an undergraduate reading about Jacobi's power series in Hardy and Wright, Cohen dreamed there should be a decision procedure for power series. Years later, a decision procedure took shape to decompose a domain (in a field of characteristic zero, complete under a discrete valuation) into cells on which elementary questions are trivially decided. One of his original applications of the cell decomposition was to prove that the truth of certain statements is independent of the characteristic of the underlying field, in the spirit of Ax and Kochen. Motivic integration builds on Cohen's concept of cell decomposition in the work of Pas, Cluckers, Denef, and Loeser. In motivic integration, the measure is first defined on cells, then shown to be independent of the decomposition of the domain into cells. Motivic integration develops the theme of field independence even further: as a corollary of general results about the field independence of $p$-adic integrals, it transfers Ngô's proof of the fundamental lemma from positive
characteristic to characteristic zero, where it has profound applications to the theory of automorphic representations.

Paul Cohen's proof of quantifier elimination for the elementary theory of the real numbers has been influential in formal proofs. The CohenHörmander decision procedure has been implemented by Harrison and McLaughlin in the formal proof assistant HOL-Light. There are other algorithms for quantifier elimination that have faster execution, but none that can compete with his in ease of implementation and in simplicity of formal expression.

He held that great mathematics is simple and can come in a flash. He disliked the rise of ponderous research programs with multitudes contributing small steps. Trying to sum up his thought, I think of Zarathustra's aphorism on aphorisms, "In the mountains, the shortest way is from peak to peak: but for that one must have long legs." His proofs are aphorisms that span some of math's most majestic peaks.

## Mihalis Kolountzakis

## Being a Student of Paul Cohen

I first met Paul in September 1989. It was my second day at Stanford as a graduate student, and Paul was giving us a real analysis exam. I did not know him before that, though I certainly knew of him and his work on the continuum hypothesis. At that point I knew nothing about his work in harmonic analysis nor, in particular, about his idempotent theorem for general groups and his results on the Littlewood conjecture, which were his contributions that would influence my mathematics the most.

About a year after that, Paul taught a twoquarter course on harmonic analysis. I liked his lectures very much. They were sufficiently improvised to allow one to see how he thought, not just the finished proofs. He also had the singular habit of spending about half of each lecture going over what he did in the previous lecture. This repetition was very helpful to me, if somewhat unorthodox.

After taking that course I asked him to be my thesis advisor. The deal was this: he thought I was a good student and he would take me on, but I should not expect him to give me a problem to work on. I could not imagine a shortage of problems, only of solutions, so I did not think twice. I was not even swayed by some stories that were circulating among the graduate student population at Stanford regarding Paul. During those three-plus years that he was my thesis advisor, Paul proved

[^7]to be a very pleasant person, and I never had any complaints about him of a personal nature.

My only problem was that it was a little hard to make him think about the problems I was working on. Most of our regular meetings were spent in talking about various important problems of mathematics, especially the Riemann hypothesis. I should clarify here that my contribution to these discussions was more that of a sounding board. We also had lots of very enjoyable discussions about physics, computers, history, and politics. Occasionally I would succeed in talking to him about my results and failures. He was very supportive, especially with the latter. He somehow managed to show me much more trust than I had for myself. If there is a single aspect of his personality as a teacher that I'd like to keep from him, that's it.

I remember once Paul was away on sabbatical, and I had developed some interest in decidability and complexity questions about polynomial equations over the reals, largely unrelated to my thesis problems. I had not talked to Paul about this, but I spoke once to Greg Brumfiel and asked him some related questions. He smiled and informed me that a person who had very significant contributions to this was in fact Paul Cohen! I had no idea about this.

A couple of years ago I was interested in more concrete forms of Wiener's Tauberian theorem. Once again, a paper by Paul came up in my search ("A note on constructive methods in Banach algebras") that was extremely relevant to what I was looking for. I suppose such things are not really coincidences: one's taste in problems is influenced in ways one does not realize until later.

