An interview with
Peter D. Lax

Conducted by Philip Colella
On
14 November, 2003
University of California, Los Angeles
and
24 April, 2004
Courant Institute, New York University

Interview conducted by the Society for Industrial and Applied Mathematics, as part of grant # DE-FG02-01ER25547 awarded by the US Department of Energy.

Transcript and original tapes donated to the Computer History Museum by the Society for Industrial and Applied Mathematics

ABSTRACT:
Peter Lax speaks with Phil Colella about a range of topics from his distinguished career in computing and numerical analysis. During World War II, Lax spent a year at Los Alamos, which he describes as a nearly ideal intellectual environment. There, John von Neumann was looking to computing to help solve atomic bomb calculations, and Lax began programming what computers existed at the time. He then went to the Courant Institute at NYU, where he remained for the rest of his career. He developed the celebrated Lax equivalence theorem in the early 1950s. Lax then moved to shock capturing and conservation laws. Among the strongest influences on Lax was von Neumann, who helped Lax upon his initial arrival in the U.S. in 1941 and continued to help him in his early career. A number of Lax's students have enjoyed very successful careers, including Ami Harten, Burt Wendroff, and Alexandre Chorin. Lax also discusses his interaction with a number of current and former Courant colleagues, including Paul Garabedian, Tony Jameson, and Marsha Berger. For most of the 1970s, Lax was director of the NYU computing center, and he was instrumental in acquiring a CDC 6600 from the AEC. Lax and several colleagues actually saved the 6600 machine from being destroyed in the aftermath of an antiwar protest in which the machine had been taken hostage. He also led the very influential Lax Report, completed in 1982, which noted the lack of supercomputing facilities in universities and argued that scientists without access to supercomputers were eschewing problems requiring computational power and jeopardizing American scientists' leadership in certain research areas. Mathematics will continue to be important to computing, Lax believes, and he thinks that computational mathematics has a very bright future.

COLELLA: This is Friday, November 14, 2003 at UCLA. This is Philip Colella interviewing Peter Lax for the SIAM Numerical Analysis Oral History Project. So, thank you Peter. So, I'd like to start out by asking you a little bit about where you first got started in computing.

LAX: You probably remember that I spent a year at Los Alamos during the war. I came there in 1945, and then when I got my degree in 1949 I took a staff position at Los Alamos for a year. And by that time [John] von Neumann had started to spend a lot of time there and the focus of his interest was numerical schemes. We didn't have a computer yet. You remember that – or you may not remember that – already during the war von Neumann realized that to do bomb calculations analytical methods were useless and one needed massive computing. He also realized that computing is good not only for bomb making but for solving any large-scale scientific or engineering problem. And he also realized that it's not only used for schemes for solving concrete problems but rather to explore which way science should be developed. And there's a nice quote, "It gives us those hints without which any progress is unattainable, what the phenomena are that we are looking for." I coined a capsule summary of that, computing the tool for the theorist used in the manner of the experimentalist. So, when I got back in 1949 there was a great deal of research on different schemes for partial differential equations, but it wasn't until 1952 that von Neumann's machine the JOHNIAC or something like that, MANIAC, was built simultaneously in many places: at the Institute for Advanced Study, and also Illinois, also Argonne and in particular at Los Alamos with [Nicholas C.] Metropolis and [James] Richardson most deeply involved in it.[1] And then I started to make my experiments on shock capturing. I remember writing machine code for the MANIAC. I had some help also.

COLELLA: You actually got in there and programmed these naked machines?

LAX: I actually programmed, yes. Later on I used assembly language, but still pre-FORTRAN.

COLELLA: So you were experimenting with shock capturing at Los Alamos in the late 1940s – well, I guess when the first computer arrived.

LAX: Yes. And then I came back every summer after that and each summer there was a brand new machine there which made last year's model incredibly primitive. Of course these machines are primitive, incredibly primitive, by our standards, but it would do more and more and more.

COLELLA: So how long were you on staff at Los Alamos?

LAX: I think about a year, 1949.

[1] The name of the machine von Neumann completed in 1952 was the MANIAC-1.

COLELLA: Okay. And then after that you were back at NYU?

LAX: I was back at NYU.

COLELLA: One of the things in reading [Floyd] Richtmyer, the original Richtmyer, is he talked about a seminar in 1953 in which you presented your first version of the Lax equivalence theorem. And I'd like to ask you to talk a little bit about the things that led up to you looking at that issue and then the context.

LAX: Well, I was very much influenced by work by Fritz John, I think maybe 1952, on parabolic equations. Fritz in his work emphasized the importance of stability. In his case he used stability in the maximum norm, which is natural for parabolic problems, but for hyperbolic problems the L2 norm is more natural. To prove that stability and consistency imply convergence was a very simple argument, but it's a very general theorem. For instance, it includes such theorems as the dot-product formula, many, many things. Now the necessity of it – I was moved to include that because I heard a lecture at Los Alamos, the speaker asserted, and I suspected he was not alone in his belief, that the blow-up of unstable schemes is due to the magnification of round-off errors. And if you had a machine, which of course you can never have, which calculates with infinite accuracy, even a non-stable scheme would converge. So, I wanted to explode that idea.

COLELLA: I see. What I've heard is that there was a lot of confusion about stability at that time – someone else, John Bell, talked to Don Peaceman about that, when he was at Exxon – and that was a enormous problem for that community as well. Did you have any contact with those folks?

LAX: Well, I learned of the Peaceman-Rachford scheme early on and found it quite remarkable. It was a good idea. And I guess David Young was in there early.

COLELLA: Looking at another theme that you've seen here – and I'm trying to fit together the pieces – was von Neumann's notion of stability, Fourier stability, of schemes. Where did that fit into the puzzle, both in terms of the time and in terms of the conceptual framework?

LAX: Well, von Neumann saw the importance of stability and furthermore he was able to analyze it. His analysis was rigorous for equation's response to coefficients, linear analysis usually tells the story. And he conjectured but left open the problem that if you applied the von Neumann criterion locally, by freezing the coefficients, and if the scheme passes the test at each point, then the whole scheme would be stable. And this was shown to be true. Oh, you had to put down a few minor but important restrictions, but von Neumann was right, by and large right. I have made contributions to this, so has Heinz Kreiss, Burt Wendroff and I. Louis Nirenberg and I had a paper which has a section on stability for difference situations – that was Louis's only brush with this subject.[2]

[2] P. Lax and L. Nirenberg, On the stability for difference schemes: a sharp Gårdings inequality, Comm. Pure Appl. Math. 19 (1966), 473-492.

