Remembering Paul Cohen

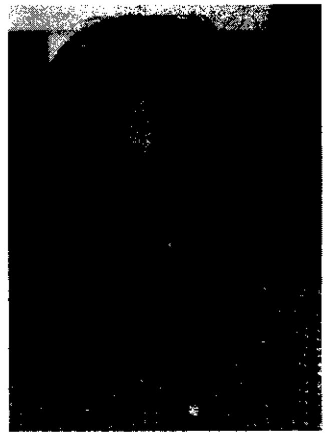
By Peter Sarnak

Paul Joseph Cohen, one of the stars of 20th century mathematics, passed away in March, 2007 at the age of 72. 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 which 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, firstly, the heart of the subject and, secondly, that they 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 with a keen interest in number theory, which he had learned by reading 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. Instead, he studied with Antoni Zygmund, writing a thesis in Fourier Series on the topic of sets of uniqueness.



Paul Cohen. Photograph courtesy of Stanford News Service.

In Chicago he formed many long lasting friendships with some of his fellow students (John Thompson, for example, who remained a lifelong close friend).

The period after he graduated with a PhD was very productive: he enjoyed a series of successes in his research. He solved a problem of Walter Rudin's in Group Algebras and soon after that he obtained his first breakthrough on what was considered to be a very difficult problem — the Littlewood Conjecture. He gave the first nontrivial lower bound for the L1 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 instuctor at MIT, Paul was introduced to the question of uniqueness for the Cauchy problem in linear partial differential equations. Alberto Calderon and others had obtained uniqueness results under some hypotheses, but it was unclear whether the various assumptions were essential. Paul clarified this much studied problem by constructing examples where 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 ONR Technical report) but Lars Hörmander incorporated it into his 1963 book on linear partial differential equations, so that it became well known. This was one of many instances in which 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 70s, 80s, and 90s.

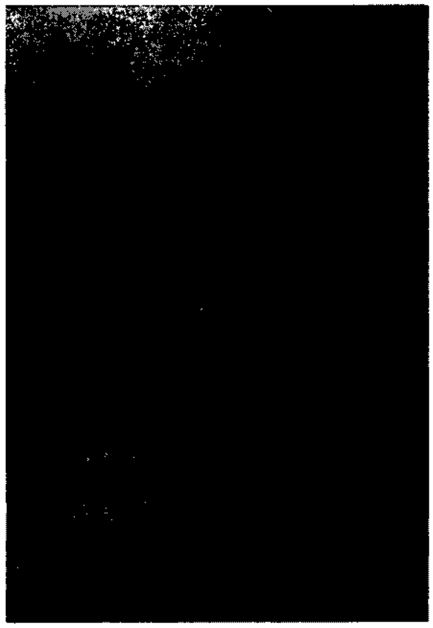
After spending some time in Princeton at the Institute for Advanced Study, Paul moved to Stanford in 1960; there he remained for the rest of his life. He said that getting away from the lively but hectic mathematical atmosphere on the East Coast allowed him to sit back and think freely about other fundamental problems. He has described in a number of places (see, for example [Yandell]) 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 giving 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].

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 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 to 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 sort of description of Paul, which she e mailed 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 with 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 moved to number theory and in particular the Riemann Hypothesis, and so in an instant my interests changed and moved in that direction too.



The Stuyvesant Math Team, Fall 1948. Top row, left to right: Unknown, Unknown, Elias Stein, Harold Widom, Paul Cohen. Bottom row: Unknown, Martin Brilliant, Unknown. Photograph courtesy of Martin Brilliant, from Indicator, *January 1949.*

Given his stature and challenging style he was naturally intimidating to students (and faculty!). This bothered some; it 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 welcomed with enthusiasm and appreciation your insights when they were keener than his (which I must confess wasn't that frequent). 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, he would skim the paper, often creating his own improvised proofs of key lemmas and theorems) and then rush back to explain it.

His ideas on the Riemann Hypothesis (about which I will write elsewhere) 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 one or two years going through this paper of Selberg and providing detailed proofs of the many theorems that were announced there. We wrote these up as lecture notes, which both Paul and I used repeatedly over the years as lecture notes 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 life long 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 where Paul would treat a visitor to a personal guided tour of San Francisco and the bay area. This opening of their house and their hospitality is remembered fondly by many mathematicians around the world.

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.

References

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A. Macintyre, in records of proceedings at meetings of the London Mathematical Society meeting, June 22, 2007.