When I was still a student I never thought I'd use Paul's idempotent theorem. After all, before Paul proved the idempotent theorem for all locally compact abelian groups, it was already proved by Helson and Rudin for many concrete groups. Not liking abstract things more than I had to, I did not expect to ever need the general case. But Paul's theorem almost made my career. A year after I had left Stanford I cooperated with Jeff Lagarias on a paper about the structure of tilings of the real line by translation of a function. The main technical ingredient of that paper was Paul's general idempotent theorem (in a form given to it by Yves Meyer). That led to a series of results that formed the bulk of my work for a number of years. Hard to escape from Paul's legacy.

## Gerald Alexanderson

I had left Stanford by the time Paul Cohen joined the Stanford faculty, but I recall hearing at Stanford his explication of his amazing results in foundations

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Page from the Stanford Quad (1967).
just after they were published. It was pretty exciting stuff, and I was always impressed that a student of Zygmund's would, when so young, be so versatile that he could move into foundations with such resounding success. Virginia (Mrs. Paul R.) Halmos remembers him very fondly from his student days at the University of Chicago, when her husband was on the faculty there. She recalls that Cohen as a young student had an unusually sweet nature. Later in his career there were rumors floating around of his rather dismissive treatment of colleagues, something I never witnessed.

For a long period I rarely saw him, and we only got back in touch after George Pólya died in 1985. When I was clearing out Pólya's things from his office on campus-something that took many hours-largely late in the day or on weekends Paul would sometimes come by, and we would have a long talk, often looking over Pólya's very large collection of reprints, some his own papers but many from friends like Harald Bohr, or Hardy and Littlewood, and often related to the Riemann hypothesis. It was not surprising that Paul was interested in these. Up to the last couple of weeks of his life Pólya remained obsessed with this problem. On several occasions during his last few weeks in the hospital he asked me to go into his study at home and look on his desk where he claimed that he had written down on a sheet of paper what he


Paul and Charles Cohen at the Mittag-Leffler Institute in Djursholm, Sweden, in the sabbatical year 1977-1978.
thought was a very promising idea for proving the R.H. Unfortunately I could not find such a paper.

Paul was clearly very fond of Pólya and thought highly of his contributions over the many decades. During those conversations he couldn't have been nicer and, though he had something of a reputation around Stanford for not suffering fools gladly, he suffered this one quite well on those occasions. And I came to like him as a person. My last encounter with him was at the memorial service at the Quaker Meeting House in Palo Alto for Hans Samelson. By that time Paul had suffered his fall, from which, I gather, he never fully recovered. During the service, at the point when the organizers of the event asked if there was anyone present who wanted to say something about Hans, Paul stood up and tried to talk about what Hans had meant to him personally as a colleague and friend. Unfortunately, he was so overcome his voice broke, and he had to sit down after only a short time. It was a touching moment.

## Ilan Vardi

My first encounter with Paul Cohen was as a precocious high school student, when I somehow got the right to take books out of the McGill University Math Library. At that time, I decided that the continuum hypothesis was the "coolest" famous result, at least one for which I could understand the statement, so I set out to read the book Set Theory and the Continuum Hypothesis. Of course, I didn't get very far, and my impression was of a very dry incomprehensible text. I found that first experience very ironic when I finally read

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the book last winter on the occasion of its new Dover edition. On the contrary, I now found this to be one of the most readable mathematics books ever written, a personal account of Paul Cohen's journey into logic. It highlights his good mathematical taste, as he surveys the most beautiful and important results in the field, even if they do not serve to prove the main result of the book. Of course, what makes the book even more amazing is that it gives a solution to the most important problem in the field.