COLELLA: So that was what in the 1960s, I believe?

LAX: That was in the 1960s. [Kurt] Friedrichs had an important contribution about positive schemes.

COLELLA: Yes. Any other comments about this particular area of stability, the Lax equivalence sorts of ideas?

LAX: Well, there is of course a new wrinkle if you consider not pure initial value problems but mixed initial value problems. And here this is a tricky subject both for differential equations and for difference approximations. The problems are always trickier for the difference approximation. In both cases you…for a partial differential equation it's called *a priori* estimates for difference equations it's called stability, but it's the same thing, of course. So, there Friedrichs had a class of well-posed boundary value problems for so-called symmetric schemes and Heinz-Otto Kreiss had a not much more general theory, which was also found out at the same time by a Japanese mathematician, who was a woman. I forget her name, but Heinz could tell it. In Japan things are much more backward for women in science and everything, so it was a singular event. Maybe I should say a few words about solving nonlinear equations –

COLELLA: Yes. That's what I was going to ask you about next, conservation laws.

LAX: Conservations laws. Well, when in the middle of the 19th century it was observed that solutions of equations of compressible fluids developed singularities, it rang an alarm bell; after all, the equation seemed so plausible. It was a genuine crisis. People like [George] Stokes and others were baffled. And the right answer was found by no less a mathematician than [G. F. Bernhard] Riemann. Riemann observed that the physical laws, as they originate, are integral conservation laws and from them we derive differential equations, which are equivalent to the integral equations for solutions of the integral equations which are differentiable, or at least continuous. But integral equations have perfectly good discontinuous solutions. Riemann went further and derived the laws of propagation of these discontinuities, which today are called the Rankine-Hugoniot relations. Really they should be called Riemann-Rankine-Hugoniot relations. Riemann made a little mistake. The mistake was in thermal dynamics, which was not yet a science at this time. And then von Neumann came up with the idea of shock capturing, as it is called today, which means that you do not record and keep track of a shock as a distinct discontinuity, but you regard it instead as a rapid transition. My contribution to this point of view was that if you do that, then you have to set up a difference equation, which is in conservation form, which is consistent with the original conservation loss. That was a very useful observation.

COLELLA: When did you first come up with that?

LAX: Oh, 1952.

COLELLA: Did you actually try it out?

LAX: Oh yeah. I had tried it out and it worked. The calculations are in that paper, which was published in 1954 by CPA [*Communications on Pure and Applied Mathematics*].[3]

COLELLA: Right. I guess further on down the line, a little later, there were two other…well, there are a couple of issues here. One is at approximately the same time –

[Someone comes into room and they stop tape]

COLELLA: I think we're back on the air again. So, here's an obvious question: conservation form was a really important conceptual breakthrough and change, what led you to it? What made you think to do that?

LAX: Well, because that's what is behind the success of shock capturing: that one could actually prove that if an equation in conservation form converges strongly then it satisfies, in the weak sense, the differential equation. That was not anything special to the conservation laws of fluid dynamics; it was true for all hyperbolic conservation laws. And then I discovered that other things are also true in general that were –

COLELLA: So, you had the proof of weak convergence in mind when you first developed this in 1952?

LAX: Well, strong convergence –

COLELLA: Yes, strong convergence to weak solutions.

LAX: Weak solutions, that's right. Weak convergence is another story –

COLELLA: Yes, indeed.

LAX: I got into that later.

COLELLA: But strong convergence to weak solutions was part of your thinking, even back then.

LAX: Yes, but even in my very first paper I was able to prove what then seemed rather remarkable compactness of solutions. There I used weak convergence versus strong convergence. It's in the last section of that paper; it's quite amusing. So, the result was that the number of people who were willing to work on the equations of compressible fluids was always limited; the number of people who were willing to work on hyperbolic systems of conservation laws was much larger. [laughter]

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[3] Peter D. Lax, "Weak solutions of nonlinear hyperbolic equations and their numerical computation," *Comm. Pure Appl. Math*, v.7**,** 1954, pp.159-193.

COLELLA: So, from the conservation form issues in the early 1950s, one of the next stops along the road here was looking at second-order methods for these things ?

LAX: Yes.

COLELLA: So you have shocks, why second-order? What were you thinking?

LAX: Well, you want greater accuracy, but even more you want greater resolution. I defined a concept of resolution. If you take a difference method and you consider a set of initial value problems of interest, which in practice could be some ball in L1-space, anything will do, and then you look at the states into which it develops after the unit-time, any given time, that's another set. The first surprise is that this set is much smaller for nonlinear…for linear equations where time is reversible, the size of this set is roughly the same as the original set. For nonlinear equations, which are not reversible and where the wave information is actually destroyed, it's a much smaller set. And the measure of the set that is relevant is what's called entropy or capacity with respect to some given scale delta. So the first thing to look at is what is the capacity or entropy of this set of exact solutions. Then you take a numerical method, you start, you discretize the same set of initial data, then you look at what you get after time t goes to whatever the test time was. A method has a proper resolving power if the size of this set is comparable to the size of the exact solution; if it's very much smaller it clearly cannot resolve. And first-order methods have resolution that is too low, and many details are just washed out. Second-order methods have better resolution. In fact, I was trying to – well, I want to bring up the question: could it be that methods that are even higher order (third, fourth) have perhaps too much resolution, more resolution than is needed? I just bring this up as a question. There are some people who swear by fourth-order methods. I don't know how it fits into your religion.

COLELLA: I'm agnostic – I wait for results. On this notion of resolution, is that written up somewhere?

LAX: Yes, it's written up in a conference proceeding, one of the Wisconsin conference proceedings and the year is, I don't know, maybe 1968.[4] Ami Harten said it's a good notion, but I did nothing with it. [laughter]

COLELLA: And then let me go a little farther in time. There's a paper by you, Ami, Mac [James M.] Hyman, and Barbara Keyfitz on methods for scalar equations and discussions of monotone schemes [5]

LAX: Yes.

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[4] The date is actually 1978. Peter D. Lax, "Accuracy and resolution in the computation of solutions of linear and nonlinear equations," *Recent advances in numerical analysis (Proc. Sympos., Math. Res. Center, Univ. Wisconsin, Madison, Wis., 1978)*, pp. 107-117*, Publ. Math. Res. Center Univ. Wisconsin, 41, Academic Press, New York-London,* 1978.

