Interview with Peter D. Lax

Martin Raussen and Christian Skau

Peter D. Lax is the recipient of the 2005 Abel Prize of the Norwegian Academy of Science and Letters. On May 24, 2005, prior to the Abel Prize celebrations in Oslo, Lax was interviewed by Martin Raussen of Aalborg University and Christian Skau of the Norwegian University of Science and Technology. This interview originally appeared in the *European Mathematical Society Newsletter*, September 2005, pages 24–31.

Raussen & Skau: *On behalf of the Norwegian and Danish Mathematical Societies we would like to congratulate you on winning the Abel Prize for 2005.*

You came to the U.S. in 1941 as a fifteen-year-old kid from Hungary. Only three years later, in 1944, you were drafted into the U.S Army. Instead of being shipped overseas to the war front, you were sent to Los Alamos in 1945 to participate in the Manhattan Project, building the first atomic bomb. It must have been awesome as a young man to come to Los Alamos to take part in such a momentous endeavor and to meet so many legendary famous scientists: Fermi, Bethe, Szilard, Wigner, Teller, Feynman, to name some of the physicists, and von Neumann and Ulam, to name some of the mathematicians. How did this experience shape your view of mathematics and influence your choice of a research field within mathematics?

Lax: In fact, I returned for a year's stay at Los Alamos after I got my Ph.D. in 1949 and then spent many summers as a consultant. The first time I spent in Los Alamos, and especially the later exposure, shaped my mathematical thinking. First of all, it was the experience of being part of a scientific team—not just of mathematicians, but people with different outlooks—with the aim being not a theorem, but a product. One cannot learn that from books, one must be a participant, and for that reason I urge my students to spend at least a summer as a visitor at Los Alamos. Los Alamos has a very active visitor's program. Secondly, it was there—that was in the 1950s—that I became imbued with the utter importance of computing for science and mathematics. Los Alamos, under the influence of von Neumann, was for a while in the 1950s and the early 1960s the undisputed leader in computational science.

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Research Contributions

R & S: *May we come back to computers later? First some questions about some of your main research contributions to mathematics: You have made outstanding contributions to the theory of nonlinear partial differential equations. For the theory and numerical solutions of hyperbolic systems of conservation laws your contribution has been decisive, not to mention your contribution to the understanding of the propagation of discontinuities, so-called shocks. Could you describe in a few words how you were able to overcome the formidable obstacles and difficulties this area of mathematics presented?*

Lax: Well, when I started to work on it I was very much influenced by two papers. One was Eberhard Hopf's on the viscous limit of Burgers' equation, and the other was the von Neumann-Richtmyer paper on artificial viscosity. And looking at these examples I was able to see what the general theory might look like.

R & S: *The astonishing discovery by Kruskal and Zabusky in the 1960s of the role of solitons for solutions of the Korteweg-deVries (KdV) equation, and the no less astonishing subsequent explanation given by several people that the KdV equation is completely integrable, represented a revolutionary development within the theory of nonlinear partial differential equations. You entered this field with an ingenious original point of view, introducing the so-called Lax-pair, which gave an understanding of how the inverse scattering transform applies to equations like the KdV, and also to other nonlinear equations which are central in mathematical physics,* (Photo: Knut Falch/Scanpix) Copyright: Det Norske Videnskaps-Akademi/Abelprisen.

Peter D. Lax was interviewed by Martin Raussen and Christian Skau at the Hotel Continental in Oslo.

like the sine-Gordon and the nonlinear Schrödinger equation. Could you give us some thoughts on how important you think this theory is for mathematical physics and for applications, and how do you view the future of this field?

Lax: Perhaps I should start by pointing out that the astonishing phenomenon of the interaction of solitons was discovered by numerical calculations, as was predicted by von Neumann some years before, namely that calculations will reveal extremely interesting phenomena. Since I was a good friend of Kruskal, I learned early about his discoveries, and that started me thinking. It was quite clear that there are infinitely many conserved quantities, and so I asked myself: How can you generate all at once an infinity of conserved quantities? I thought if you had a transformation that preserved the spectrum of an operator then that would be such a transformation, and that turned out to be a very fruitful idea, applicable quite widely.

