Memorial Article for John Tate

Edited by Barry Mazur and Kenneth A. Ribet

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Barry Mazur and Ken Ribet

John Tate was born on March 13, 1925, and died on October 16, 2019. His mathematical work was fundamental in forming the shape of modern number theory; we refer the reader to the book [HP14] for a short curriculum vitae compiled by H. Holden and R. Piene, and to [Mil17] and [Col17] for detailed discussions of Tate's work.

John supervised 41 graduate students between 1958 and 1998. The Mathematics Genealogy Project reports currently that he has 772 mathematical descendants. This statistic only hints at John's contribution as a mentor: John influenced the research of generations of mathematicians who were not formally his students.

Even as John encouraged his students to find their own problems, he was always available to hear about their work, and about the work of his colleagues. He elevated his students and postdocs by treating them as colleagues, even when they were only starting out on their research. As was discussed in Harvard University's "Minutes" in memory of John, an initial meeting with a graduate student who aspired to work with Tate

> ...might end with John complaining that he didn't know enough to help. A few days later, he would pass this student in the hall and say that he had been thinking a bit more about it, and perhaps understood what they had been telling him. This would be followed by a complete explanation, in John's characteristically lucid style. He would also encourage his students to communicate with each other, to work together. This extended to the sport that John loved: basketball. One year (1977) an entire basketball team consisting of John's PhD students graduated together (they signed a basketball as a gift to John).

We all know of the perfection he demanded of his own writings—and those of us who coauthored papers with John have experienced this most keenly. John would continue to improve and reflect on his writings and letters as they were freely circulated (sometimes for decades).

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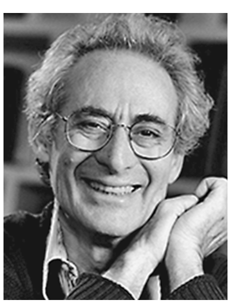
Young John Tate (undated).

His letters represent a vital contribution to our subject. The 1500 pages of his correspondence with Jean-Pierre Serre (which spans half a century) exhibit the force and emotional exuberance of their authors' new discoveries—discoveries that shaped much of modern number theory. In addition, those letters present a portrait of a close friendship.

John Tate had the precious talent of being able to enjoy personal and mathematical friendships with a large number of people. Mathematically, John felt comfortable discussing his own ideas even when they were far from fully formed; he also enjoyed serving as a sounding board for the ideas of others that were quite far from mature. John was consistently modest and generous in his mathematical conversations.

Innumerable mathematicians cherish personal memories of John's kindness, his intense love of mathematics and his way of inspiring all of us to aim high. We thank the editors of the *Notices of the AMS* for giving us the opportunity to present some personal reflections of a few of our colleagues.



Barry Mazur

Kenneth A. Ribet

Dustin Clausen

When I was a child, John Tate was just my "Texan" grandfather, with his slow way of talking and his easy, slightly mischievous smile. But when I started to get interested in math as a 15-year-old, I remembered that he was also a mathematician. Actually, I thought I had a proof that there were no odd perfect numbers, and I sent it to him. He pointed out my mistake: I had read that σ was a multiplicative function and thought that this meant $\sigma(mn) = \sigma(m)\sigma(n)$ for all m and n, but actually it only means that when m and n are relatively prime. He explained that what I had really proved was that there were no *squarefree* odd perfect numbers, a result he said was "at the level of a good undergraduate exercise in elementary number theory." He told me that instead of trying to tackle unsolved problems I should just focus on learning math, and to help me along he sent me a set of exercises—the first of many—and two wonderful books, Davenport's *The Higher Arithmetic* and Hardy and Wright's classic *An Introduction to the Theory of Numbers*.

This first mathematical interaction with him was typical. He was encouraging, generous, and keen to challenge me, but at the same time very grounded and realistic, making sure I wasn't getting ahead of myself. The exercise sets he sent me over the next two years, composed ad hoc I believe, were excellent introductions to some beautiful mathematical ideas which he valued, and many of them are permanently engraved in me, such as the one proving the irreducibility of cylcotomic polynomials via reduction mod p and Frobenius, and the one proving the fundamental theorem of algebra using a homotopy argument: "if a man walks around a flagpole with a dog on a leash, then the dog also walks around the flagpole." Thanks to his guidance, I arrived at college well equipped to further study mathematics. (This is an understatement, but he also taught me to make understatements.)

As I progressed through my mathematical life I of course kept up contact with him, and he always wanted to hear what I was doing. He liked to complain that he was getting too old and slow to follow current developments, but what really shone through was not this oft-expressed negativity but rather its underlying cause, which is itself a thing of positivity and beauty: his remarkable love of mathematics and his true and simple desire to understand. There was also a corresponding positivity in how he viewed others: he was clearly in awe of and had great respect for the many mathematicians of all generations, up through and even beyond mine, who contributed to the

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development of mathematics and the unraveling of its mysteries. I'm forever impressed that he seemed to have completely preserved his childlike sense of wonder at the marvels of mathematics, and indeed of the whole mathematical enterprise, despite being himself a leading figure in that enterprise. He was a fantastic mentor, an exceptional role model, and a wonderfully loving grandfather.