[5] Harten, Ami; Hyman, J. M.; Lax, Peter D.," On finite-difference approximations and entropy conditions for shocks," with an appendix by B. Keyfitz, *Comm. Pure Appl. Math*, v. 29, no. 3, 1976, pp.297-322.

COLELLA: Again, another important step in the road that led us to what is the current state of the art. So tell us a little bit about that.

LAX: Gosh, I should have looked at that paper. I remember one peculiar thing we found that if the flux function was not convex, then a perfectly innocuous difference scheme could give the wrong shock.

COLELLA: Yes, yes that's right. And the monotone schemes sufficiently strongly defined and with the appropriate time-step control avoid that problem. This leads again into another line of investigation that strictly speaking is not numerics but has so much influence, which is – again, start from the mid-1950s, your work on the analysis of the Riemann problem and then Glimm's method and your work with [James] Glimm on decay of solutions and then further. So let's talk about that a little bit.

LAX: Well, Glimm's idea, which was in 1965, has been really a landmark in this field. It provided estimates that led to renormalization groups. And that's just what is missing in the theory for two dimensions. It would take another Glimm, perhaps, to come up with such a clever idea. In fact it's sort of a scandal of the theory that nothing is known for flows with shocks in two dimensions. We have a great deal of numerical experience, and indeed this experience is all to the good. There are a number of test problems like the Riemann problem or flow with a partially obstructed channel that have been calculated repeatedly by three, four, five quite different methods. And the resulting flows are remarkably in agreement. So they must be the truth. So the truth exists, but why can't we prove it?

COLELLA: A good question. Okay, let me go back to a little earlier – to the 1957 paper on the Riemann problem.[6] In that paper there are two things. One is the general formulation – at least in my reading, that was quite new – and also this breakdown into generally nonlinear, linearly degenerate. Did you suspect or know that there were other things besides that?

LAX: No, I knew that shocks and contacts were very different and so I was interested in putting this not only in the context of fluid dynamics but in a general context. And there was a natural way of doing that. A little bit of surprise was perhaps that for weak shocks the deviation from the rarefaction rate was of third order. In fluid dynamics the statement is that entropy changes in third-order shock steps. And that turns out to be not a theorem of fluid dynamics but just a general algebraic fact about solutions of systems of conservation laws – that surprised me.

COLELLA: Yes. Okay. You actually worked on random-choice ideas, Glimm's-method ideas, and got to look at decay of solutions with Jim [Glimm]. Could you say a little bit about what the nature of that collaboration was?

[6] Peter D. Lax, "Hyperbolic systems of conservation laws. II," *Comm. Pure Appl. Math.*, v. 10, 1957, pp. 537-566.

LAX: It's a paper that appeared in the memoirs of the American Math Society.[7] It was a hundred pages and it is very painful to read. I assure the reader it was even more painful to write. Fortunately, there is a simplified version in the dissertation of Katrina (what's her name?), a student of [Constantine] Dafermos who used a notion developed by Dafermos of generalized characteristics. I'll think of her name.[8]

COLELLA: Out of that line of work – the Riemann problem, the Glimm's method things – out of that came TVD [Total Variation Diminishing] schemes because of the analysis methods. The use of the Riemann problem taught us not to scared by that as a numerical thing led to the higher-order [Sergei K.] Godunov things, generalizing Godunov's work. So there's a lot of rain that comes out of the clouds of the theory. I wanted to invite you to comment on that phenomenon; to what extent do you think that it's a general one for computing?

LAX: Well, the work of Godunov was very important. I'm sure that's how the Soviets built their atomic weapons. But it was one of the basic ingredients of Glimm's work, too.

COLELLA: Okay, I'd like to change what we're talking about a little bit, and talk a little bit more about people and their influence. I'm going to go through this in some sense by age, by chronology, looking at people older than you, who appear to have influenced you. At the top of the list, of course, is von Neumann –

LAX: Yes, yes.

COLELLA: What was the nature of your interaction with him, particularly when you were first starting out in the late 1940s and early 1950s?

LAX: Well, he was always very kind to me. My introduction to him was preceded by…when I came to the United States from Hungary as a refugee – rather late, at the end of 1941 – I was fifteen and a half. I had been tutored in mathematics. I knew quite a bit and had solved some problems in high school context, and my mentors in Budapest wrote to von Neumann that he should look after me. And he has. Shortly after we arrived he interviewed me, and then at Los Alamos when we met again he was always available. When I latched on to some of his ideas like von Neumann criterion, stability, shock capturing, he was interested in my ideas and I profited from his ideas.

COLELLA: Another person, Friedrichs.

LAX: Yes, well from Friedrichs I learned…he has a general theorem about positive schemes that I had used in my very first numerical experiments. Those positive schemes are of first-order accuracy and therefore lack resolution. But very much later, in the 1990s, I had a notion of how to combine positivity with higher-order accuracy. That first

[7] Glimm, James and Lax, Peter D, "Decay of solutions of systems of nonlinear hyperbolic conservation laws," *Memoirs of the American Mathematical Society, No. 101*, American Mathematical Society, Providence, R.I., 1970, 112 pp.

[8] This is most likely a reference to Konstantina Trivisa.

runs counter to a theorem, which says that it cannot be done, but if you employ the idea of switching from a higher-order scheme to a lower-order scheme then the solution lacks the kind of smoothness that justifies the higher-order scheme. Then, one can devise schemes, which to be sure are nonlinear, that are nevertheless positive in some sense. Xu-Dong Liu and I – it was mainly Xu-Dong Liu who came up with the specific schemes – we were able to devise a number of so-called positive schemes that work extraordinarily well. So that was Friedrichs's influence on me. Curiously, Friedrichs was interested in positive schemes merely as tools for proving existence. He was not interested in doing computing.

COLELLA: So another piece of work that I'm familiar with that touches on these topics was – Frederichs is cited extensively in the MHD [magneto- hydrodynamics] literature for his work on jump relations.

LAX: That's absolutely right. He analyzed Lundquist equations [ideal, nondissipative form for the equations of MHD] and worked out the shock-wave theory. It's a two- or even three-dimensional theory – you have six different propagations meeting the corresponding shock waves and the line ends up…very complicated. Freidrichs was the first to work that out.

COLELLA: Was that example in your mind when you did your work on the Riemann problem?

LAX: Actually not, actually not.

COLELLA: Another person who you actually wrote a paper with [Xu-] Dong Liu dedicated to, was [it] Robert Richtmyer? [9]
LAX: Yes.