I first met Paul Cohen in person when I got a job at Stanford as an assistant professor. My thesis advisor, Dorian Goldfeld, had warned me about being cornered by Paul Cohen, who could spend hours discussing his ideas about the Riemann hypothesis. In fact, that never happened. The first few months at Stanford were quite disorienting, the new assistant professors were left completely to their own devices, and I never had any contact with the full professors unless it was about some minor administrative matter. The exception was Paul Cohen, who early on invited me and another assistant professor, Norman Wildberger, to dinner at his home. Norman and I gave him a bit of a hard time about wanting to "touch" his Fields Medal, but he was a good sport and he eventually showed it to us. On that occasion it was quite obvious how devoted Paul was to his family, something which is very important to mention.

This was entirely consistent with the following fifteen years I spent in the Stanford area-Paul was the only academic that I would regularly run into in Palo Alto. In fact, I would often see him and his wife, Christina, at the local cafes, and upon reflection it seems probable that he had me in mind for these outings: "Christina!" "Yes, Paul?" "Let's go to Caffe Verona, Vardi will be there!" Hanging out in cafes was more or less frowned upon in that part of the world, and I would get annoyed at the people who would tell me in an accusatory tone: "You're always here," especially because it was true, though they didn't have the evidence to back it up unless they did the same. Then one day Paul Cohen walked into Printer's Inc., the only cafe bookstore in Palo Alto at the time, and told me: "Vardi, you come here at least as often as I do!" which for me was the final proof of his superior mathematical ability.

This shows that, unlike many stereotypical academics, Paul was a very social person who needed contact with different kinds of people and to do different kinds of things. Later in life, he would also take Christina out to comedy clubs, something completely unusual for academics.

Back in the department, Paul had the habit of challenging people, which could be disconcerting, especially to graduate students, though it never bothered me. For example, he would repeatedly ask me if I knew Feuerbach's theorem, a.k.a. the
nine-point circle, and I eventually looked it up so I could reply that it could be proved by inversion, which basically kept him quiet. The funny thing is that, when I think about it now, it seems perfectly normal to me that any mathematician or aspiring mathematician should be aware of Feuerbach's theorem and its proof. So it seems that Paul Cohen had a beneficial influence on me by convincing me that mathematicians should have a wide knowledge of the most beautiful mathematical results.

When I was at the Institut des Hautes Études Scientifiques (IHÉS), I got interested in uniqueness of trigonometric series, and, much to my surprise, I found a copy of his unpublished Ph.D. thesis in the Orsay library. I decided to typeset it and wanted to know whether I could post it on my website. I called him to ask for his permission and to catch up on the previous few years: "Christina!" "Yes, Paul?" "You'll never believe it, Vardi got married!" That was the last time I ever spoke to him. I found out about his death by reading the annual end-of-year obituary listing in Time magazine, which was more proof of the extent of his fame.

I have since revisited Christina at their home on the Stanford campus. This house is unusually large for California and is at the very heart of faculty housing just next to the president's house. It seems that, at the height of his career, he chose Stanford to get some relief from the pressure of East Coast academia, and that big house represents the shelter he found with his family. Inside his study one can get an idea of his inspirations: above his desk are pictures of André Weil, Atle Selberg, and Peter Sarnak.

In order to get a better understanding of Paul Cohen, I strongly suggest viewing the videos of his Gödel Conference lectures in Vienna, which are posted on YouTube. What I find striking when seeing him again is the delicate way in which he expresses himself and which faithfully reflects his intellectual qualities. My wife, who had never met him in person, found him quite seductive in those videos; of course, she did marry a mathematician....

## Charles Cohen

## My Father the Romantic

For my father, being a mathematician was not a career, it was his calling. Although he solved some of the great problems of twentieth-century mathematics, he was at heart a nineteenth-century mathematician. Even more than that, I would call him a nineteenth-century romantic. To him, mathematics was something beautiful and transcendent, and he lived completely outside the mold
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Left to right: Charles, Eric, Christina, Paul, and Steven Cohen.
of the professionalized and specialized modern academic. Instead, my father studied every subject that interested him, and did so with an astonishing capacity for the absorption and mastery of diverse ideas.