Dustin Clausen

John Coates

I first met John Tate when he came to Cambridge (UK) fairly early in 1969 for a stay of about a week. At the time, I had just finished my doctoral thesis, and was looking for a new direction of research which was somehow related to the arithmetic of elliptic curves, and more specifically to the conjecture of Birch and Swinnerton-Dyer. I already knew a little of Tate's enormous reputation as an arithmetic geometer. His celebrated doctoral thesis had at last been published in the Proceedings of the Brighton conference [CF10], and his masterful account of global class field theory was given in the same volume. In addition, his 1966 Bourbaki seminar [Tat95] not only established all of the basic functorial properties of the conjecture of Birch and Swinnerton-Dyer for abelian varieties over number fields, but it also described joint work he had done with M. Artin which went a remarkably long way towards proving the function field analogue of this conjecture. Thus it was with some trepidation that I attended his first lecture in Cambridge, in which he explained the conjecture he had recently formulated with Birch asserting that the tame kernel of any totally real number field is finite, and proposing an exact formula for its order in terms of the value of the zeta function of the field at the point $s = -1$. Let J be any field, and write $J^\times$ for the multiplicative group of J. The Milnor K_2 of J is then defined by

$$K_2J = (J^\times \otimes_{\mathbf{Z}} J^\times)/W,$$

where W is the subgroup of the tensor product generated by all elements $a \otimes b$ with $a + b = 1$. If v is any discrete valuation of J with residue field j_v, the formula $\lambda_v(a, b)$ = residue class of $(-1)^{v(a)v(b)}a^{v(b)}/b^{v(a)}$ defines a homomorphism $\lambda_v : K_2J_v \to j_v^\times$ called *the tame symbol* at v. Suppose now that J is a finite extension of $\mathbf{Q}$, and let $\phi_J : K_2J \to \prod_v j_v^\times$ be the map given by the tame symbols at all finite places v of J. The tame kernel R_2J is then defined to be the kernel of the map ϕ_J. Let $\zeta(J, s)$ denote the complex zeta function of J. When J is totally real, old work of Klingen and Siegel had shown that $\zeta(J, -n)$ is a nonzero rational number for all odd positive integers n, but nothing was known about the arithmetic significance of these special values prior to the conjecture of Birch and Tate. Always assuming that J is totally real, Birch and Tate had conjectured that $R_2(J)$ is finite, and that its order is given by the absolute value of $w_2(J)\zeta(J, -1)$, where $w_2(J)$ denotes the largest integer m such that the Galois group of the extension of J obtained by adjoining the mth root of unity is annihilated by 2. I still vividly remember how clear and down-to-earth Tate's first lecture explaining this conjecture was, as were also the remaining two lectures he gave on his visit. Moreover, when one posed questions to him after the lectures, his answers were always very precise and illuminating.

When I arrived in Cambridge (MA) in September 1969 on a Benjamin Pierce postdoctoral position, Tate had just returned from his sabbatical in Paris, and was starting his term as Head of the Harvard mathematics department. The department at that time was located in very cramped quarters at 2 Divinity Avenue, above the Harvard-Yenching Library. In fact, the physical smallness of the location turned out to be ideal for both postdocs and graduate students because every day one met informally many of the very distinguished senior faculty in the tiny coffee room or corridors between the offices. Despite being very busy with his duties as Head of the department, Tate ran a weekly seminar throughout the academic year 1969–1970 about his conjecture with Birch, and it turned out to be a golden opportunity for me. His seminar lectures were a remarkable mixture of abstract ideas, always illustrated by subtle numerical examples. In addition, he was always very open to answering questions and having discussions in his large office at the top of the central stairs in 2 Divinity Avenue. Whenever one came into his office with a mathematical question, he would take one over to a small

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blackboard where he would carefully explain his answer using the blackboard, or ask one to explain what one was saying on the blackboard. I found this method of discussion so effective that I always used it later with my own graduate students. Sometime in the spring of 1970, Tate found a proof of the analogue of the Birch–Tate conjecture for curves in one variable over a finite field, which, in particular, made crucial use of the theorem of Weil asserting that the zeta function of such a curve could be realized as the characteristic polynomial of the Frobenius automorphism acting on the Tate module of the curve, divided by a simple pole term. At the end of this memorable lecture, Tate made the comment that he believed that Iwasawa had recently proven some analogue of Weil's theorem for the field obtained by adjoining all p-power roots of unity to the rational field $\mathbf{Q}$, and speculated whether this might be useful in attacking his conjecture with Birch for totally real number fields. At this point, I knew nothing about Iwasawa's work. However, his paper [Iwa69] had just been published, and I spent the whole weekend at home reading it. I was absolutely delighted to find that, when the totally real field J is a finite abelian extension of $\mathbf{Q}$, the mysterious $w_2(J)$ factor appearing in the conjecture of Birch and Tate had a simple explanation in terms of Iwasawa's description of the pole term occurring in his construction in [Iwa69] of the p-adic analogue of the Leopoldt–Kubota p-adic zeta function for the field J using Stickelberger ideals. I still remember my immense pleasure at explaining this to Tate in his office on the next Monday morning, and his warm encouragement to pursue the whole question further. However, to do this, it was clear that I had to learn much more of the background of Iwasawa's work. One of the very good conditions of the Benjamin Pierce position was that one could teach a graduate course each year as part of one's teaching duty, and Tate suggested that the best way to learn about Iwasawa's work was to teach a course on it in the coming Fall term. I followed his advice, and gradually began to feel at home with Iwasawa's ideas, greatly aided by some beautiful lecture notes which Iwasawa had kindly sent me. Moreover, Tate himself continued in his seminar to do fundamental work relating K_2 to Iwasawa theory [Tat73]. In addition, Tate invited his former doctoral student Steve Lichtenbaum to give a seminar talk in Harvard, and I learnt then that Lichtenbaum had independently realized the connexion of the Birch–Tate conjecture with Iwasawa's analogue of the Jacobian, and also formulated some striking generalizations of the conjecture to the values of the zeta function of a totally real number field at all odd negative integers involving Quillen's higher K-groups of the ring of integers of the number field. However, it was only some years later, when the deep work of Mazur–Wiles (for totally real abelian fields) and Wiles (for all totally real number fields) established Iwasawa's analogue of the Jacobian in general, that one finally was able to almost prove the original conjecture of Birch and Tate (the 2-part of the conjecture is still unknown).

