Interview with Abel Laureate John Tate

Martin Raussen and Christian Skau

John Tate is the recipient of the 2009 Abel Prize of the Norwegian Academy of Science and Letters. This interview took place on May 25, 2010, prior to the Abel Prize celebration in Oslo, and originally appeared in the September 2010 issue of the *Newsletter of the European Mathematical Society*.

Education

Raussen and Skau: *Professor Tate, you have been selected as this year's Abel Prize Laureate for your decisive and lasting impact on number theory. Before we start to ask you questions, we would like to congratulate you warmly on this achievement. You were born in 1925 in Minneapolis in the United States. Your father was a professor of physics at the University of Minnesota. We guess he had some influence on your attraction to the natural sciences and mathematics. Is that correct?*

Tate: It certainly is. He never pushed me in any way, but on a few occasions he simply explained something to me. I remember once he told me how one could estimate the height of a bridge over a river with a stopwatch, by dropping a rock, explaining that the height in feet is approximately sixteen times the square of the number of seconds it takes until the rock hits the water. Another time he explained Cartesian coordinates and how one could graph an equation and, in particular, how the solution to two simultaneous linear equations is the point where two lines meet. Very rarely, but beautifully, he just explained something to me. He did not have to explain negative numbers—I learned about them from the temperature in the Minnesota winters.

But I have always, in any case, been interested in puzzles and trying to find the answers to questions. My father had several puzzle books. I liked reading them and trying to solve the puzzles. I enjoyed thinking about them, even though I did not often find a solution.

Raussen and Skau: *Are there other persons that have had an influence on your choice of fields of interest during your youth?*

Tate: No. I think my interest is more innate. My father certainly helped, but I think I would have done something like physics or mathematics anyway.

Raussen and Skau: *You started to study physics at Harvard University. This was probably during the Second World War?*

Tate: I was in my last year of secondary school in December 1941 when Pearl Harbor was bombed. Because of the war Harvard began holding classes in the summer, and I started there the following June. A year later I volunteered for a Naval Officer Training Program in order to avoid being drafted into the army. A group of us was later sent to M.I.T. to learn meteorology, but by the time we finished that training and Midshipman School it was VE day.[1] Our campaign in the Pacific had been so successful that more meteorologists were not needed, and I was sent to do minesweeping research. I was in the Navy for three years and never aboard a ship! It was frustrating.

Raussen and Skau: *Study conditions in those times must have been quite different from conditions today. Did you have classes regularly?*

Tate: Yes, for the first year, except that it was accelerated. But then in the Navy I had specific classes to attend, along with a few others of my choice I could manage to squeeze in. It was a good program, but it was not the normal one.

It was not the normal college social life either, with parties and such. We had to be in bed or in a

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This is a slightly edited version of an interview taken on the morning preceding the prize ceremony: May 25, 2010, at Oslo.

[1] *Victory in Europe day: May 8, 1945.*

study hall by ten and were roused at 6:30 A.M. by a recording of reveille, to start the day with calisthenics and running.

Raussen and Skau: *Then you graduated in 1946 and went to Princeton?*

Tate: Yes, that's true. Harvard had a very generous policy of giving credit for military activities that might qualify—for instance, some of my navy training. This and the wartime acceleration enabled me to finish the work for my undergraduate degree in 1945. On my discharge in 1946, I went straight from the Navy to graduate school in Princeton.

Raussen and Skau: *When you went to Princeton University, it was still with the intention to become a physicist?*

Tate: That's correct. Although my degree from Harvard was in mathematics, I entered Princeton graduate school in physics. It was rather silly, and I have told the story many times: I had read the book *Men of Mathematics* by Eric Temple Bell. That book was about the lives of the greatest mathematicians in history, people like Abel. I knew I wasn't in their league and I thought that unless I was, I wouldn't really be able to do much in mathematics. I didn't realize that a less talented person could still contribute effectively. Since my father was a physicist, that field seemed more human and accessible to me, and I thought that was a safer way to go, where I might contribute more. But after one term it became obvious that my interest was really in mathematics. A deeper interest, which should have been clear anyway, but I just was too afraid and thought I never would be able to do much research if I went into mathematics.

Raussen and Skau: *Were you particularly interested in number theory from the very beginning?*

Tate: Yes. Since I was a teenager I had an interest in number theory. Fortunately, I came across a good number theory book by L. E. Dickson, so I knew a little number theory. Also I had been reading Bell's histories of people like Gauss. I liked number theory. It's natural, in a way, because many wonderful problems and theorems in number theory can be explained to any interested high-school student. Number theory is easier to get into in that sense. But of course it depends on one's intuition and taste also.