COLELLA: He was someone who was very much in evidence in a lot different ways. I'd like to hear you views on him.

LAX: He appears on the scene in 1949, 1950, in a joint paper with von Neumann where they combined shock capturing with artificial viscosity. That worked very well and dominated shock-wave calculations in the 1950s. Maybe I should add the name of Marshall Rosenbluth, who added a rather important detail, namely that once you turned on the artificial viscosity only compression, not rarefaction is important. Marshall Rosenbluth had another very big discovery. It goes by the name of the Metropolis algorithm in statistical mechanics, which has been declared the number one algorithm, but it's really due to Rosenbluth. He and I go way back to high school. We were classmates at Stuyvesant [High School]; we were on the math team. That goes back sixty years. He just died.

COLELLA: Yes I know. So okay, but Richtmyer was also at the Courant for a while –

[9] The paper with Liu is dedicated to Tony Jameson. The paper with Harten and Hyman (above) is dedicated to Richtmyer.

LAX: Yes, yes. [Richard] Courant had the bright idea to interest Richtmyer in coming to New York, to the Courant Institute. He did that. He was there for about ten years. We wrote that joint paper, he wrote his book, he had a number of very good students, and he investigated instabilities. He was a very strong influence on the field.

COLELLA: What was the joint paper you wrote with him?

LAX: I think that was the paper where the equivalence theorem appeared in print. [10]

COLELLA: Okay, Los Alamos. I'd like to hear a little bit about your reflections on that. Let's start with the question that I wanted to…you said when you first were there that you actually programmed some of these machines and you worked with someone in programming them. Who was that?

LAX: Lester Baumhoff. He's an enormously talented guy. In a way, a wasted life compared to what he could have accomplished, but his personal difficulties were too great.

COLELLA: So what was the environment like there just after the war?

LAX: I would say ideal. You may remember – not you personally but you probably read about it – that when the war was approaching scientists, in particular [Leo] Szilard, [Albert] Einstein, [Edward] Teller, [Enrico] Fermi, tried to warn the United States government of the possibility of nuclear weapons. There was a very famous letter by Einstein, which was actually written by Szilard, that was delivered to [Franklin] Roosevelt. Roosevelt gave it to the wrong person, unfortunately, a man named [Lyman J.] Briggs, who was head of the Bureau of Standards, who just sat on it and did nothing. It was then that the British – in fact, [Rudolf] Peierls and [Otto] Frisch – who first calculated the amount of uranium-235 that was needed for a bomb.[11] It turned out to be a relatively small quantity, so that showed that it was possible and finally that lit the fire under the Americans. They started in 1942, whereas Germans started in 1939 and they got nowhere. Anyway, I mention this background because the military realized that they blundered by not recognizing the importance of science and they decided never again to expose themselves to such danger. Their respect for science grew enormously and enormous resources were put at the disposal of scientists, and they were wise enough to see that it did not have to be devoted to a specific task, but general research had to be supported. And that spirit still lives on, although not nearly to the same extent that it did then. Then Russians got ahead of us, and Sputnik helped too.

COLELLA: So this led to a very congenial atmosphere at Los Alamos to work?

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[10] P.D. Lax; R.D. Richtmyer, Survey of stability of linear finite difference equations, Comm. Pure and Appl. Math., vol 9, 1956, pp. 267-293.

[11] Peierls and Frisch had fled Germany and were living in Britain, but they were not yet British citizens at the time.

LAX: Yes, yes.

COLELLA: You mentioned something here that reminds of something else that's a question that comes up now and again that maybe you have the answer to. One of the things that is said in lots of places, including what we write about math program at DOE [Department of Energy], was that in the early 1950s, at the suggestion of von Neumann, the AEC [Atomic Energy Commission] math program was begun. I'm not quite sure where that citation comes from. Could you shed a little light that?

LAX: I'm certainly not aware of a paper trail. But you remember that von Neumann was only one of the five Atomic Energy Commissioners, so it's probably in the archives of the Atomic Energy Commission. One person that looked at that material is von Neumann's biographer, Norman (what's his name, Englishman, I'll think of his name) and there may be something about that in there. One could take a look in there.[12]

COLELLA: Okay, I should try to make some effort to try to track that down. So who were some of the people at Los Alamos, mathematicians that you talked to and were influenced by your first 1950s?

LAX: Well I talked to a lot, to Stan Ulam. An important person there was and is Frank Harlow. He is not nearly as well known as he should be. It's his own fault: he never went to meetings and didn't publish much either, but he had very original ideas: the PIC [Particle-In-Cell] method and then later he had original ideas in turbulence.[13] We were friends and I've talked to him. Later on J. U. Brackbill was a source of ideas.

[End of Tape One, Side A – Beginning of Tape One, Side B]

COLELLA: You mentioned Marshall Rosenbluth in the context of the Metropolis algorithm. Did you have any interaction with Metropolis?

LAX: Yes, yes. He was very supportive of calculations and he made the machine available and he was interested in the result. His own work, his most interesting work other than machine design was with Paul Stein and others, experimental calculations which revealed some unexpected universal patterns. There was a tradition at Los Alamos not to publish – a deplorable tradition.

COLELLA: Someone else that I can't forbear asking about is Edward Teller.

LAX: Well, of course blood is thicker than water, so we were friends. [laughter]

COLELLA: Did you have any interaction with him in your early days at Los Alamos?

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[12] Norman MacRae is the biographer referenced here. Norman MacRae, *John von Neumann*, 1992, Pantheon Books, New York.

[13] Lax here is perhaps downplaying Harlow's publication record: Harlow did publish over 150 papers during his career.

LAX: I met him once during the war. Most of our interaction was afterwards. There is a quip by his friend, Enrico Fermi, who said, "Edward is a monomaniac with many manias." [laughter] Teller had a very fertile imagination and had many ideas. A few of them were very good and a lot of them were not good. However, he was perfectly open about it and willing to discuss it with his colleagues. And if [someone] brought up good arguments trying to argue, he would admit it. After his enormous success with the hydrogen bomb, he rose and operated among people who were unable to give him an argument. So some of his ideas which were wrong, such as Star Wars, were not shot down before they did a lot of damage.[14] I think that was Edward's tragedy.

COLELLA: Yes. Let's move on now to the [Courant] Institute in the 1950s. Again looking at Richtmyer's book, he talks a lot about, he devotes a lot of space in the introduction to the people who were there and the general kind of level of (he didn't use this word) ferment. So there were…who were some of the people: [George] Forsythe, [Wallace] Givens, [Eugene] Isaacson, Herb Keller, the Todds [Jack Todd and Olga Taussky-Todd]. So what was the atmosphere like scientifically? What was the nature of the interaction of the people?