All too soon, my three-year post at Harvard was finished, and sadly I never was in the same department as Tate for long periods after that, except for a long visit he made to Orsay in Paris around 1980. My time at Harvard led me into mathematical problems and ideas which I have spent the rest of my life working on. While I benefited greatly from discussions with Barry Mazur, and also Ken Ribet and Mike Razar who were both graduate students of Tate, it was above all many mathematical conversations with Tate himself, as well as his lectures, which profoundly influenced me. I often wondered afterwards how much of Tate's ability to mix abstract ideas with concrete numerical examples came from his teacher Emil Artin. As I left Harvard, I confessed to Tate again my desire to establish some analogue of Iwasawa's analogue of the Jacobian for elliptic curves with complex multiplication, and to use it to prove some cases of the conjecture of Birch and Swinnerton-Dyer, in a similar spirit to his work with M. Artin on the function field analogue. He was, as always, quietly encouraging, and happily Wiles and I succeeded in doing this several years later. After leaving Harvard, I occasionally had the great pleasure of receiving a handwritten letter from Tate (these were the days before the internet). The one I remember above all was sent to me in Cambridge (UK) in 1976, in which he asked me to keep an eye on a very gifted young Harvard undergraduate called Robert Coleman who had come to Cambridge for a year to do Part III of the Mathematical Tripos. As always, I quickly realised how right Tate was in his judgement of Coleman.

John H. Coates

Benedict H. Gross

John Tate was a wonderful graduate advisor, but he had some well-developed defense mechanisms to head off

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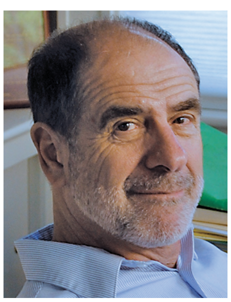
potential students. When I arrived at Harvard, John already had five students working with him and was reluctant to take on any more. "Why are you asking me these questions about Galois cohomology? I don't know anything about the subject—go ask Barry!" However, once we started to work together, he was available at any time (including off-hour visits to his home) to talk. His enthusiasm for number theory was contagious, and he made his students feel that we were already contributing to it. One day he showed me how the q-parametrization of elliptic curves worked perfectly over the reals, giving a bijection between the isomorphism classes of real elliptic curves and the set of real numbers q with $0 < |q| < 1$. Beautiful (and very useful too). I would always leave his office thinking—this is the way mathematics should be done!

I got to know John better in the fall of 1980, when we were both visiting Paris. He gave a course on Stark's conjectures at Orsay, and I was thinking about p-adic analogs of these conjectures, so we met frequently. One day we were heading out to Bures to speak with Deligne, and missed our train at Montparnasse. John suggested that we have a coffee at a place nearby. When we arrived at the café, he insisted that we sit at a certain table. When I asked why, he replied that he had been sitting at that table when he found the argument in his paper [Tat66]. The isogeny theorem was one of John's favorites, and he returned to the spot whenever he was in the neighborhood, hoping that lightning would strike again.

Mathematical lightning struck John many times in his career—his discoveries have great depth and clarity. He was always extremely modest about his contributions. We spent the summer of 2009 as senior scholars at the Park City Math Institute, and I had the chance to introduce John for a talk he gave to an audience consisting largely of high school teachers and undergraduates. His title was "The L-series of Euler and Dirichlet." I told the audience that just as we study the ideas of Euler and Dirichlet today, people would study John Tate's work far into the future. John spent the first ten minutes of his talk protesting this comparison: "Dick, that's just ridiculous!"

John was a kind and generous man. Those of us who were lucky to study with him will never forget it.



Benedict H. Gross

Jonathan Lubin

I entered the Harvard graduate program in 1957, but John Tate was visiting in France that year, so my first contact with him was in 1958. From then on, he gave a string of courses and seminars at the beginning research level: there were at least two in the arithmetic of elliptic curves, one in class field theory, and an informal seminar on group cohomology. Whenever he lectured, he would throw out a shower of citations to papers that dealt with matters related to the topic that he was lecturing on. This and the clarity of his lecturing attracted a large graduate-student audience. Some who were there with me in 1958 included Leonard Evens, Andy Ogg, Steve Shatz, Judith Hirschfield Obermayer, and Steve Lichtenbaum.

Like many of the other graduate students, I found Tate's teaching inspiring enough to give me hope that I might do a thesis under his direction. My memory is clear that I went into his office one afternoon and asked whether he could suggest something to work on in class field theory, and that he responded, "There's nothing I would like better than to know a good problem in class field theory." Ironic, in light of the fact that my eventual thesis led to John's and my paper on that subject.

John was not the kind of advisor who would assign a problem to a student and tell them how to attack it. In my case at least, he would instead seize on an interesting fact I had noticed, and use that as an occasion for putting it in broader context, which I could use for further exploration. That was his way of guiding a student's research: opening doors, but not pushing anybody through.

He seemed always to be eager to impart mathematical insight and information. In 1962 I started teaching at Bowdoin College and working on my thesis, getting partial results that I reported to John by post. My office-mate there in Brunswick was a new PhD from another university, who saw a letter from Tate on my desk and read it. He told me that there was more mathematics in that one letter than he had gotten from his own advisor in all the time he had been working on his research.

John was open and welcoming, did not stand on ceremony. In 1968–1969, I was visiting the University of Paris. I had found myself an apartment that happened to be directly across Rue de Verneuil from the place where John and Karin Tate were staying. When I wanted to discuss some mathematics with him, I would always call and ask whether I might stop over to talk. He expressed surprise that I would not just drop in without invitation.

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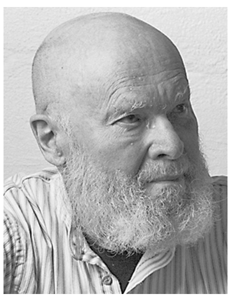
John Tate was not jolly, not a jokester, not the sort of person you would go out drinking with. But he was easy-going, seemed to put people at ease, and had a welcoming personality. I can't imagine anyone disliking him, and in fact he inspired not only universal respect for his mathematics, but universal warmth toward him as a person.