Raussen and Skau: *Many important questions are easy to explain, but answers are often very tough to find.*

Tate: Yes. In number theory that is certainly true, but finding good questions is also an important part of the game.

Teachers and Fellows

Raussen and Skau: *When you started your career at Princeton you very quickly met Emil Artin, who became your supervisor. Emil Artin was born in Austria and became a professor in mathematics at the University of Hamburg. He had to leave Germany in 1937 and came to the United States. Can you tell us more about his background? Why did he leave his chair, and how did he adjust when he came to the States?*

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Abel interview, from left to right: Martin Raussen, Christian Skau, and John Tate.

Tate: His wife was half Jewish, and he eventually lost his position in Germany. The family left in '37, but at that time there weren't so many open jobs in the United States. He took a position at the University of Notre Dame in spite of unpleasant memories of discipline at a Catholic school he had attended in his youth. After a year or two he accepted an offer from Indiana University and stayed there until 1946. He and his wife enjoyed Bloomington, Indiana, very much. He told me it wasn't even clear that he would have accepted Princeton's offer in 1946 except that President H. B. Wells of Indiana University, an educational visionary, was on a world tour, and somehow Indiana didn't respond very well to Princeton's offer. Artin went to Princeton the same year I did.

Raussen and Skau: *Artin had apparently a very special personality. First of all, he was an eminent number theorist, but also a very intriguing person; a special character. Could you please tell us a bit more about him?*

Tate: I think he would have made a great actor. His lectures were polished: He would finish at the right moment and march off the scene. A very lively individual with many interests: music, astronomy, chemistry, history…. He loved to teach. I had a feeling that he loved to teach anybody anything. Being his student was a wonderful experience; I couldn't have had a better start to my mathematical career. It was a remarkable accident. My favorite theorem, which I had first learned from Bell's book, was Gauss's law of quadratic reciprocity, and there, entirely by chance, I found myself at the same university as the man who had discovered *the* ultimate law of reciprocity. It was just amazing.

Raussen and Skau: *What a coincidence!*

Tate: Yes, it really was.

Raussen and Skau: *You wrote your thesis with Artin, and we will certainly come back to it. After that you organized a seminar together with Artin on class field theory. Could you comment on this seminar: What was the framework and how did it develop?*

Tate: During his first two years in Princeton, Artin gave seminars in algebraic number theory, followed by class field theory. I did not attend the former, but one of the first things I heard about Artin concerned an incident in it. A young British student, Douglas Northcott, who had been captured when the Japanese trapped the British army in Singapore and barely survived in the Japanese prison camp, was in Princeton on a Commonwealth Fellowship after the war. Though his thesis was in analysis under G. H. Hardy, he attended Artin's seminar, and when one of the first speakers mentioned the characteristic of a field, Northcott raised his hand and asked what that meant. His question begot laughter from several students, whereupon Artin delivered a short lecture on the fact that one could be a fine mathematician without knowing what the characteristic of a field was. And, indeed, it turned out that Northcott was the most gifted student in that seminar.

But I'm not answering your question. I attended the second year, in which class field theory was treated, with Chevalley's nonanalytic proof of the second inequality, but not much cohomology. This was the seminar at the end of which Wang discovered that both published proofs of Grunwald's theorem, and in fact the theorem itself, were not correct at the prime 2.

At about that time, Gerhard Hochschild and Tadasi Nakayama were introducing cohomological methods in class field theory and used them to prove the main theorems, including the existence of the global fundamental class which A. Weil had recently discovered. In 1951–52 Artin and I ran another seminar giving a complete treatment of class field theory incorporating these new ideas. That is the seminar you are asking about. Serge Lang took notes, and thanks to his efforts they were eventually published, first as informal mimeographed notes and, in 1968, commercially, under the title *Class Field Theory*. A new edition (2008) is available from AMS-Chelsea.

Raussen and Skau: *Serge Lang was also a student of Emil Artin and became a famous number theorist. He is probably best known as author of many textbooks; almost every graduate student in mathematics has read a textbook by Serge Lang. He is also quite known for his intense temper, and he got into a lot of arguments with people. What can you tell us about Serge Lang? What are your impressions?*

Tate: He was indeed a memorable person. The memories of Lang in the May 2006 issue of the *Notices of the AMS*, written by about twenty of his many friends, give a good picture of him. He started Princeton graduate school in philosophy, a year after I started in physics, but he, too, soon switched to math. He was a bit younger than I and had served a year and a half in the U.S. Army in Europe after the war, where he had a clerical position in which he learned to type at incredible speed, an ability which served him well in his later book writing.