LAX: Well, computing was simply a new dimension in doing mathematics, and Courant realized that. So he used as much influence as he could command to have the Atomic Energy Commission place a computer at the Courant Institute. That happened first in 1954 when the famous UNIVAC was installed.

COLELLA: So that very widely circulated photo with all of those older distinguished gentleman and a much younger man on the left… [laughter]

LAX: Right, right. You know, I feel I was too shy to ask General [Douglas] MacArthur, in the presence of General [Leslie] Groves, "While you were planning your campaigns in the Pacific did you know that the atomic bomb was being prepared?"

COLELLA: Missed opportunity. [laughter]

LAX: Missed opportunity.

COLELLA: A name we haven't touched on yet is Courant – his impact on computing.

LAX: Well, that famous paper in 1928 – the Courant-Friedrichs-Lewy paper – where the CFL condition was introduced for the first time. I happen to know it was [Hans] Lewy's observation, actually. But it was –

COLELLA: Why did they look at this? This was before computers were even a glimmer in anyone's eye.

[14] Star Wars was the nickname of a U.S. missile defense system developed in the 1980s and 1990s. It never became operational.

LAX: Yes, that is absolutely true, and I can't answer that. I should…all three of them are gone. How did they look at it? They were not that much interested in practical computation, it was more the theory. [Kurt] Friedrichs and Lewy were at that time just turning to proving the existence of solutions and difference methods played an important role in that, so that was their motivation. I don't know. I should know, I had a chance to find out, but again another missed opportunity.

COLELLA: So Courant clearly showed a great deal of interest much later in supporting computing, and viewed this as an opportunity then. Did you know Lewy at all?

LAX: I knew him a little bit. Better near the end of his life; we became quite friendly then. He had a very mercurial temperament. I remember way back during the war he spent half his time at Courant. And he told Charlie DePrima, who was then Courant's assistant, that he, Lewy, and Friedrichs had worked together so long and their minds were so much in tune with each other that they could communicate just a few words, practically pure thought. And Charlie was impressed, except his office was next to Friedrichs's – the walls were very thin – and that very morning he could hear Lewy screaming at Friedrichs, "Do idio! ??You idiot??" [laughter]

COLELLA: Okay. The 1950s is also when you started taking students. I got a list of your students off the Web and circled ones that (I think) I could identify as numerical ones, or ones that worked on computing. So Milt Rose, I guess, was one of your very early students.

LAX: Yes, yes. I think his greatest services were as an administrator. He was a very imaginative administrator and at NSF [National Science Foundation] he established the regional conferences, which have been very influential. And then at ICASE [Institute of Computer Applications to Science and Engineering] he saw to it that there was an influx of summer visitors.[15] So that's two great things.

COLELLA: Burt Wendroff.

LAX: Ah, well he was one of the…I just had two students with whom I continued to work after they did their Ph.D.s, and Burt was one of them. He is very good. The other was Ami [Amiram] Harten. He was one of my favorite students and his premature death has very much affected me. I'd been screaming at him to stop smoking. He was a heavy smoker and he drank twenty cups of coffee a day, and he was overweight, and so…well, I must be very careful. Anyway, it was a great pity. He was just starting an interesting line [of research] on multi-resolution. But he had other very good ideas. And switching from a second order scheme to a diffusive scheme near a discontinuity– that was his Master's dissertation with [Gideon] Zwas[16]. And ENO [Essentially Non-oscillatory] was his idea, I think. He had many, many good ideas.

[15] ICASE no longer exists. It closed on December 31, 2002.

[16] A. Harten and G. Zwas, Self-adjusting hybrid schemes for shock computations, J. Comput. Phys. , 6, 1972, pp569-589.

COLELLA: Another person who is not from Ami's period but from the 1950s is Godunov. He worked on a lot of the same problems that you've mentioned: boundary stability, celebrated Godunov's methods. To what extent were people aware of that work in the West.

LAX: Oh yes. Many of his publications appeared in Russian, but they were translated. He doesn't speak English, but French, but I think his ideas did spread into…became general knowledge. He also had some interesting ideas about writing the equations of fluid dynamics in symmetric form. He's still active. I saw him this spring in Cambridge, England. One of his interests recently has been the notion of pseudospectrum, points where an operator matrix is invertible, where the norm of the resolvent is enormous. It's an idea that has also been taken up by Nick Trefethen. And also set of instability, which is usually attributed to eigenvalues, where there is the wrong sign, a sign of opposite value. He recognized that it can be triggered off by pseudospace – an important idea.

COLELLA: So moving on to the 1960s, the people I know that were there, or I heard that were there, Heinz Kreiss, who we just saw. One of the things I notice is that if I read Richtmyer's book, which was written in the late 1950s, and contrast it with the second edition Richtmyer/Morton, there's a sea change there.[17] I'm not quite sure how to put my finger on it, but my intuition is that it partially reflects the change in scientific interests and also some changes of the people who were around.

LAX: Well, that could be. I've never looked at the second edition, I must confess. Can you characterize it for me?

COLELLA: Oh, I think one of the things that's much more prominently…there's a sense in which, in the original edition there is more of an effort to explain by possibly making some simplifications. In the second edition it's a little more formal. And also the substance of things – the Kreiss matrix theorem didn't exist in 1957, and that really plays a much more prominent role here. So Heinz Kreiss was obviously someone who was influential.

LAX: Yes, that is right. Now I played a small but important role. It was this. His first paper in *Mathematica*,[18] which was a very distinguished and very theoretical journal to which he submitted his paper, and the editor sent it to me asking, "Is this the kind of thing that we should be publishing?" And I said yes. [laughter]

COLELLA: Was he there as a regular member of the staff at that time?

LAX: For a certain period he was a faculty member on half-time. Now in general that doesn't work out so well. Half-time is not really half the value, but he was so good that it was still a plus.

[17] RD Richtmyer and KW Morton, Difference Methods for Initial-Value Problems, Interscience Publishers (J. Wiley & Sons), 1967, New York.

[18] It is not clear if this refers to the matrix theorem which was published by Kreiss in BIT, vol 2 (1962).

COLELLA: Let's see, what other people. So, when Glimm was there in the 1960s did he show any interest in the computational aspects of the work he was doing?

