Autobiography

John Tate

What follows is a sketch of some incidents in my education and early career, followed by a very brief summary of the last 50 years of my mathematical life to retirement.

Before beginning, I would like to thank Jim Milne for his willingness to take on the job of writing about my work, and for the remarkably thorough account he has given of it, with indications of its place in different aspects of the development of arithmetic geometry during the last half of the 20th century.

1925–1937

I was born on March 13, 1925, in Minneapolis, an only child. My father was an experimental physicist at the University of Minnesota who was Dean of the College of Arts and Sciences for a few years before he was called to lead the US antisubmarine research effort during WWII. The Physics building at the University of Minnesota is named the Tate Laboratory of Physics after him. His father was a doctor in rural Iowa, descended from Scotch Presbyterian ministers who had been in the US for several generations, moving west with the frontier. I know little about his mother, except that she was of Irish descent and died when my father was about 12.

My mother had a thorough knowledge of the classics. Before I was born she taught English in high school. Her father had come to the US from Germany as a teenager, settled in Lincoln, Nebraska, and eventually became head of the Germanic Languages department at the University of Nebraska. Her mother had come to Lincoln from Sweden with her family as a child.

I learned about negative numbers at an early age from the mercury thermometer mounted outside our kitchen window. That instrument also illustrated for me the

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J. Tate (✉)
Department of Mathematics, Harvard University, 1 Oxford Street, Cambridge MA 02138, USA
e-mail: tate@math.utexas.edu

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John Tate, age 4

concepts of linear function and coordinate change, for it was marked in Fahrenheit on one side and in Celcius on the other. The point marked -40 on both sides was near the bottom of the scale. The mercury was always above that, but it did fall below -30 F on a few winter mornings, which was a welcome event, for it meant no school.

I loved puzzles. I liked the Pastime jigsaw puzzles cut from plywood which my mother rented at a department store. Even more, I liked to read my father's puzzle books by Henry Dudeney. Most of the puzzles were too difficult for me to solve, but I enjoyed just contemplating the questions, which were usually of a mathematical or logical nature.

From my father I had a good idea of what science was about. He did not push me, but would sometimes explain some basic fact, such that the distance a body falls in x seconds is about $16.1x^2$ feet, or how something worked, like locks in a canal. I liked math and science, but was not very good at arithmetic, and especially hated the long division drills in fourth grade.

1937–1946

For my secondary education I attended Saint Paul Academy, a private day school, where I liked my first math teacher, Max Sporer. Somehow we understood each other. Once when I asked a question he said "your problem, Tate, is that you are trying to think". Another time we were doing a class exercise in which he would write a number on the board and ask one of us to declare it prime or to factor it. I enjoyed this until he gave me 91. By then I was 12 or 13 and my interest in mathematics was becoming clearer. I still remember Mr. Sporer's proof of the quadratic formula, which I thought was quite elegant because he multiplied by $4a$ before completing the square.

After I learned that $n!$ means the product of the numbers from 1 to n, I decided I would privately denote the sum of the numbers from 1 to n by $n?$. I soon realized that this was rather silly since my $n?$ is equal to $(n^2 + n)/2$. However, this led eventually to my first attempt at mathematical research. After learning about the formulas for the sum of the first N squares and the sum of the first N cubes, and playing with sequences and their difference sequences for some time, I managed to convince myself that there were polynomials $P_n(x) = \frac{x^{n+1}}{n+1} + \frac{x^n}{2} + \cdots + C_n x$ such that $\sum_{i=1}^{N} i^n = P_n(N)$, and that these polynomials without constant term could be computed inductively from the relation $P_n'(x) = nP_{n-1}(x) + C_n$, if only one knew the constants C_n. I assumed that there would be a simple formula for C_n in terms of n and set out to try to guess it by computing the first few C_n's. It seemed clear that $C_n = 0$ for odd $n > 1$, but finding $C_2 = \frac{1}{6}$, $C_4 = -\frac{1}{30}$, $C_6 = \frac{1}{42}$, I was baffled, and gave up. Had I gone farther, I would have been even more baffled, because the sequence continues $-1/30$, $5/66$, $-691/2730$, $7/6, \ldots$. The C_n's are the Bernoulli numbers.