Now you ask how important is it? I think it is pretty important. After all, from the point of view of technology for the transmission of signals, signalling by solitons is very important and a promising future technology in trans-oceanic transmission. This was developed by Linn Mollenauer, a brilliant engineer at Bell Labs. It has not yet been put into practice, but it will be some day. The interesting thing about it is that classical signal theory is entirely linear, and the main point of soliton signal transmission is that the equations are nonlinear. That's one aspect of the practical importance of it.

As for the theoretic importance: the KdV equation is completely integrable, and then an astonishing number of other completely integrable systems were discovered. Completely integrable systems can really be solved in the sense that the general population uses the word solved. When a mathematician says he has solved the problem he means he knows the solution exists, that it's unique, but very often not much more.

Now the question is: Are completely integrable systems exceptions to the behavior of solutions of non-integrable systems, or is it that other systems have similar behavior, only we are unable to analyze it? And here our guide might well be the Kolmogorov-Arnold-Moser theorem which says that a system near a completely integrable system behaves as if it were completely integrable. Now, what near means is one thing when you prove theorems, another when you do experiments. It's another aspect of numerical experimentation revealing things. So I do think that studying completely integrable systems will give a clue to the behavior of more general systems as well.

Who could have guessed in 1965 that completely integrable systems would become so important?

R & S: *The next question is about your seminal paper "Asymptotic solutions of oscillating initial value problems" from 1957. This paper is considered by many people to be the genesis of Fourier Integral Operators. What was the new viewpoint in the paper that proved to be so fruitful?*

Lax: It is a micro-local description of what is going on. It combines looking at the problem in the large and in the small. It combines both aspects, and that gives it its strengths. The numerical implementation of the micro-local point of view is by wavelets and similar approaches, which are very powerful numerically.

R & S: *May we touch upon your collaboration with Ralph Phillips—on and off over a span of more than thirty years—on scattering theory, applying it in a number of settings. Could you comment on this collaboration, and what do you consider to be the most important results you obtained?*

Lax: That was one of the great pleasures of my life! Ralph Phillips is one of the great analysts of our time and we formed a very close friendship. We had a new way of viewing the scattering process with incoming and outgoing subspaces. We were, so to say, carving a semi-group out of the unitary group, whose infinitesimal generator contained almost all the information about the scattering process. So we applied that to classical scattering of sound waves and electromagnetic waves by potentials and obstacles. Following a very interesting discovery of Faddeev and Pavlov, we studied the spectral theory of automorphic functions. We elaborated it further, and we had a brand new approach to Eisenstein series for instance, getting at spectral representation via translation representation. And we were even able to contemplate—following Faddeev and Pavlov—the Riemann hypothesis peeking around the corner.

R & S: *That must have been exciting!*

Lax: Yes! Whether this approach will lead to the proof of the Riemann hypothesis, stating it, as one can, purely in terms of decaying signals by cutting out all standing waves, is unlikely. The Riemann hypothesis is a very elusive thing. You may remember in Peer Gynt there is a mystical character, the Boyg, which bars Peer Gynt's way wherever he goes. The Riemann hypothesis resembles the Boyg!

R & S: *Which particular areas or questions are you most interested in today?*

Lax: I have some ideas about the zero dispersion limit.

Pure and Applied Mathematics

R & S: *May we raise a perhaps contentious issue with you: pure mathematics versus applied mathematics. Occasionally one can hear within the mathematical community statements that the theory of nonlinear partial differential equations, though profound and often very important for applications, is fraught with ugly theorems and awkward arguments. In pure mathematics, on the other hand, beauty and aesthetics rule. The English mathematician G.H. Hardy is an extreme example of such an attitude, but it can be encountered also today. How do you respond to this? Does it make you angry?*

Lax: I don't get angry very easily. I got angry once at a dean we had, terrible son of a bitch, destructive liar, and I got very angry at the mob that occupied the Courant Institute and tried to burn down our computer. Scientific disagreements do not arouse my anger. But I think this opinion is definitely wrong. I think Paul Halmos once claimed that applied mathematics was, if not bad mathematics, at least ugly mathematics, but I think I can point to those citations of the Abel Committee dwelling on the elegance of my works!