LAX: Yes, he was quite interested in seeing how Glimm's scheme works in practice. That was the last code I wrote – I remember I called it Glimmer – and somehow I gave it to be typed up, but they lost it and I was so angry. I never rewrote it. But it was you who then, under Alexandre's direction –

COLELLA: Alexandre did some of it, and I did a bunch more for my thesis.

LAX: Very much later I wrote a little note analyzing one aspect, which bothered me a little bit in Glimm's original work. You remember Glimm showed that except for a set of small measure, you get the weak form of the conservation equations. But if you look at how small that measure is, it's not that small. It's proportional to maybe delta squared. To make the probability less than one thousandth, you would have to take it down to unrealistically small. And I got a different estimate for the size of that set, and that is exponentially small.

COLELLA: Okay, good. Did that get published somewhere?

LAX: It was published, let's see, it was published in that whole volume that was dedicated to Cathleen Morawetz's seventy-fifth birthday. It was a journal called…I think it's one of the Asiatic journals.

COLELLA: Okay, I'm sure we can find it.[19] Let's see, another thing I heard is that some of the prominent people in linear algebra were at least frequent visitors, so Gene Goloub and Beresford Parlett.

LAX: Yes, Parlett worked for three years at Courant. And Wallace Givens came for two years.

COLELLA: Ah, yes. So what was their influence, what was the nature of interaction with them, with the linear algebra crowd?

LAX: Well, of course I realized that numerical linear algebra breathes new life into linear algebra. It was regarded as a dead subject, and then suddenly new opportunities came and the results. And that revitalized the subject. I do regret, however, one thing. That is that it had a deleterious influence on the teaching of linear algebra. It was taught entirely from the point of view of resolving linear equations. Even as good a man as Gil Strang, in as good a book as his *Linear Algebra*, states in the preface the preposterous statement that the subject of linear algebra is the solution of systems of linear equations.[20] It is

[19] Peter D. Lax, "On the accuracy of Glimm's scheme," *Methods and Applications of Analysis*, v. 7, n. 3, 2000, pp.473-477.

[20] It is not clear whether this is a reference to Strang's *Linear Algebra and Its Applications*, 3rd ed, Saunders (1988) or his *Introduction to Linear Algebra*, Wellesley-Cambridge Press Third Edition (2003).

much more than that. And I've taught linear algebra on and off for thirty years. At Courant, it was a kind of an introductory graduate course. And then near the end of my teaching career I wrote a text on linear algebra, which I believe is a very well balanced book.[21] And if my publisher would promote it a little more it would be widely available.

COLELLA: It's still a controversy, how it should be taught. I recently was trying to send one of my employees to a linear algebra course on the Berkeley campus and there's clearly a fissure.

LAX: Yes, yes. Oh, what's his name, very prominent man, somewhat eccentric –

COLELLA: Velvel [William] Kahan. [laughter]

LAX: Velvel Kahan. A man of strong opinions. He's very brilliant. I presume he taught it.

COLELLA: Yes, they occasionally will let him interact with undergraduates. Again, looking at the 1960s, the other thing is the people who were students there – who were young people, who are not-so-young people now. And of course at the top of my list is I have to ask about Alexandre Chorin.

LAX: Yes, well…one of my favorite students. I cannot even be quite objective about him, but he has done wonderful things. One of his early discoveries – inventions – was the vortex blob method, which was used by many including Charles Peskin and his heart models.[22] That was a great idea. Then for many years he beat his head against the problem of turbulence. He had very good ideas there also.

COLELLA: His early work was on actually finite difference methods for incompressible flow.

LAX: That's right, mainly in vortex.

COLELLA: And so projection methods and artificial compressibility. Were there other people working on that at the Institute at the time or was he off on his own?

LAX: Let's see, Don Goldfarb was there, but I don't quite remember what he was working on at that time. Herb Keller was more just low-key at that time.

COLELLA: Let's see, someone else we have to mention since you're sitting at UCLA, Stan Osher.

LAX: Yes, yes. He was really my student. He did his Ph.D. with Jack [Jacob T.] Schwartz, and it was, seemed to me, a dead-end subject that he worked on, but maybe

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21 Peter D. Lax, Linear Algebra, Wiley-Interscience, 1996, New York.

22 Alexandre Chorin, Vortex sheet approximation of boundary layers, J. Computational. Phys., 27, no.3, 1978, 428-442.

I'm…maybe just temporarily. Anyway, I got him interested in numerical calculations. He did a very clever thing on the stability of difference schemes and initial boundary value problems. He used difference operators in a very imaginative way. But then he got into scientific computing. He worked on a number of numerical methods including elaborated [tape malfunction]. And then he worked on the problem of image reconstruction – blurred images, refocused images – that's a very important subject. I think he even had (or has) a commercial venture in that [Levelset System, Inc].

COLELLA: What did he actually do for this thesis?

LAX: I think it was something in operator theory.

COLELLA: Oh, I see. So you gave him a push in the right direction.

LAX: I gave him a push in the right direction. Not push, pull.

COLELLA: Good. One other person who was a junior person there and now is a senior person at the Institute is Olof Widlund.

LAX: Yes, yes. Now he has been a very good influence, a very steady person. He and Chorin were and are great friends. He found his niche in domain decomposition, and I think he's now regarded as one of the founding fathers in the subject. I like to tease him – in 1991 where there was a big domain decomposition congress in the Soviet Union, and indeed within six months it's domain was decomposed. [laughter]

COLELLA: Right, good. We kind of put an arbitrary cutoff of the late 1970s for this, but it does allow me to slip under the wire a couple of names that I know you think well of. So, Paul Garabedian was not just a very distinguished theoretical mathematician but was someone who really was interested in computation.

LAX: Oh yes, Paul was one of the most original applied mathematicians that I know, also a distinguished pure mathematician. He was among the first to solve the reentry problem. When these intercontinental missiles were first conceived it wasn't clear that they could reenter the atmosphere without being destroyed, as the *Challenger* was, for instance. It shows how tricky it is. And Paul had a procedure for showing that it can be solved. But it would have been better for the world if this problem had no solution.

COLELLA: When was this work done?

LAX: In the 1950s.

COLELLA: Oh, okay. This is very early on.

LAX: Very early on. And then in the late 1960s he turned to the design of shockless airfoils, and that was an enormously original piece of work. He complexified the space coordinates and turned the electric equation to hyperbolic ones. That had the disadvantage that no aeronautical engineer would believe in such a thing, and therefore they refused to test his designs. He had to go all the way to Canada, where he found an aeronautical engineer, originally Polish (I think Aprozinski was his name), who was more sophisticated mathematically and believed Paul and tested it. And indeed it was shockless. And then, of course, everybody jumped on it. He also had other good ideas. He had a very neat analysis of over-relaxation. He's an all around brilliant guy. The last twenty years he has been working on fusion.