In later years, John and Carol Tate would occasionally make it to Pasadena, to the annual summer party that my husband and I give. When the Tates moved back to New England, I was glad that I was able to visit them once a year from our summer place in Maine. We saw them in August 2019 for lunch; John had already had a number of serious health problems, but even though his previous vigor was gone, he was speaking of going back to France. But that was not to be, and I will miss him very much.



Jonathan Lubin

Bernadette Perrin-Riou

Les concepts et résultats

> *Hauteur de Néron-Tate, module de Tate, groupe de Barsotti-Tate, loi de groupe formel de Lubin-Tate, théorème de Serre-Tate, groupe de Tate-Shafarevich, algorithme de Tate, décomposition de Hodge-Tate, conjecture de Mazur-Tate-Teitelbaum*

font partie de ma culture mathématique et m'ont accompagnée tout au long de ces années.

Les articles de John Tate sont une référence. D'autres ont mieux expliqué que je ne pourrais le faire leur intérêt mathématique. Je voudrais juste insister sur la manière dont ses articles sont écrits. Que de fois ai-je dit: "c'est Tate qui l'a écrit, donc il ne peut pas y avoir d'erreur !"

Il appréciait aussi les mathématiques "concrètes" et les formules explicites ; son algorithme concernant la mauvaise réduction des courbes elliptiques a pu être implémenté tel quel dans Pari/GP et on peut trouver dans sa correspondance des programmes dans ce logiciel. En relisant son rapport sur ma thèse d'état, j'ai été amusée de voir qu'il signalait comme un plus le fait qu'il y avait un algorithme pour calculer les hauteurs p-adiques et des exemples numériques.

Une anecdote: quand j'ai visité Harvard en 1983, nous cherchions à acheter une voiture. Il se trouve que John Tate est passé alors que nous étions dans la rue en négociation avec un vendeur. Quand je lui ai dit qu'avec le contrôle des changes avec la France, nous aurions du mal à réunir la somme dans le délai demandé (c'est-à-dire dans les trois jours . . .), il nous a immédiatement proposé de nous prêter la somme le jour même. Ce qui nous a permis de visiter la région avec notre fils de 2 ans et de profiter de l'été indien.

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Bernadette Perrin-Riou

V. Kumar Murty

I met John when I arrived as a graduate student at Harvard in 1977. Personally, he was always friendly, unassuming and approachable, though it should be quite understandable that a new graduate student would be totally in awe of this mathematical icon! In the weekly number theory seminar, Barry Mazur and John would be seated in the front row and graduate students used to sit further back. In my first year, the topic was automorphic forms and the adelic generalization of Hecke theory. The running joke amongst the graduate students who were struggling to follow the talks was "What the Hecke is going on?" When I started to discuss serious mathematics with John, I was struck by two things that have remained with me all these years. The first is that he treated even a beginning graduate student as a colleague. This meant that he gave the same level of respect, but also held us to a high standard. He wasn't there to hold our hand, but to engage us in a serious, and sometimes blunt, discussion in which we had to defend our ideas, and in the process, he helped us to more clearly shape those ideas and move forward. I don't think this approach suited everyone, but it was good for me. At the same time, he had his own way of being very

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encouraging. I met him weekly to report on what I had been doing, and in one such meeting, said that I was studying a paper of Weil on a proposed "counterexample" to the Hodge conjecture, but that I had nothing to report. He immediately responded by saying something like studying a paper is not "nothing," and for the rest of the meeting, he asked me to explain what I had read, even though he was totally familiar with the contents. I often remember this when I am working with my own students, and take the same approach. The other thing that impressed me was his approach to studying interesting problems, and not backing off because conventional wisdom said that a particular problem was difficult. It seems to me that he just pursued the mathematics wherever it led him. An example is the way the Sato-Tate conjecture is formulated almost as an afterthought following his fundamental conjectures on Galois invariants in the etale cohomology of varieties. I remember a conversation with him where I said that I had an idea but didn't feel it could work because a consequence of this idea would solve a case of one of his conjectures that had been open for some time. He didn't say anything but his expression was of incredulity as if to say "that's exactly when you pursue an idea." I had limited contact with John after graduating, but whenever we met he was always keen to hear what I was up to. The last time I met him was in October 2017 when I came to Harvard to give a number theory seminar talk. He was already over 90 but sat in the front row and seemed to be paying keen attention, and expressed appreciation afterwards. John was an extraordinary and unusual mathematician, perhaps a kind of Gauss who didn't publish much, but whose every paper has had profound effect. I consider myself quite fortunate to be counted amongst his students.

V. Kumar Murty

Karen Acquista

It's difficult to overstate John Tate's influence on many of the mathematicians that I admire and branches of mathematics that I find exciting. Because he was the advisor of my advisor Steve Lichtenbaum, my mathematical perspective is deeply influenced by John Tate. When Steve introduced me to John at a conference, I was starstruck—after all, this was the person who had written *Class Field Theory* with Emil Artin [AT09], a book I had spent months poring over in grad school! But on top of all that, he was genuinely nice, even to tongue-tied grad students. Invariably polite and curious, he seemed to have the ability to make the people around him feel comfortable.

My most memorable personal encounter with John Tate happened shortly after I graduated in 2005. I had a postdoc at Boston University, and he was visiting Harvard that year. Early in the semester, I was slated to give a talk at our local number theory seminar. When I walked into the basement seminar room, I almost fell over in shock—John Tate was sitting in the audience! I remember feeling a little awkward, as there were Tate cohomology groups, Tate twists, and other Tate-influenced objects in my talk. When it was over, I thanked him for coming, and explained that it was very unusual to see anyone from across the river at our seminar. He waved his hand and simply said, "Well, it sounded interesting." Its not something I'll ever forget; it was so unpretentious, generous, and encouraging.