Now about Hardy: When Hardy wrote *A Mathematician's Apology* he was at the end of his life, he was old, I think he had suffered a debilitating heart attack, he was very depressed. So that should be taken into account. About the book itself: There was a very harsh criticism by the chemist Frederick Soddy, who was one of the co-discoverers of the isotopes—he shared the Nobel Prize with Rutherford. He looked at the pride that Hardy took in the uselessness of his mathematics and wrote: "From such cloistral clowning the world sickens." It was very harsh because Hardy was a very nice person.

My friend Joe Keller, a most distinguished applied mathematician, was once asked to define applied mathematics and he came up with this: "Pure mathematics is a branch of applied mathematics." Which is true if you think a bit about it. Mathematics originally, say after Newton, was designed to solve very concrete problems that arose in physics. Later on, these subjects developed on their own and became branches of pure mathematics, but they all came from applied background. As von Neumann pointed out, after a while these pure branches that develop on their own need invigoration by new empirical material, like some scientific questions, experimental facts, and, in particular, some numerical evidence.

R & S: *In the history of mathematics, Abel and Galois may have been the first great mathematicians that one may describe as "pure mathematicians", not being interested in any "applied" mathematics as such. However, Abel did solve an integral equation, later called "Abel's integral equation", and Abel gave an explicit solution, which incidentally may have been the first time in the history of mathematics that an integral equation had been formulated and solved. Interestingly, by a simple reformulation one can show that the Abel integral equation and its solution are equivalent to the Radon Transform, the mathematical foundation on which modern medical tomography is based.*

Examples of such totally unexpected practical applications of pure mathematical results and theorems abound in the history of mathematics—group theory that evolved from Galois' work is another striking example. What are your thoughts on this phenomenon? Is it true that deep and important theories and theorems in mathematics will eventually find practical applications, for example in the physical sciences?

Lax: Well, as you pointed out, this has very often happened: Take for example Eugene Wigner's use of group theory in quantum mechanics. And this has happened too often to be just a coincidence. Although, one might perhaps say that other theories and theorems which did not find applications were forgotten. It might be interesting for a historian of mathematics to look into that phenomenon. But I do believe that mathematics has a mysterious unity which really connects seemingly distinct parts, which is one of the glories of mathematics.

R & S: *You have said that Los Alamos was the birthplace of computational dynamics, and I guess it is safe to say that the U.S. war effort in the 1940s advanced and accelerated this development. In what way has the emergence of the high-speed computer altered the way mathematics is done? Which role will high-speed computers play within mathematics in the future?*

Lax: It has played several roles. One is what we saw in Kruskal's and Zabusky's discovery of solitons, which would not have been discovered without computational evidence. Likewise the Fermi-Pasta-Ulam phenomenon of recurrence was also a very striking thing which may or may not have been discovered without the computer. That is one aspect.

But another is this: in the old days, to get numerical results you had to make enormously drastic simplifications if your computations were done by hand, or by simple computing machines. And the talent of what drastic simplifications to make was a special talent that did not appeal to most mathematicians. Today you are in an entirely different situation. You don't have to put the problem on a Procrustean bed and mutilate it before you attack it numerically. And I think that has attracted a much larger group of people to numerical problems of applications—you could really use the full theory. It invigorated the subject of linear algebra, which as a research subject died in the 1920s. Suddenly the actual algorithms for carrying out these operations became important. It was full of surprises, like fast matrix multiplication. In the new edition of my linear algebra book I will add a chapter on the numerical calculation of the eigenvalues of symmetric matrices.

You know it's a truism that due to increased speed of computers, a problem that took a month forty years ago can be done in minutes, if not seconds today. Most of the speed-up is attributed, at least by the general public, to increased speed of computers. But if you look at it, actually only half of the speed-up is due to this increased speed. The other half is due to clever algorithms, and it takes mathematicians to invent clever algorithms. So it is very important to get mathematicians involved, and they are involved now.