He had many interests and talents. I think his undergraduate degree from Caltech was in physics. He knew a lot of history and he played the piano brilliantly.

He didn't have the volatile personality you refer to until he got his degree. It seemed to me that he changed. It was almost a discontinuity; as soon he got his Ph.D. he became more authoritative and asserted himself more.

It has been noted that there are many mathematical notions linked to my name. I think that's largely due to Lang's drive to make information accessible. He wrote voluminously. I didn't write easily and didn't get around to publishing; I was always interested in thinking about the next problem. To promote access, Serge published some of my stuff and, in reference, called things "Tate this" and "Tate that" in a way I would not have done had I been the author.

Throughout his life, Serge addressed great energy to disseminating information; to sharing where he felt it was important. We remained friends over the years.

Research Contributions

Raussen and Skau: *This brings us to the next topic: Your Ph.D. thesis from 1950, when you were twenty-five years old. It has been extensively cited in the literature under the sobriquet "Tate's thesis". Several mathematicians have described your thesis as unsurpassable in conciseness and lucidity and as representing a watershed in the study of number fields. Could you tell us what was so novel and fruitful in your thesis?*

Tate: Well, first of all, it was not a new result, except perhaps for some local aspects. The big global theorem had been proved around 1920 by the great German mathematician Erich Hecke, namely the fact that all *L*-functions of number fields, abelian *L*-functions, generalizations of Dirichlet's *L*-functions, have an analytic continuation throughout the plane with a functional equation of the expected type. In the course of proving it Hecke saw that his proof even applied to a new kind of *L*-function, the so-called *L*-functions with Grössencharacter. Artin suggested to me that one might prove Hecke's theorem using abstract harmonic analysis on what is now called the adele ring, treating all places of the field equally, instead of using classical Fourier analysis at the archimedian places and finite Fourier analysis with congruences at the p-adic places as Hecke had done. I think I did a good job —it might even have been lucid and concise!—but in a way it was just a wonderful exercise to carry out this idea. And it was also in the air. So often there is a time in mathematics for something to be done. My thesis is an example. Iwasawa would have done it had I not.

Raussen and Skau: *What do you think of the fact that, after your thesis, all places of number fields are treated on an equal footing in analytic number theory, whereas the situation is very different in the classical study of zeta functions; in fact, gamma factors are very different from nonarchimedean local factors.*

Tate: Of course there is a big difference between archimedean and nonarchimedean places, in particular as regards the local factors, but that is no reason to discriminate. Treating them equally, using adeles and ideles, is the simplest way to proceed, bringing the local–global relationship into clear focus.

Raussen and Skau: *The title of your thesis was* Fourier Analysis in Number Fields and Hecke's Zeta-Functions. *Atle Selberg said in an interview five years ago that he preferred—and was most inspired by—Erich Hecke's approach to algebraic number theory, modular forms and L-functions. Do you share that sentiment?*

Tate: Hecke and Artin were both at Hamburg University for a long time before Artin left. I think Artin came to number theory more from an algebraic side, whereas Hecke and Selberg came more from an analytic side. Their basic intuition was more analytic and Artin's was more algebraic. Mine was also more algebraic, so the more I learned of Hecke's work, the more I appreciated it, but somehow I did not instinctively follow him, especially as to modular forms. I didn't know much about them when I was young.

I have told the story before, but it is ironic that being at the same university, Artin had discovered a new type of L-series and Hecke, in trying to figure out what kind of modular forms of weight one there were, said they should correspond to some kind of L-function. The L-functions Hecke sought were among those that Artin had defined, but they never made contact—it took almost forty years until this connection was guessed and ten more before it was proved, by Langlands. Hecke was older than Artin by about ten years, but I think the main reason they did not make contact was their difference in mathematical taste. Moral: Be open to all approaches to a subject.

Raussen and Skau: *You mentioned that Serge Lang had named several concepts after you, but there are lots of further concepts and conjectures bearing your name. Just to mention a few: Tate module, Tate curve, Tate cohomology group, Shafarevich-Tate group, Tate conjecture, Sato-Tate conjecture, etc. Good definitions and fruitful concepts, as well as good problems, are perhaps as important as theorems in mathematics. You excel in all these categories. Did all or most of these concepts grow out of your thesis?*

Tate: No, I wouldn't say that. In fact, I would say that almost none of them grew out of my thesis. Some of them, like the Tate curve, grew out of my interest in p-adic fields, which were also very central in my thesis, but they didn't grow out of my thesis. They came from different directions. The Tate cohomology came from my understanding the cohomology of class field theory in the seminar that we discussed. The Shafarevich-Tate group came from applying that cohomology to elliptic curves and abelian varieties. In general, my conjectures came from an optimistic outlook, generalizing from special cases.