COLELLA: I guess that does bring us to the question of the tenth floor folks, I mean the fusion group. To what extent has there been an interaction between their computing and what happens with the other more mathematical things in the building?

LAX: Well, I think Paul Garabedian was the link. I think that is correct to say. Well, maybe Jerry Blackwell [Brackbill?] when he was there.[23] He was part of that group, but he could also talk to me. Another great contribution of Paul Garabedian was he discovered Tony Jameson.

COLELLA: That's the next person I had on the list, Tony.

LAX: Is that right. Tony was working, I think at Grumman [Aerospace], and Paul was consulting for Grumman and he listened to Jameson and saw that he was a brilliant fellow. So he snatched him up and brought him to Courant, made him a professor of computer science. And he developed the most perfect methods for calculating first around air flows and then around larger units and finally around whole aircraft with all the geometrical details. And he's still active, I saw him a week ago at Stanford, where he's a professor of engineering. He's still holding his method. He also has a program top optimize shapes. He's head and shoulders above others. Although, that brings us to Marsha Berger, who also had many brilliant ideas in aerodynamic computing. She had very big successes, and one of them is [modeling of flow over] the C-17, a military aircraft. You probably know that story, so maybe you can correct me if I'm wrong on some detail. It was a transport plane and one of its tasks was to discharge paratroopers. And they found out that there was a danger that they would become tangled in I forget what part of the aircraft… [24]

[There is a knock on the door.]

COLELLA: Just give me five more minutes and we'll wrap this up.

LAX: So something had to be done; the aircraft was already built. Marsha figured out that if they lowered the speed it would save discharging paratroopers from not being tangled.

[23] See reference 11 in Octavio Betancourt and Paul Garabedian, Equilibrium and stability code for diffuse plasmas, Proc. Natl. Acad Sci U S A, April; 73(4): 984-987.

[24] The danger was that two paratroopers exiting opposite doors would become entangled with each other.

COLELLA: Moving to one other student from the 1970s I forgot to ask you about is [James] Mac Hyman.

LAX: Oh yes, he's another one of my favorites. He was and is the most enterprising of my students. Whenever I was at a conference and I posed something, he always came back with something; it was not always what I wanted, but he always did something positive. He concentrated on writing big programs that would generate versatile codes. At that time it was just a dream. In fact, I haven't followed that direction today with big programs and very powerful computers; you know much more about that than I. But he was into that very early on. He did some very good work at Los Alamos and he is a leader.

COLELLA: Yes, he has broken the Los Alamos habit of hiding in New Mexico, being the president of SIAM. [laughter] I have one other question, which is not about people or science, but about something else. One of the reasons for this [question] is for younger people to understand how things are the same and how things are different. It's on the issues of accommodation of funding and government. You have been an administrator – I know you've said that you don't necessarily believe in professional administrators. My impression is that people have to worry at younger and younger ages about where grants are coming from and where resources are coming from. Could you talk a little bit about that issue?

LAX: Yes, I think it's a pity that the sources of funds are…that it's a battle that has to be fought over every year or every three years. I think it makes life less attractive. I can't say that I have the solution to it. One cannot demand of the granting agencies that they should commit themselves.

COLELLA: Did you have to worry about this when you were in your twenties?

LAX: Not when I was in my twenties; then my elders took care of it, although I certainly was aware. I was involved in making presentations. I was not the one who wrote the grants, but I was aware of it. I wrote up chunks of it. I was aware that what was proposed had to fit in a very general way with the aims of the agency. You remember I said earlier that in more of the halcyon period, because the military and all government agencies realized what a vital part what scientists do and think plays for defense and everything else that affects national life. That has somewhat dissipated. But in those days, the administrators had a vision of their own. Well, for instance von Neumann was one them, although on a very high level. Still, a good, understanding administrator plays a very important role. Did I mention Fred Howes?

COLELLA: He was also the head of math program at DOE [Department of Energy] for a while.

LAX: Yes. [Fred] Howes played a very important role. I am a little bit concerned about young people, that they are thrust into the grant game too early – but I don't have a general remedy for that.

COLELLA: Let me mention one, and that's that those of us who are a little older need to take some responsibility. I think we should probably end here. Thank you, Peter. I may actually try to get a little bit of your time the next time when you are in New York, but this has been very helpful. Thank you very much.

LAX: Sure.

[End of Part One, Beginning of Part Two]

COLELLA: This is April 26, 2004, and I'm continuing my interview with Peter Lax for the SIAM Numerical Analysis Oral History project. So when we left off last time, Peter, we started talking about issues having to do with governance and funding and the practical matters working in this environment. You've been involved in this in a lot of different ways – let me just pick a couple of them at random to start. One of them, you were director here at the Institute, were you also director of this computing center at one point?

LAX: Oh, yes.

COLELLA: Yes, that's what I thought, so –

LAX: I was director, when Bob Richtmyer left for Colorado I took over the directorship and I managed it for a number of years. Perhaps my finest contribution was when it became clear that the, as it was called, AEC Center, was losing its usefulness, because the computer we had, which was the CDC 6600, was no longer unique, other people acquired computers and suddenly the 6600 became a white elephant of huge proportions. At that time, Max Goldstein and I had the good idea to see to it that the AEC, turned it over to New York University, and it became the university computing center, where it was very useful.

COLELLA: So when was that?

LAX: Oh, I wish I knew. Sometime in the 1980s, I think.

COLELLA: In the 1980s? That late?

LAX: No, it couldn't be, it can't be. Well, it may have been. I know the deathblow was the establishment for the thermonuclear fusion network.

COLELLA: So that would have been around 1972, 1974.

LAX: Yeah.

COLELLA: Okay, yes.

LAX: We lost a lot of customers that way.

COLELLA: Up until that point though, you had the resource that…I mean, CDC 6600 was a supercomputer in its time, and that was in 1964 through the early 1970s. How much control did you have over the resource?

LAX: We cooperated with the, I forget whether it was still AEC or ERDA [Energy Research and Development Administration], or DOE –

COLELLA: Still AEC at the time, I think.

LAX: AEC. And we had a lot of outside users who, if I remember correctly, came to use it on the premises, the use over the ARPANET was not yet perfected.