R & S: *Could you give us personal examples of how questions and methods from applied points of view have triggered "pure" mathematical research and results? And conversely, are there examples where your theory of nonlinear partial differential equations, especially your explanation of how discontinuities propagate, have had commercial interests? In particular, concerning oil exploration, so important for Norway!*

Lax: Yes, oil exploration uses signals generated by detonations that are propagated through the earth and through the oil reservoir and are recorded at distant stations. It's a so-called inverse problem. If you know the distribution of the densities of materials and the associated waves' speeds, then you can calculate how signals propagate. The inverse problem is that if you know how signals propagate, then you want to deduce from it the distribution of the materials. Since the signals are discontinuities, you need the theory of propagation of discontinuities. Otherwise it's somewhat similar to the medical imaging problem, also an inverse problem. Here the signals do not go through the earth but through the human body, but there is a similarity in the problems. But there is no doubt that you have to understand the direct problem very well before you can tackle the inverse problem.

Hungarian Mathematics

R & S: *Now to some questions related to your personal history. The first one is about your interest in, and great aptitude for, solving problems of a type that you call "Mathematics Light" yourself. To mention just a few, already as a seventeen-year-old boy you gave an elegant solution to a problem that was posed by Erdős and is related to a certain inequality for polynomials, which was earlier proved by Bernstein. Much later in your career you studied the so-called Pólya function which maps the unit interval continuously onto a right-angled triangle, and you discovered its amazing differentiability properties. Was problem solving specifically encouraged in your early mathematical education in your native Hungary, and what effect has this had on your career later on?*

Lax: Yes, problem solving was regarded as a royal road to stimulate talented youngsters, and I was very pleased to learn that here in Norway they have a successful high-school contest, where the winners were honored this morning. But after a while one shouldn't stick to problem solving, one should broaden out. I return to it every once in a while, though.

Back to the differentiability of the Pólya function: I knew Pólya quite well having taken a summer course with him in 1946. The differentiability question came about this way: I was teaching a course on real variables, and I presented Pólya's example of an area-filling curve, and I gave as homework to the students the problem of proving that it's nowhere differentiable. Nobody did the homework, so then I sat down and I found out that the situation was more complicated.

There was a tradition in Hungary to look for the simplest proof. You may be familiar with Erdős' concept of The Book. That's The Book kept by the Lord of all theorems and the best proofs. The highest praise that Erdős had for a proof was that it was out of The Book. One can overdo that, but shortly after I had gotten my Ph.D., I learned about the Hahn-Banach theorem, and I thought that it could be used to prove the existence of Green's function. It's a very simple argument—I believe it's the simplest—so it's out of The Book. And I think I have a proof of Brouwer's Fixed Point Theorem, using calculus and just change of variables. It is probably the simplest proof and is again out of The Book. I think all this is part of the Hungarian tradition. But one must not overdo it.

R & S: *There is an impressive list of great Hungarian physicists and mathematicians of Jewish background that had to flee to the U.S. after the rise of fascism, Nazism and anti-Semitism in Europe. How do you explain this extraordinary culture of excellence in Hungary that produced people like de Hevesy, Szilard, Wigner, Teller, von Neumann, von Karman, Erdős, Szegő, Pólya, yourself, to name some of the most prominent ones?*

Lax: There is a very interesting book written by John Lukacs with the title "Budapest 1900: A Historical Portrait of a City and its Culture", and it chronicles the rise of the middle class, rise of commerce, rise of industry, rise of science, rise of literature. It was fueled by many things: a long period of peace, the influx of mostly Jewish population from the East eager to rise, and intellectual tradition. You know in mathematics, Bolyai was a cultural hero to Hungarians, and that's why mathematics was particularly looked upon as a glorious profession.

R & S: *But who nurtured this fantastic flourishing of talent, which is so remarkable?*

Lax: Perhaps much credit should be given to Julius Kőnig, whose name is probably not known to you. He was a student of Kronecker, I believe, but he also learned Cantor's set theory and made some basic contribution to it. I think he was influential in nurturing mathematics. His son was a very distinguished mathematician, Denes Kőnig, really the father of modern graph theory. And then there arose extraordinary people. Leopold Fejér, for instance, had enormous influence. There were too many to fill positions in a small country like Hungary, so that's why they had to go abroad. Part of it was also anti-Semitism.