Although concepts, definitions, and conjectures are certainly important, the bottom line is to prove a theorem. But you do have to know what to prove, or what to try to prove.

Raussen and Skau: *In the introduction to your delightful book* Rational Points on Elliptic Curves *that you coauthored with your earlier Ph.D. student Joseph Silverman, you say, citing Serge Lang, that it is possible to write endlessly on elliptic curves. Can you comment on why the theory of elliptic curves is so rich and how it interacts and makes contact with so many different branches of mathematics?*

Tate: For one thing, they are very concrete objects. An elliptic curve is described by a cubic polynomial in two variables, so they are very easy to experiment with. On the other hand, elliptic curves illustrate very deep notions. They are the first nontrivial examples of abelian varieties. An elliptic curve is an abelian variety of dimension one, so you can get into this more advanced subject very easily by thinking about elliptic curves. On the other hand, they are algebraic curves. They are curves of genus one, the first example of a curve which isn't birationally equivalent to a projective line. The analytic and algebraic relations which occur in the theory of elliptic curves and elliptic functions are beautiful and unbelievably fascinating. The modularity theorem stating that every elliptic curve over the rational field can be found in the Jacobian variety of the curve which parametrizes elliptic curves with level structure its conductor is mind-boggling.

By the way, by my count about one quarter of Abel's published work is devoted to elliptic functions.

Raussen and Skau: *Among the Abel Prize laureates so far, you are probably the one whose contributions would have been closest to Abel's own interests. Could we challenge you to make a historical sweep, to put Abel's work in some perspective and to compare it to your research? In modern parlance, Abel studied the multiplication-by-n map for elliptic equal parts and studied the algebraic equations that* *arose. He studied also complex multiplication and showed that, in this case, it gave rise to a commutative Galois group. These are very central concepts and observations, aren't they?*

Tate: Yes, absolutely, yes. Well, there's no comparison between Abel's work and mine. I am in awe of what I know of it. His understanding of algebraic equations, and of elliptic integrals and the more general, abelian integrals, at that time in history is just amazing. Even more for a person so isolated. I guess he could read works of Legendre and other great predecessors, but he went far beyond. I don't really know enough to say more. Abel was a great analyst and a great algebraist. His work contains the germs of many important modern developments.

Raussen and Skau: *Could you comment on how the concept of "good reduction" for an elliptic curve is so crucial, and how it arose?*

Tate: If one has an equation with integer coefficients, it is completely natural, at least since Gauss, to consider the equation mod p for a prime p, which is an equation over the finite field F_p with p elements.

If the original equation is the equation of an elliptic curve E over the rational number field, then the reduced equation may or may not define an elliptic curve over F_p. If it does, we say E has "good reduction at p". This happens for all but a finite set of "bad primes for E", those dividing the discriminant of E.

Raussen and Skau: *The Hasse principle in the study of Diophantine equations says, roughly speaking: If an equation has a solution in* p*-adic numbers, then it can be solved in the rational numbers. It does not hold in general. There is an example for this failure given by the Norwegian mathematician Ernst Selmer…*

Tate: Yes. The equation $3x^3 + 4y^3 + 5z^3 = 0$.

Raussen and Skau: *Exactly! The extent of the failure of the Hasse principle for curves of genus 1 is quantified by the Shafarevich-Tate group. The so-called Selmer groups are related groups, which are known to be finite, but as far as we know the Shafarevich-Tate group is not known to be finite. It is only a conjecture that it is always finite. What is the status concerning this conjecture?*

Tate: The conjecture that the Shafarevich group Sha is finite should be viewed as part of the conjecture of Birch and Swinnerton–Dyer. That conjecture, BSD for short, involves the L-function of the elliptic curve, which is a function of a complex variable s. Over the rational number field, $L(s)$ is known to be defined near s=1, thanks to the modularity theorem of A. Wiles, R. Taylor, et al. If $L(s)$ either does not vanish or has a simple zero at s=1, then Sha is finite and BSD is true, thanks to the joint work of B. Gross and D. Zagier on Heegner points and the work of Kolyvagin on Euler systems. So, by three big results which are the work of many people, we know a very special circumstance in which Sha is finite.