I left academia in 2007, and was extremely lucky to find a nonacademic math research job. Stuck at home during the pandemic, I've spent some time in 2020 reconnecting with my academic research program. Recently, I had the pleasure of rereading Tate's classic article "Relations between K_2 and Galois cohomology" [Tat76], and I was surprised to find that it contained more than one idea that I now consider to be part of my mathematical toolkit. I hadn't remembered *learning* these ideas at all, they felt like a natural part of the landscape. But that's what it's like reading one of Tate's concise, beautifully written articles—you can finish it in a week, but it can impact your way of thinking for years to come.

Although the extraordinary man himself is gone now, and his gentle encouragement will be missed, I have no doubt that future generations of mathematicians will continue to be inspired by his fascinating body of work.

Karen Acquista

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Stephen Lichtenbaum

When I was a junior at Harvard in 1958 I wanted to take a course in algebraic number theory, but such a course was not being offered at Harvard that academic year. However, Harvard did have a course then called Mathematics 60 Undergraduate Reading in Mathematics, and I had a friend who had taken such a course the year before supervised by George Mackey, so I decided to see if I could enroll in Mathematics 60 to study algebraic number theory under Tate's guidance. I knocked, a little nervously, on Tate's office door and explained what I wanted to do. He had never supervised a Math 60 before and I think was probably not even aware that there was such a course. But once he determined that I had the necessary background he was very enthusiastic and agreed to tutor me. It was a marvelous course. We met once a week and he gave me notes to read and problems to work out, and then eventually I also wrote my undergraduate thesis (on elliptic curves) under his direction. Tate was visiting the institute for Advanced Study in the Fall term of my senior year, and he even invited me to visit him for a couple of days to discuss mathematics. Of course eventually I stayed at Harvard to do graduate studies, and Tate remained my advisor. We stayed in touch for the next half-century. I very much enjoyed knowing John Tate and I will miss him greatly.

Stephen Lichtenbaum

Cristian D. Popescu

I first met John Tate in Burlington, Vermont, at the AMS Mathfest of August 1995. John attended the meeting as that year's awardee of the Steele Prize for Lifetime Achievement in mathematics. I attended as an unassuming graduate student, giving my very first conference lecture in a special session on Stark's conjectures—a research theme in number theory that had been deeply influenced by John's work during the previous two decades. I gave my lecture (right after Stark opened the proceedings, a tough act to follow), and was getting ready to leave the amphitheater for the break, when I saw John Tate walking towards me. I had failed to notice him in the audience! He introduced himself very casually, showed genuine interest in my results, and asked me a couple of related mathematical questions. I was deeply moved by his interest in my work, as I had learned the subject from his 1984 book on Stark's conjectures [Tat84], and my lecture was just building on the foundations he had laid out and the techniques he and Deligne had developed in the characteristic p case of the conjectures [Tat84, V].

We corresponded via email for the rest of that summer and early fall. He was kind and generous with ideas and suggestions. That email exchange gave me an extra boost of energy and confidence which helped me finish writing my PhD thesis. A few months later, John offered me via email my first job, a postdoctoral position at U. Texas at Austin. Although I replied with an enthusiastic "yes" right away, John insisted that I visit him in Austin before making a decision.

I remember our drive down Congress Avenue during that visit, John talking about mathematics while driving his white Toyota and making a sudden, screeching U-turn, realizing that he had just missed the exit to Iron Works, his favorite BBQ place. I also remember my wondering how a brilliant man like John could possibly be such an erratic driver! I wound up spending three wonderful years in Austin during which John and Carol Tate treated both my wife, Alexandra, and me with true friendship and kindness. At some point during my stay, I had the audacious idea of running a learning seminar on Euler systems (and, implicitly, on Tate–Poitou duality), with John in the audience. Lecturing on Tate–Poitou duality in front of John was an almost religious experience for me—he would catch every mistake I made (and there were many!), and would walk up to the board and give intricate examples and counterexamples on the spot, which was awe-inspiring to me. I came to realize how truly deep his understanding of Galois cohomology was. John had developed part of the subject and many of its applications to number theory in his youth. He was now well into his 70s, yet all the technical subtleties he had discovered as a young man were still crystal clear in his mind. A true master!

I left Austin in August 2000, first for Johns Hopkins U. and later for UC San Diego, to face the world and "stand on my own feet," as John himself put it. However, throughout my career it never felt like I really left Austin, as John remained a mentor and a friend to whom I would always turn for advice and support.

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One of my highest career honors was to give back-to-back lecture series with John at the 2009 IAS–Park City Math. Institute. John lectured on the classical aspects of Stark's conjectures, I on the more modern ones. Our lectures had to be coordinated and we managed to do that during a memorable eleven-hour drive which John and I took together in my brand new BMW from San Diego to Park City, at the end of June 2009. I could tell that John was tempted to share in the driving excitement, and he even explicitly offered to do so somewhere near Las Vegas. However, remembering our Austin rides with John at the wheel, I politely declined. As I was driving and John did not have to concern himself with speed limits in Nevada and Utah, he regaled me with delightful stories of his 1980–1981 lectures in Paris which resulted in his book on Stark's conjectures. As we approached Park City, John went much farther back in time, and talked about his interactions in Princeton with Emil Artin, his "teacher," as he called Artin with great deference, while John was writing his PhD thesis [Tat67]—a landmark document in number theory.

It was an unforgettable eleven-hour lesson on our subject's history for me, painted on the background of the magical Nevada and Utah deserts, which we both admired and regretted not having time to stop and explore.