There is a charming story about the appointment of Leopold Fejér, who was the first Jew proposed for a professorship at Budapest University. There was opposition to it. At that time there was a very distinguished theologian, Ignatius Fejér, in the Faculty of Theology. Fejér's original name was Weiss. So one of the opponents, who knew full well that Fejér's original name had been Weiss, said pointedly: This professor Leopold Fejér that you are proposing, is he related to our distinguished colleague Father Ignatius Fejér? And Eőtvős, the great physicist who was pushing the appointment, replied without batting an eyelash: "Illegitimate son." That put an end to it.

R & S: *And he got the job?*

Lax: He got the job.

Scribbles That Changed the Course of Human Affairs

R & S: *The mathematician Stanislaw Ulam was involved with the Manhattan Project and is considered to be one of the fathers of the hydrogen bomb. He wrote in his autobiography Adventures of a Mathematician: "It is still an unending source of surprise for me to see how a few scribbles on a blackboard, or on a sheet of paper, could change the course of human affairs." Do you share this feeling? And what are your feelings about what happened to Hiroshima and Nagasaki, to the victims of the explosions of the atomic bombs that brought an end to World War II?*

Lax: Well, let me answer the last question first. I was in the army, and all of us in the army expected to be sent to the Pacific to participate in the invasion of Japan. You remember the tremendous slaughter that the invasion of Normandy brought about. That would have been nothing compared to the invasion of the Japanese mainland. You remember the tremendous slaughter on Okinawa and Iwo Jima. The Japanese would have resisted to the last man. The atomic bomb put an end to all this and made an invasion unnecessary. I don't believe reversionary historians who say: "Oh, Japan was already beaten, they would have surrendered anyway." I don't see any evidence for that.

There is another point which I raised once with someone who had been involved with the atomic bomb project. Would the world have had the horror of nuclear war if it had not seen what one bomb could do? The world was inoculated against using nuclear weaponry by its use. I am not saying that alone justifies it, and it certainly was not the justification for its use. But I think that is a historical fact.

Now about scribbles changing history: Sure, the special theory of relativity, or quantum mechanics, would be unimaginable today without scribbles. Incidentally, Ulam was a very interesting mathematician. He was an idea man. Most mathematicians like to push their ideas through. He preferred throwing out ideas. His good friend Rota even suggested that he did not have the technical ability or patience to work them out. But if so, then it's an instance of Ulam turning a disability to tremendous advantage. I learned a lot from him.

R & S: *It is amazing for us to learn that an eighteen-year-old immigrant was allowed to participate in a top-secret and decisive weapon development during WWII.*

Lax: The war created an emergency. Many of the leaders of the Manhattan Project were foreigners, so being a foreigner was no bar.

Collaboration. Work Style

R & S: *Your main workplace has been the Courant Institute of Mathematical Sciences in New York, which is part of New York University. You served as its director for an eight-year period in the 1970s. Can you describe what made this institute, which was created by the German refugee Richard Courant in the 1930s, a very special place from the early days on, with a particular spirit and atmosphere? And is the Courant Institute today still a special place that differs from others?*

Lax: To answer your first question, certainly the personality of Courant was decisive. Courant saw mathematics very broadly, he was suspicious of specialization. He wanted it drawn as broadly as possible, and that's how it came about that applied topics and pure mathematics were pursued side by side, often by the same people. This made the Courant Institute unique at the time of its founding, as well as in the 1940s, 1950s, and 1960s. Since then there are other centers where applied mathematics is respected and pursued. I am happy to say that this original spirit is still present at the Courant Institute. We still have large areas of applied interest, meteorology and climatology under Andy Majda, solid state and material science under Robert Kohn and others, and fluid dynamics. But we also have differential geometry as well as some pure aspects of partial differential equations, even some algebra.