If $L(s)$ has a higher order zero at s=1, we know nothing, even over the field of rational numbers. Over an imaginary quadratic field we know nothing, period.

Raussen and Skau: *Do you think that this group is finite?*

Tate: Yes. I firmly believe the conjecture is correct. But who knows? The curves of higher rank, or whose L-functions have a higher order zero—BSD says the order of the zero is the rank of the curve—one knows nothing about.

Raussen and Skau: *What is the origin of the Tate conjecture?*

Tate: Early on I somehow had the idea that the special case about endomorphisms of abelian varieties over finite fields might be true. A bit later I realized that a generalization fit perfectly with the function field version of the Birch and Swinnerton-Dyer conjecture. Also it was true in various particular examples which I looked at and gave a heuristic reason for the Sato-Tate distribution. So it seemed a reasonable conjecture.

Raussen and Skau: *In the arithmetic theory of elliptic curves, there have been major breakthroughs like the Mordell-Weil theorem, Faltings' proof of the Mordell conjecture, using the known reduction to a case of the Tate conjecture. Then we have Wiles's breakthrough proving the Shimura-Taniyama-Weil conjecture. Do you hope the next big breakthrough will come with the Birch and Swinnerton-Dyer conjecture? Or the Tate conjecture, maybe?*

Tate: Who knows what the next big breakthrough will be, but certainly the Birch and Swinnerton-Dyer conjecture is a big challenge; and also the modularity, i.e., the Shimura-Taniyama-Weil idea, which is now seen as part of the Langlands program. If the number field is not totally real, we don't know much about either of these problems. There has been great progress in the last thirty years, but it is just the very beginning. Proving these things for all number fields and for all orders of vanishing, to say nothing of doing it for abelian varieties of higher dimension, will require much deeper insight than we have now.

Raussen and Skau: *Is there any particular work from your hand that you are most proud of, that you think is your most important contribution?*

Tate: I don't feel that any one of my results stands out as most important. I certainly enjoyed working out the proofs in my thesis. I enjoyed very much proving a very special case of the so-called Tate conjecture, the result about endomorphisms of abelian varieties over finite fields. It was great to be able to prove at least one nontrivial case and not have only a conjecture! That's a case that is useful in cryptography, especially elliptic curves over finite fields. Over number fields, even finitely generated fields, that case of my conjecture was proved by Faltings, building on work of Zarhin over function fields, as the first step in his proof of the Mordell conjecture. I enjoyed very much the paper which I dedicated to Jean-Pierre Serre on the K^2 groups of number fields. I also had fun with a paper on residues of differentials on curves giving a new definition of residue and a new proof that the sum of the residues is zero, even though I failed to see a more important aspect of the construction.

Applied Number Theory

Raussen and Skau: *Number theory stretches from the mysteries of the prime numbers to the way we save, transmit, and secure information on modern computers. Can you comment on the amazing fact that number theory, in particular the arithmetic of elliptic curves, has been put to use in practical applications?*

Tate: It certainly is amazing to me. When I first studied and worked on elliptic curves I had no idea that they ever would be of any practical use. I did not foresee that. It is the high-speed computers which made the applications possible, but of course many new ideas were needed also.

Raussen and Skau: *And now it's an industry: elliptic curves, cryptography, intelligence, and communication!*

Tate: It's quite remarkable. It often happens that things which are discovered just for their own interest and beauty later turn out to be useful in practical affairs.

Raussen and Skau: *We interviewed Jacques Tits a couple of years ago. His comment was that the Monster group, the biggest of all the sporadic simple groups, is so beautiful that it* has *to have some application in physics or whatever.*

Tate: That would be interesting!

Collaboration and Teaching

Raussen and Skau: *You have been one of the few non-French members of the Bourbaki group, the group of mathematicians that had the endeavor to put all existing mathematics into a rigid format. Can you explain what this was all about and how you got involved?*

Tate: I would not say it was about putting mathematics in a rigid format. I view Bourbaki as a modern Euclid. His aim was to write a coherent series of books which would contain the fundamental definitions and results of all mathematics as of mid-twentieth century. I think he succeeded pretty well, though the books are somewhat unbalanced—weak in classical analysis and heavy on Lie theory. Bourbaki did a very useful service for a large part of the mathematics community just by establishing some standard notations and conventions.

The presentation is axiomatic and severe, with no motivation except for the logic and beauty of the development itself. I was always a fan of Bourbaki. That I was invited to collaborate may have been at Serge Lang's suggestion, or perhaps Jean-Pierre Serre's also. As I mentioned, I am not a very prolific writer. I usually write a few pages and then tear them up and start over, so I never was able to contribute much to the writing. Perhaps I helped somewhat in the discussion of the material. The conferences were enjoyable, all over France, in the Alps, and even on Corsica. It was a lot of fun.