We started lecturing two days later, John striving for perfection, as usual, and worrying that his lecture notes would never be in good enough shape to be presented or distributed. His notes were always crystal clear, as is the case with all of his published work, but then again, his mathematical writing standards had always been notoriously high.

In 2010, John was awarded the prestigious Abel Prize in mathematics by the King of Norway. As soon as I heard the news, I called Carol, thinking that John was busy on the phone with the media. John was, in fact, sitting next to Carol, in an airport in Colorado, on their way to visit family. Carol put John on the phone, I congratulated him, he thanked me, but immediately added that he did not think that the Abel Prize committee had made the right choice, that there were other mathematicians more deserving of the prize. His modesty rendered me speechless for a few seconds. Although I was extremely tempted to remind him of the many mathematical objects and breakthroughs that bear his name and that shed new light on number theory during the past half a century or so, I did not do that then. In the end, we all had the chance to express our deep appreciation for John Tate's mathematics at the First Abel Conference celebrating his work, held in Minneapolis, his birth place, in January 2011.

I was fortunate to spend the academic year 2015–2016 at Harvard, as a Simons visiting scholar, after John had returned to Harvard as a Professor Emeritus, retired from UT Austin. His presence there, my having the chance to spend more time with him, to go to an occasional concert or a museum with him and Carol, made my stay in Cambridge very special. Although his interest in mathematics was still very much present, he had started avoiding technical mathematical conversations by then. I remember bringing up some calculations with adjoints of Iwasawa modules which I was doing at the time, and he said "I never really understood adjoints," trying to take the conversation in a different direction. "But you discovered adjoints, John!," I said. "Really? Who says that?" "Iwasawa says it, right here, in writing. . . ." "Then, I might have had something to do with it. . . ." The game of bridge was very much on his mind at that time, and he was getting good at it, taking lessons and playing with his neighbors in his retirement community.

I saw John Tate for the last time in May 2017, over dinner with Karl Rubin and Carol at Toscano, one of John's favorite Italian restaurants in Harvard Square. John was cheerful that evening. He treated us all, and enjoyed his usual Martini. We did not discuss mathematics, we talked about food, family, art events in Boston, and John's increasing popularity with the bridge players at the retirement community. After dinner, we all walked in Harvard Square and stopped for a while to admire a tiny magnolia tree that had just begun blooming. We said goodbye in the balmy evening.

That is my last image of John—a kind and generous man, a giant mathematician in his twilight years, standing next to a tiny, blooming magnolia tree in Harvard Square. He left behind beautiful mathematics, for the delight and wonder of generations to come.

Cristian D. Popescu

Joseph H. Silverman

John Tate was an inspiration to generations of mathematicians, for the breadth and depth and originality of his

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mathematics, and for his mentorship and collegiality with his students and colleagues. I was fortunate to be one of his students at Harvard during the late 1970s, and to be able to see him frequently over the subsequent years.

As it happened, John spent my fourth year of graduate school on sabbatical in Paris. These being the preinternet days, communication was by trans-Atlantic snail mail, so I dutifully sent him periodic updates on my work. In return, he sent me a mimeographed preprint of a talk by David Masser whose title was, as it happens, the working title of my thesis! After briefly panicking, I realized that Masser's results were orthogonal to mine, so all was well.

I defended my thesis in December 1981, and the spring was devoted to writing up results and working on new research. This is when I got to know John best as a mathematician, since we started meeting weekly to discuss problems related to the variation of the canonical (aka Néron–Tate) height in families. John and I had each previously proved limit formulas for 1-dimensional families, with his being more precise and mine being somewhat more general, but we struggled to prove anything analogous for higher dimensional families. I well recall staring at the blackboard one day when John said "I wish that I understood algebraic geometry." At the time I was flabbergasted, since his knowledge of algebraic geometry was so much greater than mine was, or indeed, has ever been. But in later years, my interpretation became that John's statement was a mix of his natural modesty and an unspoken final four words that might have been "like David Mumford does." We ultimately discovered that the sort of limit formula that we wanted could not exist, which was disappointing; but a further analysis of the counterexamples yielded a height comparison formula that subsequently turned out to be quite useful. This was a great lesson to learn, that even an apparent failure can often be put to good use.

A decade later I was a professor teaching undergraduate abstract algebra and decided to spend half the semester covering Tate's famous Haverford lecture notes on elliptic curves. These notes were one of the primary sources for anyone wanting to study the subject, and they were passed from student to student via increasingly illegible mimeograph or Xerox copies. (Again, for you younger readers, this was the preinternet world!) So I decided to retype John's beautiful lectures using TEX, with some added material and exercises. After doing this, I asked John if he would be amenable to my adding a couple more chapters and seeing if there was any interest in publishing the notes as a textbook. He was very enthusiastic, and even offered to write an appendix giving an elementary proof of Bézout's theorem. As the publisher's deadline approached, John was in Texas and started faxing me handwritten material for the appendix, which I picked up at a local store for dollars per page. (See [Sil17] for more about our experiences writing *Rational Points on Elliptic Curves*.)

In his quiet way, John was a very competitive person, and he retained his competitive spirit up to the end of his life. After he and Carol moved to a retirement community in Waltham, John started playing bridge on a regular basis, and he invited me once to be his partner in a weekly duplicate game when his regular partner was away. This was a lot of fun, but I was quite rusty and we finished last. John was very gracious about it, but it was clear that he preferred not be in that position. I hoped to have an opportunity to make amends, but unfortunately this was shortly before he became so ill.

It was a privilege for the mathematical community to have John Tate amongst us for so many years, yet a source of sorrow that he is gone. He will be sorely missed, even as we honor his many achievements as a mathematician and as a person.