I am very pleased how the Courant Institute is presently run. It's now the third generation that's running it, and the spirit that Courant instilled in it—kind of a family feeling—still prevails. I am happy to note that many Norwegian mathematicians received their training at the Courant Institute and later rose to become leaders in their field.

R & S: *You told us already about your collaboration with Ralph Phillips. Generally speaking, looking through your publication list and the theorems and methods you and your collaborators have given name to, it is apparent that you have had a vast collaboration with a lot of mathematicians. Is this sharing of ideas a particularly successful, and maybe also joyful, way of advancing for you?*

Lax: Sure, sure. Mathematics is a social phenomenon after all. Collaboration is a psychological and interesting phenomenon. A friend of mine, Vera John-Steiner, has written a book (*Creative Collaboration*) about it. Two halves of a solution are supplied by two different people, and something quite wonderful comes out of it.

R & S: *Many mathematicians have a very particular work style when they work hard on certain problems. How would you characterize your own particular way of thinking, working, and writing? Is it rather playful or rather industrious? Or both?*

Lax: Phillips thought I was lazy. He was a product of the Depression, which imposed a certain strict discipline on people. He thought I did not work hard enough, but I think I did!

R & S: *Sometimes mathematical insights seem to rely on a sudden unexpected inspiration. Do you have examples of this sort from your own career? And what is the background for such sudden inspiration in your opinion?*

Lax: The question reminds me of a story about a German mathematician, Schottky, when he reached the age of seventy or eighty. There was a celebration of the event, and in an interview like we are having, he was asked: "To what do you attribute your creativity and productivity?" The question threw him into great confusion. Finally he said: "But gentlemen, if one thinks of mathematics for fifty years, one must think of something!" It was different with Hilbert. This is a story I heard from Courant. It was a similar occasion. At his seventieth birthday he was asked what he attributed his great creativity and originality to. He had the answer immediately: "I attribute it to my very bad memory." He really had to reconstruct everything, and then it became something else, something better. So maybe that is all I should say. I am between these two extremes. Incidentally, I have a very good memory.

Teaching

R & S: *You have also been engaged in the teaching of calculus. For instance, you have written a calculus textbook with your wife Anneli as one of the coauthors. In this connection you have expressed strong opinions about how calculus should be exposed to beginning students. Could you elaborate on this?*

Lax: Our calculus book was enormously unsuccessful, in spite of containing many excellent ideas. Part of the reason was that certain materials were not presented in a fashion that students could absorb. A calculus book has to be fine-tuned, and I didn't have the patience for it. Anneli would have had it, but I bullied her too much, I am afraid. Sometimes I dream of redoing it because the ideas that were in there, and that I have had since, are still valid.

Of course, there has been a calculus reform movement and some good books have come out of it, but I don't think they are the answer. First of all, the books are too thick, often more than 1,000 pages. It's unfair to put such a book into the hands of an unsuspecting student who can barely carry it. And the reaction to it would be: "Oh, my God, I have to learn all that is in it?" Well, all that is not in it! Secondly, if you compare it to the old standards, Thomas, say, it's not so different—the order of the topics and concepts, perhaps.

In my calculus book, for instance, instead of continuity at a point, I advocated uniform continuity. This you can explain much more easily than defining continuity at a point and then say the function is continuous at every point. You lose the students; there are too many quantifiers in that. But the mathematical communities are enormously conservative: "Continuity has been defined pointwise, and so it should be!"

Other things that I would emphasize: To be sure there are applications in these new books. But the applications should all stand out. In my book there were chapters devoted to the applications, that's how it should be—they should be featured prominently. I have many other ideas as well. I still dream of redoing my calculus book, and I am looking for a good collaborator. I recently met someone who expressed admiration for the original book, so perhaps it could be realized, if I have the energy. I have other things to do as well, like the second edition of my linear algebra book, and revising some old lecture notes on hyperbolic equations. But even if I could find a collaborator on a calculus book, would it be accepted? Not clear. In 1873, Dedekind posed the important question: "What are, and what should be, the real numbers?" Unfortunately, he gave the wrong answer as far as calculus students are concerned. The right answer is: infinidecimals. I don't know how such a joke will go down.