Raussen and Skau: *You mentioned Jean-Pierre Serre, who was the first Abel Prize laureate. He was one of the driving forces in the Bourbaki project after the Second World War. We were told that he was—as* [was] *Serge Lang—instrumental in getting some of your results published in the form of lecture notes and textbooks. Do you have an ongoing personal relation with Jean-Pierre Serre?*

Tate: Yes. I'm looking forward to meeting him next week when we will both be at Harvard for a conference in honor of Dick Gross on his sixtieth birthday. Gross was one of my Ph.D. students.

I think Serre was a perfect choice for the first Abel Prize laureate.

Raussen and Skau: *Another possible choice would have been Alexander Grothendieck. But he went into reclusion. Did you meet him while you were in Paris or maybe at Harvard?*

Tate: I met him in Paris. I had a wonderful year. Harvard had the enlightened policy of giving a tenure-track professor a year's sabbatical leave. I went to Paris for the academic year 1957–58, and it was a great experience. I met Serre, I met Grothendieck, and I was free from any duty. I could think and I could learn. Later, they both visited Harvard several times, so I saw them there too. It's great good fortune to be able to know such people.

Raussen and Skau: *Did you follow Grothendieck's program reconstructing the foundations of algebraic geometry closely?*

Tate: Well, yes, to the extent I could. I felt "ah, at last, we have a good foundation for algebraic geometry." It just seemed to me to be the right thing. Earlier I was always puzzled, do we have affine varieties, projective varieties? But it wasn't a category. Grothendieck's schemes, however, did form a category. And breaking away from a ground field to a ground ring, or even a ground scheme, so that the foundations could handle not only polynomial equations but also Diophantine equations and reduction mod p, was just what number theorists needed.

Raussen and Skau: *We have a question of a more general and philosophical nature: A great mathematician once mentioned that it is essential to possess a certain naiveté in order to be able to create something really new in mathematics. One can do impressive things requiring complicated techniques, but one rarely makes original discoveries without being a bit naive. In the same vein, André Weil claimed that breakthroughs in mathematics *are typically not done by people with long experience and lots of knowledge. New ideas often come without that baggage. Do you agree?*

Tate: I think it's quite true. Most mathematicians do their best work when they are young and don't have a lot of baggage. They haven't worn grooves in their brains that they follow. Their brains are fresher, and certainly it's important to think for oneself rather than just learning what others have done. Of course, you have to build on what has been done before or else it's hopeless; you can't rediscover everything. But one should not be prejudiced by the past work. I agree with the point of view you describe.

Raussen and Skau: *Did you read the masters of number theory already early in your career?*

Tate: I've never been such a good reader. My instincts have been to err on the side of trying to be independent and trying to do things myself. But as I said, I was very fortunate to be in contact with brilliant people, and I learned very much from personal conversations. I never was a great reader of the classics. I enjoyed that more as I got older.

Raussen and Skau: *You have had some outstanding students who have made important contributions to mathematics. How did you attract these students in the first place, and how did you interact with them, both as students and later?*

Tate: I think we were all simply interested in the same kind of mathematics. You know, with such gifted students there is usually no problem: After getting to know them and their interests you suggest things to read and think about, then just hear about progress and problems, offering support and encouragement as they find their way.

Raussen and Skau: *Did you give them problems to work on or did they find the problems themselves?*

Tate: It varies. Several found their own problems. With others I made somewhat more specific suggestions. I urged Dick Gross to think about a problem which I had been trying unsuccessfully to solve, but very sensibly he wrote a thesis on a quite different subject of his own choosing. I was fortunate to have such able students. I continued to see many of them later, and many are good friends.

Raussen and Skau: *You have taught mathematics for more than sixty years, both at Harvard and at Austin, Texas. How much did you appreciate this aspect of your professional duties? Is there a particular way of teaching mathematics that you prefer?*

Tate: I always enjoyed teaching at all levels. Teaching a subject is one of the best ways to learn it thoroughly. A few times, I've been led to a good new idea in preparing a lecture for an advanced course. That was how I found my definition of Neron's height, for example.