Joseph H. Silverman

Karen Uhlenbeck

John Tate came to the mathematics department at the University of Texas in 1989, two years after I was hired, the same year that Dan Freed arrived at the department. John and I were hired as Sid Richardson Chairs on the endowment money provided to the mathematics department by Peter O'Donnell. Despite dire predictions in the mathematics community at the outcome over building a department in this way, the years that John spent at Texas were the years in which the department improved noticeably (without in fact increasing in size very much), rising in the rankings of mathematics departments nationwide. The legacy of R. L. Moore meant there was already a strong topology group centered around Cameron Gordon. John successfully formed a number theory group; we hired in geometry, mathematical physics, and analysis; and, most difficult

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of all, the department managed to acquire representation in applied mathematics. Efraim Armendariz was the chairman for most of this period. John, Efraim, and I were able to work well with the rest of the department to keep things on an upward trajectory. John was a wonderful elder statesman: dignified, humorous, wise, and approachable. Not once did he say "At Harvard we. . . ."

These were good years in the department for all of us. Department meetings were by and large congenial, we had regular departmental picnics and a department band, and the number of research seminars multiplied. It was fortuitous that the number theory group had offices on the ninth floor along with Bob Williams, Dan Freed, and me, so we got to know each other very well.

Although you would have thought that we had little mathematical interaction, in those early years, John, Dan, and I all attended the same seminar that tried to make sense of the physics coming out of conformal field theory and string theory. Moreover, I was once brave enough to sit through a course that John taught on elliptic curves. It was well into the course that I realized I thought I was understanding things only because my knowledge base did not go beyond characteristic zero.

John became my role model as I grew older. It was not only his presence in departmental life that I tried to emulate. It is undeniable that we older mathematicians cannot lecture the way we did as spring chickens. It takes more preparation, more notes, and inevitable confusion from time to time. Also, the level of the classes at UT must be different from the level at Harvard. John was concise, clear, and good-natured, and refused to get flustered. It was a real pleasure to sit in his class, and his example served me well.

Bob Williams and I became friendly with John and Carol as couples. Despite our differences in background and experience, we were well matched in age and opinion. We alternated dinners at each other's places. As I recall, John and I both broke bones in biking accidents. When I consulted Dan, he reminded me how well John adapted quickly into the local culture: swimming in Barton Springs, lunching at Las Manitas, becoming a UT basketball fan, and sporting bolo ties.

It is hard to describe the vibrant Austin scene which drew us all to Austin in the late 80s and early 90s. Austin is the capital of Texas, and there was a heady liberalism in the air. Ann Richards became governor (1991–1995) and there was a thriving counterculture scene left over from the 60s. Mention could be made of one of Austin's attractions for all of us: Whole Foods was founded in 1980 in Austin. When the Tates arrived, it consisted of two stores, with the one on Lamar conveniently located for the Tates. That and the Wheatsville Co-Op were an integral part of the local color, where one could buy organic produce and meat in addition to seeing unusual hair, clothing, and body ornaments. Ken Ribet recalls that the Tates checked out Whole Foods and met its founder John Mackey at the recommendation of John Mackey's uncle George Mackey, who had been John's colleague at Harvard. All this and much more added to the intellectual life surrounding a rapidly developing public university. Austin was a very special place.

We were sad to see John and Carol move back to Cambridge. Luckily we were able to visit them once more in May 2019 at their assisted living facility outside Boston. John was still himself and we will all miss him very much.

Karen Uhlenbeck

José Felipe Voloch

John Tate was looking for a change when the opportunity arose for him to take up a Sid Richardson Chair at the University of Texas at Austin (UT). After a trial period, he accepted the chair and moved to Austin in 1990. He and his wife Carol adapted well to life in Austin. They were regulars at Las Manitas, a traditional Tex-Mex cafe, and became friends with the owners, local activists Lidia and Cynthia Perez (alas, their restaurant has since closed). He started the habit of having seminar dinners at the Iron Works, a funky Texas BBQ restaurant. He took to wearing a bolo tie on special occasions. But he and Carol also maintained friendships in the Northeast, and would spend time there often. Tate went into phased retirement in 2006, dividing his time between Cambridge and Austin and fully retired back to Cambridge in 2009.

He was very important for the UT Math Department for the nearly twenty years he was there. A few of us, Bill Schelter, myself, and Fernando Rodríguez Villegas, were fortunate enough to collaborate with him. Fernando and I were hired by UT on his advice and came primarily because of his presence. Together with Jeff Vaaler, we formed the core of Tate's UT number theory group. He also attracted many visitors, postdocs, and graduate students and he became a

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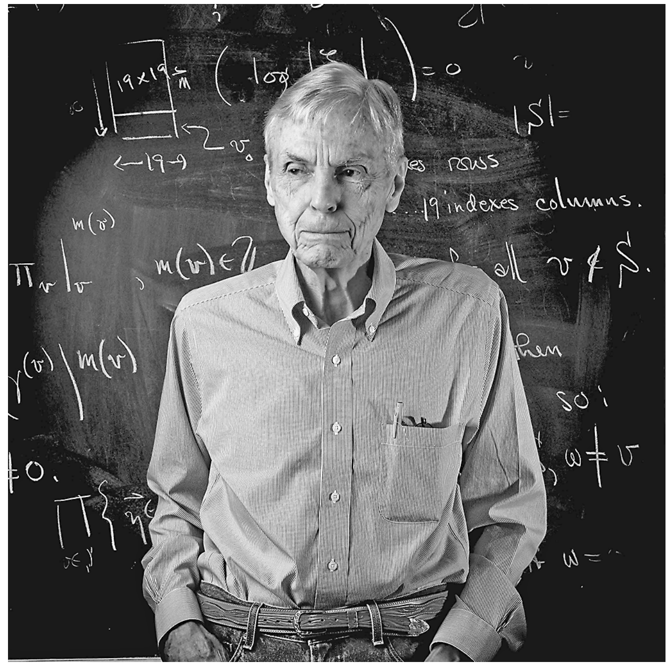
mentor to many of them. He and Carol made everyone feel welcome. Tate only supervised two PhD students at UT but helped many of the other students of the group. His dedication was inspiring. He was in his office from early in the morning until late in the afternoon and the door was always open. He welcomed everyone and was always willing to discuss mathematics. I would often go to his office to tell him what I was thinking about and, just by looking at him, I could tell if I was going in the right direction. A couple of times, after he retired, I found myself almost wandering into his old office only to realize there was someone else there now. John also gave advice, sparingly, but when he did, we listened. He set a very positive example.