E.T. Bell's book *Men of Mathematics*, made a big impression on me. Reading it gave me my first knowledge of the history of mathematics in the West and of some great theorems. From the chapter on Fermat I learned that if p is a prime, then p divides $n^p - n$ for every integer n, and that if $p = 4m + 1$ then p is the sum of two squares. From the chapter on Gauss I learned that one could construct a regular 17-gon with ruler and compass, and also learned the law of quadratic reciprocity, which Bell makes vivid by giving a few explicit examples. This law fascinated me. I tried to figure out why it was so, of course with no success.

I graduated from S.P.A. in May of 1942, and went with no break to Harvard, since it was then on a full time schedule, including a summer term, because of the war. I had five consecutive terms at Harvard, the first three as a civilian, the last two as an apprentice seaman in the Navy's V-12 officer training program for which I volunteered when I became eligible for the draft. I then spent three terms at M.I.T. being trained as a meteorologist, and a term in midshipman school at Cornell University, graduating as an Ensign in the US Navy with a special knowledge of meteorology. But by that time the war in Europe was over, and it had progressed so far in the Pacific that more meteorologists were no longer needed. I was assigned to minesweeping research until I was discharged a year later, having been in the Navy for three years without leaving the east coast of the USA.

1946–1953

During that time I managed to graduate from Harvard. My degree was in mathematics for convenience, but I decided to go to graduate school in physics. This was a strange decision. Although I had always liked both subjects, I was really much more

John T. Tate Jr. with John T. Tate Sr. in New York in 1944

interested in math and had shown more talent for it. But from reading Bell's book I had the idea that to do valuable research in mathematics one had to be a genius like the people he wrote about, whereas from my father's example I saw that with intelligence and hard work one could make a difference in physics. I knew I was no Gauss or Galois, but thought I was reasonably intelligent and could be diligent.

So I began graduate school in physics in Princeton in fall, 1946. At that time physics and math shared a common room in the old Fine Hall. One day a fellow student pointed to a man across the room and said, "That's Artin!" "Who's Artin?" I asked. I was surprised to be told "He's the great algebraist", for I had never heard of him. Only later did I notice that my favorite math book, *Moderne Algebra*, by Van der Waerden, was based on lectures by E. Artin and E. Noether.

As a Navy veteran, I took advantage of a feature of the G.I. Bill of Rights. If a professor certified that a book I wanted would be useful to me in my studies, the US government paid for it. Early in the spring term I realized that I had acquired about twenty mathematics books in this way, but only two in physics. Also, in reading Von Neuman's *Mathematical Foundations of Quantum Mechanics*, I found the first part to be a marvellously clear axiomatic characterization of Hilbert space, but the rest was not clear at all. Deciding finally that I should switch from physics to mathematics, I asked permission from Lefschetz who was then head of the math department. He told me that too many people had been wanting to make the change I requested. Did we think the math prelims, which were oral, were easier than those in physics, which were written? He said I could put my application in with all the other applications to the math department and take my chances.

I did so, and also started to sit in on a couple of math courses. One was a course of Artin's in which he was developing measure theory. At the end of one lecture he stated a lemma and challenged us try to prove it. Highly motivated to show I was a worthy applicant for admission to math, I thought about it many hours with no success, but finally saw the trick in the middle of the night before the next class. In the morning when Artin asked who had found a proof I was the only one to raise

John Tate in his office, Cambridge, MA, 1956

my hand. When he told me to go to the board and explain it to the class I was so nervous that I could barely speak or write. To my great relief, as soon as he saw that I did have a proof, he took over and explained it clearly. Soon after, I was happy to hear that I had been admitted to the math department.

It was a phenomenal bit of luck that I ended up in the department with Emil Artin, the man who had proved the ultimate generalization of my favorite theorem, the law of quadratic reciprocity, and who was also a great teacher and mentor. I became his student, learned a great deal of algebra and number theory from him, and owe to him the suggestion of a wonderful thesis topic, proving by abstract harmonic analysis on the adèle ring the functional equation for Hecke's L-functions which Hecke had proved by classical Fourier analysis. In a sense this was simply a big exercise, but I think I gave a good solution. I soon realized that the topic was in the air at the time; Iwasawa and Weil had the same idea.

After earning my Ph.D. I stayed on at Princeton for three years as an Instructor. During the second year, 1951–1952, I helped Artin in a seminar doing class field theory by cohomological methods. We were very fortunate that Serge Lang, who had just finished his Ph.D. with Artin, took notes and wrote them up into what eventually became the main part of the book *Class Field Theory*, by E. Artin and me, recently republished by AMS Chelsea.