Work Style

Raussen and Skau: *Would you consider yourself mainly a theory builder or a problem solver?*

Tate: I suppose I'm a theory builder or maybe a conjecture maker. I'm not a conjecture prover very much, but I don't know. It's true that I'm not good at solving problems. For example, I would never be good in the Math Olympiad. There speed counts and I am certainly not a speedy worker. That's one pleasant thing in mathematics: It doesn't matter how long it takes if the end result is a good theorem. Speed is an advantage, but it is not essential.

Raussen and Skau: *But you are persistent. You have the energy to stay with a problem.*

Tate: At least, I did at one time.

Raussen and Skau: *May we ask you a question that we, in various ways, have asked almost everybody in previous interviews: Look back on how you came up with new concepts or made a breakthrough in an area you had been working on for some time. Did that usually happen when you were concentrated and working intensely on the problem, or did it happen in a more relaxed situation? Do you have concrete examples?*

Tate: The first thing I did after my thesis was the determination of the higher-dimensional cohomology groups in class field theory. I had been working on that for several months, off and on. This was at the time of the seminar after my thesis at Princeton. One evening I went to a party and had a few drinks. I came home after midnight and thought I would think a little about the problem. About one or two in the morning I saw how to do it!

Raussen and Skau: *So this was a "Poincaré moment"?*

Tate: In a way. I think that, like him, I had put the work aside for a longer time when this happened. I remember what it was: I had been invited to give some talks at MIT on class field theory and I thought "what am I going to say?" So it was after a party, motivated by needing something to say at MIT, that this idea struck me. It was very fortunate.

But it varies. Sometimes I've had an idea after talking to someone and had the impression the person I was talking to had the idea and told me about it. The Ph.D. thesis of my student Jonathan Lubin was on what should be called the Lubin groups. They somehow have been called the Lubin-Tate groups. Incidentally, I think it's useful in math that theorems or ideas have two names so you can identify them. If I say Serre's theorem, my God, that doesn't say too much. But anyway, they are called Lubin-Tate groups, and it occurred to me, just out of the blue, that they might be useful in class field theory. And then we worked it out and indeed they were. One gets ideas in different ways, and it's a wonderful feeling for a few minutes, but then there is a letdown after you get used to the idea.

Raussen and Skau: *Group cohomology had been studied in various guises, long before the* *notion of group cohomology was formulated in the 1940s. You invented what is called Tate cohomology groups, which are widely used in class field theory, for instance. Could you elaborate?*

Tate: In connection with class field theory it suddenly dawned on me that if the group is finite—the operating group G—then one could view the homology theory of that group as negative dimensional cohomology. Usually the homology and the cohomology are defined in nonnegative dimensions, but suddenly it became clear to me that for a finite group you could glue the two theories together. The ith homology group can be viewed as a $(1-i)$th cohomology group and then you can glue these two sequences together so that the cohomology goes off to plus infinity and the homology goes off, with renumbering, to minus infinity, and you fiddle a little with the joining point and then you have one theory going from negative infinity to plus infinity.

Raussen and Skau: *Was this a flash of insight?*

Tate: Perhaps. There was a clue from the finite cyclic case, where there is periodicity; a periodicity of length two. For example, H^3 is isomorphic to H^1, the H^5 is isomorphic to H^3, etc., and it's obvious that you could go on to infinity in both directions. Somehow it occurred to me that one could do that for an arbitrary finite group. I don't remember exactly how it happened in my head.

The Roles of Mathematics

Raussen and Skau: *Can we speculate a little about the future development of mathematics? When the Clay Millennium Prizes for solving outstanding problems in mathematics were established back in the year 2000, you presented three of these problems to the mathematical public. Not necessarily restricting to those, would you venture a guess about new trends in mathematics: the twenty-first century compared to the twentieth century? Are there trends that are entirely new? What developments can we expect in mathematics and particularly in your own field, number theory?*

Tate: We certainly have plenty of problems to work on. One big difference in mathematics generally is the advent of high-speed computers. Even for pure math, that will increase the experimental possibilities enormously. It has been said that number theory is an experimental science, and until recently that meant experimenting by looking at examples by hand and discovering patterns that way. Now we have a zillionfold more powerful way to do that, which may very well lead to new ideas even in pure math, but certainly also for applications.

Mathematics somehow swings between the development of new abstract theories and the application of these to more concrete problems and from concrete problems to theories needed to solve them. The pendulum swings. When I was young better foundations were being developed, things were becoming more functorial, if you will, and a very abstract point of view led to much progress. But then the pendulum swung the other way to more concrete things in the 1970s and 1980s. There were modular forms and the Langlands program, the proof of the Mordell conjecture and of Fermat's last theorem. In the first half of my career, theoretical physics and mathematics were not so close. There was the time when the development of mathematics went in the abstract direction, and the physicists were stuck. But now in the last thirty years they have come together. It is hard to tell whether string theory is math or physics. And noncommutative geometry has both sides.