Heading Large Institutions

R & S: *You were several times the head of large organizations: director of the Courant Institute in 1972–1980, president of the American Mathematical Society in 1977–1980, leader of what was called the Lax Panel on the National Science Board in 1980–1986. Can you tell us about some of the most important decisions that had to be taken in these periods?*

Lax: The president of the American Mathematical Society is a figurehead. His influence lies in appointing members of committees. Having a wide friendship and reasonable judgement are helpful. I was very much helped by the secretary of the American Mathematical Society, Everett Pitcher.

As for being the director of the Courant Institute, I started my directorship at the worst possible time for New York University. They had just closed down their School of Engineering, and that meant that mathematicians from the engineering school were transferred to the Courant Institute. This was the time when the Computer Science Department was founded at Courant by Jack Schwartz. There was a group of engineers that wanted to start activity in informatics, which is the engineers' word for the same thing. As a director I fought very hard to stop that. I think it would have been very bad for the university to have had two computing departments—it certainly would have been very bad for our Computer Science Department. Other things: Well, I was instrumental in hiring Charlie Peskin at the recommendation of Alexander Chorin. I was very pleased with that. Likewise, hiring Sylvain Cappell at the recommendation of Bob Kohn. Both were enormous successes.

What were my failures? Well, maybe when the Computer Science Department was founded I should have insisted on having a very high standard of hiring. We needed people to teach courses, but in hindsight I think we should have exercised more restraint in our hiring. We might have become the number one computer science department. Right now the quality has improved very much—we have a wonderful chairwoman, Margaret Wright.

Being on the National Science Board was my most pleasant administrative experience. It's a policy-making body for the National Science Foundation (NSF), so I found out what making policy means. Most of the time it just means nodding "yes", and a few times saying "no". But then there are sometimes windows of opportunity, and the Lax Panel was a response to such a thing. You see, I noticed through my own experience and that of my friends who are interested in large scale computing (in particular, Paul Garabedian, who complained about it), that university computational scientists had no access to the supercomputers. At a certain point the government, which alone had enough money to purchase these supercomputers, stopped placing them at universities. Instead they went to national labs and industrial labs. Unless you happened to have a friend there with whom you collaborated, you had no access. That was very bad from the point of view of the advancement of computational science, because the most talented people were at the universities. At that time accessing and computing at remote sites became possible thanks to ARPANET, which then became a model for the Internet. So the panel that I established made strong recommendation that the NSF establish computing centers, and that was followed up. My quote on our achievement was a paraphrase of Emerson: "Nothing can resist the force of an idea that is ten years overdue."

R & S: *A lot of mathematical research in the U.S. has been funded by contracts from DOD (Department of Defense), DOE (Department of Energy), the Atomic Energy Commission, the NSA (National Security Agency). Is this dependence of mutual benefit? Are there pitfalls?*

Lax: I am afraid that our leaders are no longer aware of the subtle but close connection between scientific vigor and technological sophistication.

Personal Interests

R & S: *Would you tell us a bit about your interests and hobbies that are not directly related to mathematics?*

Lax: I love poetry. Hungarian poetry is particularly beautiful, but English poetry is perhaps even more beautiful. I love to play tennis. Now my knees are a bit wobbly, and I can't run anymore, but perhaps these can be replaced—I'm not there yet. My son and three grandsons are tennis enthusiasts so I can play doubles with them. I like to read. I have a knack for writing. Alas, these days I write obituaries—it's better to write them than being written about.

R & S: *You have also written Japanese haikus?*

Lax: You're right. I got this idea from a nice article by Marshall Stone—I forget exactly where it was—where he wrote that the mathematical language is enormously concentrated, it is like haikus. And I thought I would take it one step further and actually express a mathematical idea by a haiku. (See Peter Lax's haiku below.)

R & S: *Professor Lax, thank you very much for this interview on behalf of the Norwegian, the Danish, and the European Mathematical Societies!*

Lax: I thank you.

Speed depends on size
Balanced by dispersion
Oh, solitary splendor.