What is less well known was his love for calculations, both by hand and by computer. He was fond of telling how once he managed to squeeze literally the last bit out of a program written on an old HP calculator that only allowed fifty steps in its programs. He came of age mathematically in a culture that shunned calculations in favor of abstract thinking and, in his papers, the calculations that went into shaping the theory are often indiscernible. He taught me how to use Pari/GP, which was fairly new at the time. It was clear that he loved to explore number theory with it. This is just one of the many things I've learned from him.

José Felipe Voloch



John Tate in 2010.

Joe Buhler

John Tate's extraordinary impact on mathematics grew out of the depth, exquisiteness, and mathematical fearlessness of his work. This influence was magnified by his personality, which had a curious mixture of modesty, openness, and passion in mathematical as well as social settings. To illustrate some of this, I'll describe some of my early mathematical interactions with him, and then some of our joint hiking adventures.

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The Deligne–Serre theorem asserts that suitable modular forms of weight one give rise to odd two-dimensional representations of the Galois group of the rational numbers. These representations are classified as cyclic, dihedral, tetrahedral, octahedral, or icosahedral according to their image in the projective linear group $\mathbf{PGL}_2(\mathbf{C})$. John gave a series of lectures on this in the mid 1970s, taking care to work through examples in detail. At one point I offered to search for possible conductors of modular forms of weight one that could possibly correspond to odd icosahedral Galois representations, and he immediately sharply upped the ante and asked me if I wanted to work on the problem of actually proving that specific cases of such representations were modular, in the sense that they arose as in the Deligne–Serre theorem. How could I refuse such an offer? All of the other four types of such representations were either well known to be modular, or were in the process of being proved modular by Langlands and Tunnell using Langlands' results on base change for automorphic representations; icosahedral representations seemed completely immune to these techniques.

This effort turned out to require numerous ancillary results as well as novel algorithms and a lot of computation. John was excited by the project, but was especially fascinated by some of the latter.

Like most number theorists, John was fond of well-chosen examples. Although his work in algebraic number theory and arithmetic geometry gave few hints of a computational bent, he was fascinated by the explicit computations sometimes necessary to produce such examples. He bought an early HP hand calculator and would, to receptive friends and colleagues, show off his ability to generate interesting number theory using the device. He was thoroughly fascinated by my description of what the new PDP 11/70 computer in the building was going to have to do in order to find modular icosahedral representations. One example that I remember vividly was when his eyes grew larger as we discussed the problem of solving many systems of linear equations with more than 100 variables over many finite fields. We decided to predict how much time one such linear system would take. Neither of us were very confident of our estimates (I think that he guessed twenty minutes and I guessed one minute). We were happy to find that we were both way off: it took slightly over a second.

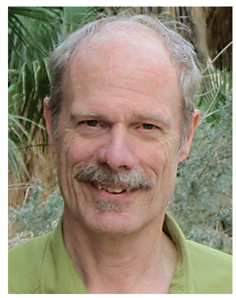
After much effort, all of the algebraic number theory worked, and the linear equations were all solved (which was overwhelming confirmation that the details had been nailed). John seemed absolutely thrilled—I had the feeling that some of this was the natural relief when any graduate student finishes, but a big part of it was the excitement about success on a project for which complete success had been anything but certain. John said that he was secretly delighted that the most onerous part of the work (requiring theory, algorithms, and lots of computational time) involved generalized class groups in sextic fields. At one point late on in this effort it dawned on me that cusp forms had to actually vanish at all cusps (!). John agreed that this was an issue, and handled it deftly by coming in two days later and giving me a short and elegant 10-minute talk on how to think about it that left me wondering why I'd ever even worried about it.

All of my subsequent mathematical conversations with him over the years were marked by his insight, subtle probing, and pure joy in talking about mathematics. Now I'd like to focus here on another way in which we repeatedly connected over a span of more than forty years.

John loved hiking. Our first joint hike was in a lava tube near Mt. St. Helens in Washington (destroyed a few years later in the eruption). Another was a climb up Three Fingered Jack in central Oregon (with Bill Casselman and his mentor Robert Langlands; a satisfactory account of the hike would take much more space than I am allotted). There were other joint hikes at math conferences, desert hikes near San Diego, and, most recently, several walks in Hawaii.

At John's 90th birthday conference I left the dinner table to talk to people before dessert was served. Upon coming back a while later I learned that John had announced to his wife Carol and my wife Danalee that he wanted to do something unrelated to mathematics, that he hadn't ever done before, but he wasn't sure what that should be. Danalee had suggested Hawaii, which John jumped at; amazingly, he had never been there. I was more than a little startled to learn that a nine-day trip to the Big Island of Hawaii was completely set. During that trip several months later, John insisted on swimming in the ocean every day, got up at 2 a.m. to see a meteor shower, tried all of the Hawaiian food and cocktails, and chose the longest or hardest option whenever a decision had to be made during our numerous walks.

In many ways the trip exhibited some of John's traits—intense enjoyment of adventure and the outdoors, a fearless mentality, and a love of companionship—that captured so many aspects of his life, intellectual or otherwise. He was an extraordinarily vivid presence in many people's lives, and I miss him greatly.



Joe Buhler

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Credits

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