Who knows what the future will be? I don't think I can contribute much in answering that question. Maybe a younger person would have a better idea.

Raussen and Skau: *Are you just as interested in mathematics now as you were when you were young?*

Tate: Well, not as intensely. I'm certainly still very much interested, but I don't have the energy to really go so deeply into things.

Raussen and Skau: *But you try to follow what is happening in your field?*

Tate: Yes, I try. I'm in awe of what people are doing today.

Raussen and Skau: *Your teacher Emil Artin, when asked about whether mathematics was a science, would rather say: "No. It's an art." On the other hand, mathematics is connected to the natural sciences, to computing and so on. Perhaps it has become more important in other fields than ever; the mutual interaction between science and engineering on one side and mathematics on the other has become more visible. Is mathematics an art, is it rather to be applied in science, or is it both?*

Tate: It's both, for heaven's sake! I think Artin simply was trying to make a point that there certainly is an artistic aspect to mathematics. It's just beautiful. Unfortunately it's only beautiful to the initiated, to the people who do it. It can't really be understood or appreciated much on a popular level the way music can. You don't have to be a composer to enjoy music, but in mathematics you do. That's a really big drawback of the profession. A nonmathematician has to make a big effort to appreciate our work; it's almost impossible.

Yes, it's both. Mathematics is an art, but there are stricter rules than in other arts. Theorems must be proved as well as formulated; words must have precise meanings. The happy thing is that mathematics does have applications which enable us to earn a good living doing what we would do even if we weren't paid for it. We are paid mainly to teach the useful stuff.

Public Awareness of Mathematics

Raussen and Skau: *Have you tried to popularize mathematics yourself?*

Tate: When I was young I tried to share my enthusiasm with friends, but I soon realized that's almost impossible.

Raussen and Skau: *We all feel the difficulty of communicating with the general audience. This interview is one of the rare occasions providing public attention for mathematics!*[2] *Do you have any ideas about how mathematicians can make themselves and what they do more well known? How can we increase the esteem of mathematics among the general public and among politicians?*

Tate: Well, I think prizes like this do some good in that respect. And the Clay Prizes likewise. They give publicity to mathematics. At least people are aware. I think the appreciation of science in general and mathematics in particular varies with the country. What fraction of the people in Norway would you say have an idea about Abel?

Raussen and Skau: *Almost everyone in Norway knows about Abel, but they do not know anything about Lie. And not necessarily anything about Abel's work, either. They may know about the quintic.*

Tate: I see. And how about Sylow?

Raussen and Skau: *He is not known either. Abel's portrait has appeared on stamps and also on bills, but neither Lie's nor Sylow's.*

Tate: I think in Japan, people are more aware. I once was in Japan and eating alone. A Japanese couple came and wanted to practice their English. They asked me what I did. I said I was a mathematician but could not get the idea across until I said: "Like Hironaka". Wow! It's as though in America I'd said "Like Babe Ruth'', or Michael Jordan, or Tiger Woods. Perhaps Hironaka's name is, like Abel's, the only one known, but in America I don't think any mathematician's name would get any response.

Private Interests

Raussen and Skau: *Our last question: What other interests do you have in life? What are you occupied with when you are not thinking about mathematics? Certainly that happens once in a while, as well?*

Tate: I'm certainly not a Renaissance man. I don't have wide knowledge or interests. I have enjoyed very much the outdoors, hiking, and also sports. Basketball was my favorite sport. I played on the Southeast Methodist church team as a teenager and we won the Minneapolis church league championship one year. There were several of us who went to church three out of four Sundays during a certain period in the winter, in order to play on the team. In the Navy I coached a team from the minesweeping research base which beat Coca-Cola for the Panama City league championship. Anyway, I have enjoyed sports and the outdoors.

I like to read a reasonable amount and I enjoy music, but I don't have a really deep or serious hobby. I think I'm more concentrated in mathematics than many people. My feeling is that to do some mathematics I just have to concentrate. I don't have the kind of mind that absorbs things very easily.

Raussen and Skau: *We would like to thank you very much for this interview; as well as on behalf of the Norwegian, Danish, and European mathematical societies. Thank you very much!*

Tate: Well, thank you for not asking more difficult questions! I have enjoyed talking with you.

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[2] *The interview was broadcast on Norwegian television; cf.* http://www.abelprisen.no/en/multimedia/2010/.