A graduate student at Stanford once told me, in amazement, how my father happened to pass by a class to which the professor had not shown up and ended up teaching the class himself: perfectly, with no advanced preparation or notes. He had a way of making even the most difficult ideas seem simple, even if afterward one could not always explain them as clearly as he had. Personally, one of my fondest memories of my father is sitting on the steps in our hallway at home, late at night, with him explaining deeper and deeper truths in the history of mathematics and physics. At such times it felt as if hundreds of years of scholarship were alive in his mind, and he was channeling the ideas directly to me. It was a truly a thrilling experience, and one that I know must have been shared by many others that he came in contact with.

And it was not just mathematics that inspired him like this. He loved knowledge of all kinds. My father used to say that the way to keep me quiet when I was little was to feed me information, and I could have had no better source of knowledge than he on virtually any topic: history, science, literature, technology. He loved solving the very puzzle of existence-I remember him always mastering one esoteric field after another-from Japanese and Oriental rugs to Morse code and artificial intelligence. Someone once said that when a person dies it is like a great library burns, and this is surely true of no one more than my father.

Of course he was a man blessed with exceptional talents, but I would also say that he had exceptional courage. He did not just dream of living a full and challenging life; he went ahead and did it. In mathematics, he dared to work on problems others were afraid to. I think he wanted to work

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on famous problems not to become famous himself but because he was drawn to great ideas, the same way a composer is drawn to great music. He felt math was something rare and deep in the universe, and this allowed him to conquer any fears of failure that he might have had. Although ultimately this led to great success for him, it also meant that he was driven to work on only the most challenging of problems. I remember walking in the foothills around the Stanford campus as he attempted to explain to me his many years of work on the Riemann hypothesis-work that lived almost completely within his mind, and the solution to which, unfortunately, always remained out of reach. But just the process of his thought was so powerful and beautiful, it remains with me to this day.

But my father was not merely a scholar. He also loved life as an experience, not just as an idea. He was gregarious. He loved fine food, traveling the world, singing, and playing the piano. He especially loved classical music. I recall him sitting at home, listening to Beethoven and reading along with the score, simultaneously decoding the musical progressions and being swept up in the emotion of the experience. In a way, I think this is very much the way he viewed mathematics as well.

Although my father was not a religious man in any doctrinaire sense, I feel that in his life he always strove for truth, beauty, and love, and to me that is the mark of a truly spiritual life, a life full of spirit. I can think of no better definition of a life well lived than that.

My father seemed to have an amazing power to touch people (even those who only met him briefly), and for those of us who knew him well, his passing is a great loss. But his own journey was as full and rich as anyone could hope for, and I hope the courage with which he tackled existence will serve as an inspiration for us all.

Note: Except where otherwise noted, all photographs used in this article are courtesy of the Cohen family.


[^0]:    $\overline{1_{B . ~ H . ~ Y a n d e l l, ~ T h e ~ H o n o r s ~ C l a s s, ~ A . ~ K . ~ P e t e r s, ~ 2002, ~}^{\text {B }}}$ 59-84.
    ${ }^{2}$ A. MacIntyre, In "Records of proceedings at meetings of the London Math. Soc.", June 22, 2007.

[^2]:    John G. Thompson is Graduate Research Professor at the University of Florida. His email address is jthompso@ math.uf1.edu.
    Saharon Shelah is professor of mathematics at the Hebrew University of Jerusalem and a Distinguished Visiting Professor at Rutgers University. His email address is she1ah@ math.rutgers.edu.

[^3]:    ${ }^{3}$ And for its connection to algebraic topology and arcconnectedness.

[^4]:    Harold Diamond is professor emeritus at the University of Illinois, Urbana-Champaign. His email address is diamond@math.uiuc.edu.

[^5]:    ${ }^{4}$ Some time later, when Selberg asked me about my trip, I joked "maybe if you had been there too, R.H. would now be a theorem." He replied: "I doubt that, but I am certain my presence would have helped keep 'the atmosphere there' relaxed."
    ${ }^{5}$ Who, ironically, was the Mittag-Leffler Institute's first director.

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