1953–1959

After a year at Columbia University, in which I was very happy to become acquainted with Bernie Dwork, I accepted a tenure track offer at Harvard. Fortunately, Harvard's enlightened policy of offering sabbatical years to tenure candidates enabled me to spend the year 1957–1958 in Paris. That was a great year for me. I attended Serre's lectures, witnessed the birth of the theory of schemes in Grothendieck's seminar, and became friends with those two amazing individuals who were to have such an important influence in my mathematical life. I found new directions for my research which I will briefly describe.

I had become interested in the arithmetic of elliptic curves and, thanks to Lang's influence, also Abelian varieties. The key results of class field theory are equivalent to knowing the Galois cohomology of the of the multiplicative group over local and global fields and their interrelations, in dimensions 1 and 2. I hoped my background in that area would help me study the local and global Galois cohomology of Abelian varieties. Just before arriving in Paris, I saw that, over a p-adic field k, there is a natural pairing between the group of torsors of an elliptic curve over k and the group of k-rational points on the curve, with values in the Brauer group of k. By local class field theory that Brauer group is canonically isomorphic to $\mathbb{Q}/\mathbb{Z}$, and I was able to prove that the pairing gives a perfect Pontrjagin duality between the compact profinite group of points and the discrete group of torsors. Lang, who was also in Paris that fall, insisted that I should extend the result to Abelian varieties and taught me what I needed to know in order to do so.

Having such success with the local theory I optimistically started thinking about the global situation. My optimism was unfounded. It soon became clear that the cohomological picture would be very nice if the group which is now called the Tate-Shafarevich group and denoted by Ш, is finite, and would be a mess if it is not. It also became clear that the global cohomology just gave a very general way of looking at the classical theory of descent and that the finiteness question was related to the effectiveness of the descent procedure. My naive hope that the cohomological machinery might enable me to find a proof of the finiteness of Ш gradually faded. In fact, it was not until almost 30 years later, with the addition of completely new methods by Thaine, Rubin, Kolyvagin, Gross, Zagier, that the finiteness was proved in special situations. The general case is still a complete mystery. Michael Artin has remarked that the question whether Ш is finite is a special case of a more general question: Is the Brauer group of every scheme proper over Spec($\mathbb{Z}$) finite?

From Grothendieck I learned that the most important thing in Galois cohomology is the cohomology of the absolute Galois group G_k of a field k, rather than that of the finite Galois groups of which G_k is the projective limit, and that G_k often has a finite cohomological dimension. For example for global and local fields k the cohomological dimension of G_k is 2 (with a grain of salt involving the real field and the prime 2). Thus the salient fact about the higher dimensional groups of class field theory which I had worked so hard to determine earlier is that they die under inflation and are trivial for G_k.

During that year in Paris I also tried to learn more about the structure of the group of points $E(k)$ of an elliptic curve E over a p-adic field k. Results of Dieudonné and Lazard on formal groups gave information about the subgroup $E_1(k)$ of points reducing to 0 over the residue field. In case of good ordinary reduction, the result was especially striking. I regret never having published it, but I did think to send the statement as a challenge to Dwork, who I saw as the world's greatest p-adic analyst. Almost by return mail he sent a proof completely different from mine, for the Legendre curve, using the hypergeometric function $F(\frac{1}{2}, \frac{1}{2}, 1, \lambda)$. He saw aspects of the situation which I had not dreamed of.

That sabbatical year in Paris which was so crucial to my career ended with a complete surprise, an invitation to collaborate with Bourbaki. This pleased me very

Barry Mazur (*right*) and John Tate (*left*)

much, for I admired his work. By the next train I went to the little village of Pelvoux in the French Alps to join his collaborators at their June congress. Working with them was a unique privilege.

A year later, still thinking about the $E(k)$ for p-adic k, I was thrilled to realize that some series expansions occurring in the classical theory of theta functions, which involve only integer coefficients, make sense over any field k complete with respect to an absolute value function $x \mapsto |x|$, and that using them one can construct, for each $q \in k$ such that $0 < |q| < 1$, an elliptic curve E_q defined over k, together with a k-analytic homomorphism $k^* = \mathbb{G}_m(k) \to E_q(k)$ which is surjective, with kernel $q^{\mathbb{Z}}$. The j invariant of E_q is given by the classical Fourier expansion of the j-function:

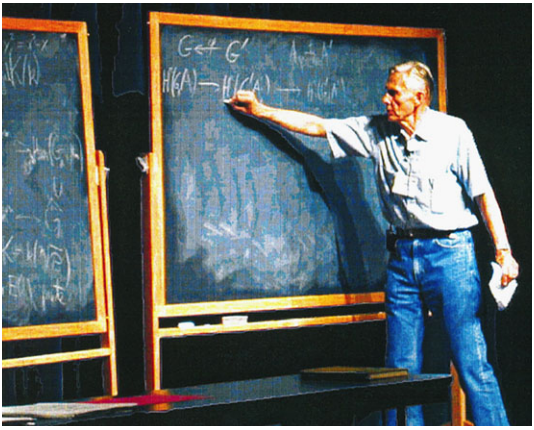
$$j = q^{-1} + 744 + 196884\,q + 21493760\,q^2 + \cdots$$

For $k = \mathbb{C}$ or $\mathbb{R}$, every elliptic curve over k can be obtained in this way, but for k nonarchimedean, e.g. p-adic, as the displayed formula shows, the j-invariant of E_q satisfies $|j| = |q|^{-1} > 1$, and $q = j^{-1} + 744j^{-2} + 750420j^{-3} + \cdots$ is uniquely determined by j. Moreover, every elliptic curve over k with such a non-integral j-invariant is a quadratic twist of the curve E_q which has the same j.

1959–1989

In fall 1959 I was given tenure at Harvard. I continued teaching at Harvard for the next thirty years, except for three more sabbatical years in Paris visiting the I.H.E.S. and/or Orsay and a half year visit to Berkeley. My research owes much to direct contact with colleagues in Cambridge and Paris. I think especially of Michael Artin, A. Grothendieck, David Mumford, J.-P. Serre and, later, Benedict Gross and Barry Mazur. Above all, an extensive postal correspondence with Serre was invaluable in helping me get my ideas straight.

Teaching at all levels, from calculus classes to mentoring Ph.D. students, has always been a pleasure for me. A good way to learn a topic is to teach it. On the



John Tate, Park City, 2000

other hand, giving graduate courses and talking with Ph.D. students is often a source of new ideas and cannot be separated from research efforts, contrary to the thinking of those who promote effort reporting. I have had more than 40 Ph.D. students, several of whom have become good friends.

1989–2009

In the late 1980's the offer of a Chair at the University of Texas at Austin prompted me to think about trying something new. This was appealing, for various reasons, and I felt fortunate to have such an opportunity. The U.T. Math. Department and the University itself were huge compared with what I had known, but size was the least of it. Nearly everything was new. I taught there for almost 20 years before retiring. During that time, although there was a great disparity among positions, the department was run democratically, and the atmosphere was very positive. I enjoyed being part of it and working with my colleagues in number theory, Vaaler, Villegas, and Voloch, from whom I learned a lot.

It was a time of change in Austin as the city grew rapidly, doubling in size in twenty years. The general spirit of the university seemed to be evolving along with the skyline of its home city. One sign of this evolution, to my delight, touched me directly, in connection with the Abel award. The 300 foot tower on the main building of the university, which is centrally located on high ground, is visible from almost anywhere in the city. By tradition it is illuminated in UT's distinctive burnt-orange color to signal an athletic victory. Under University President Bill Powers, academic achievement is honored in the same way.

2012

Looking back on my research, I take satisfaction in having proved or helped prove several important theorems, no one of which is so great that it stands out above the others. Some of my best ideas have been conjectures on which I could make little

(*From left to right*) Steve Leslie (Provost), Mary Ann Rankin (Dean), John Tate, President Bill Powers, University of Texas at Austin, 2010

progress, though others have. I liked trying to imagine what should be true, not only by making conjectures of my own, but in trying to generalize and clarify those of others, such as those of Birch and Swinnerton-Dyer, and of Stark.

Although I was born on a Friday the thirteenth, I feel I've had more than my share of good fortune. Two specific examples are my meeting, by pure chance, Emil Artin, the mentor from whom I learned so much, and my having had the opportunity to spend an especially valuable year 1958–1959 in Paris, thanks to Harvard's policy of offering junior sabbaticals. I did not write easily, and am thankful that my colleagues included unpublished results of mine in their papers and books, crediting me fully. More generally, I have had the support of family in spite of my obsession with mathematics, and have enjoyed good health to date. Finally, I feel fortunate to have been inclined toward mathematics, a field in which cooperation is so much more common than competition, and in which I could earn a living by doing what I most liked to do.