COLELLA: Yes. But you still had a lot of users in-house?

LAX: We had a lot of in-house users but that would not have justified –

COLELLA: Yes. Did you have enough of the resource that you could allocate in-house that you could take some chances and do interesting things scientifically?

LAX: Yes. The outside users paid for the usage and that was the basis of the budget for the computing center.

COLELLA: Give me an example of who the outside users were.

LAX: You know, that memory has faded –

COLELLA: That's okay.

LAX: Harold Grad's group was one –

COLELLA: That's right, fusion –

LAX: The fusion people. That's not exactly outside –

COLELLA: Presumably the Princeton plasma –

LAX: Yes, the Princeton people certainly. Did I tell you last time about the occupation of the Institute?

COLELLA: No, no.

LAX: Ah, that's a most romantic story. It happened in 1970, just a few days after the shooting at Kent State University. I don't know if you remember there was a protest there against the war –

COLELLA: Yes –

LAX: A protest there against the war in Vietnam.

COLELLA: I was a freshman in college then.

LAX: And an ill-instructed unit of the National Guard actually shot at and killed maybe four students. It was a national disaster. In fact, let me include a totally irrelevant, true story. When it happened, there was on campus, as on every campus, a McDonald's, which displayed an American flag. And the student leaders told the manager to lower the flag at half-mast, which the manager was glad to do, students were his good customers. And then someone from the American Legion noticed it and said, "No, only the President can order a flag at half-mast." At this point the manager was beyond his depth, but when he was appointed he was given a number, telephone number at headquarters, McDonald's in Washington, to call whenever he has a problem that he cannot handle. So he called that number, man at the other end listened carefully and said, "I'll take care of it". And within half an hour another delivery of hamburger meat came and in backing up, the truck accidentally knocked over the flagpole. [laughter] Now, our connection with the Kent State shooting was that two or three days later there was an occupation at New York University. They were led by a rogue professor of history and a few students – I think there were many outside elements. And they occupied this building and held the computer, the 6600, hostage in exchange for the University paying a hundred thousand dollars bail for the Black Panthers. So it lasted two days, it seemed like an eternity. There were threats and the University didn't want to call the cops because they were afraid of another shooting, that was the clever calculus by the invaders. But the University didn't give in and after 48 hours the students left and I stuck around with other members of the computing center. When they left I could smell smoke so we said, "Let's go in, some damage could come to the machine." What we found was a homemade fuse that was twirled gauze soaked in some kind of flammable liquid, lit. So two of my younger colleagues jumped in and stepped on it while the rest of us went to the machine room and removed the flammable liquids that were hung on the machine. And so the 6600 was saved.

COLELLA: Ah. So they were actually going to blow it up or burn it down or something.

LAX: Yeah, something like that, yes.

COLELLA: Good heavens. This was before or after the math center at Wisconsin was bombed?

LAX: It was a few months before.

COLELLA: Okay, so that wasn't in your minds at the time, that there was real risk.

LAX: Well, afterwards Anneli [Lax's wife] asked me, "Were you crazy to go in there?" [laughter] And I said honestly I was so mad I didn't think, I wasn't thinking.

COLELLA: Who were your two colleagues that were with you?

LAX: Oh, Fred Greenleaf and Emile Chi.

COLELLA: Okay. Another piece of administrative stuff that you have done is the Lax Report.

LAX: Yes, yes, the Lax Report. That came about…you know, the 6600 was the last super computer to be placed at the University. When the 7600 came out, no university housed one and therefore university scientists were in effect barred from using it unless they had contact with someone at Los Alamos or Livermore or the Center of Atmospheric Research in Colorado. And that was an intolerable situation. And so at that time I was in a position to do something because I was serving on the National Science Board. The idea for a report came from Kent Curtis, actually, who was head of computer science and Jim [Ettore M.] Infante[25] strongly supported it. So we made a stink, that is in issuing the report, and there was action on it. My comment on the success was, to misquote Emerson, "Nothing can resist the force of an idea that is five years overdue."

COLELLA: So this was what, 1982?

LAX: 1982.

COLELLA: And by 1985 we had supercomputer centers up and running –

LAX: That's right.

COLELLA: Funded by NSF. And in fact, one other thing that happened – and I'd like to find out to what extent it was in response to this – is that the MFE [Magnetic Fusion Energy] Center became a general office of energy research center. So they broadened their mission, as well, in computing.

LAX: Yes, yes. Also many states established centers. That was a further development. North Carolina I know did.

COLELLA: Ohio. My old boss Bill McCurdy was involved in doing that. And Minnesota, of course.

LAX: Yes, yes.

COLELLA: That's right. How did you make the argument? What was the justification?

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[25] Kent Curtis was program head for computer science at NSF. Infante was head of the Applied Mathematics Program at NSF in 1979-1980 and returned as Director of the Division of Mathematical Sciences in 1981-1984.

LAX: Well, lawyers have a neat Latin expression, “Res ipsa loquitur,” “the case speaks for itself.” Well, it didn’t. We added a few arguments, but they were obvious: that the most creative scientists are at universities and therefore if they don’t have access to supercomputers they will do things that do not involve supercomputers. It was an interesting document. I remember an interesting statistical study that the speed with which – they took a particular problem, I think solving the numerically the discretized Laplace equation – and speed with which that could be done has increased n-fold over a period of thirty years. And the square root of n of that increase was due to increased machine speed. The other square root was increased numerical methods.

COLELLA: Well, in fact I’ve been using that same argument and I can tell you what the current value of square root of n is: it’s about 16 million. [laughter]

LAX: Very impressive.

COLELLA: Yes, and that’s over fifty years. David Keyes and I have been showing around that graph, and it really tracks very closely to a straight line for both. So that’s actually a good reason not just to let scientists have computers, access to big computers, but also for letting mathematicians have access to big computers. I guess that brings me to one more thing about your administrative career: you were also director of the Institute.

LAX: That’s right.

COLELLA: To what extent did issues relating to computing impinge on that part of your administrative career?

LAX: I think it was during that time that I was helpful in turning the AEC Computer Center into the NYU computing center.

COLELLA: So when were you director?

LAX: I was director from 1972 to 1980. It was an inauspicious time to become director because in 1972 University Heights, the engineering school, was closed down. So it was a very hard time at New York University.

COLELLA: Yes, I remember that. And the 1970s were…that was when I was getting my start, but I even get the sense now that they were uneven times for the support of research in that period.

LAX: That’s right. There was a peak in Ph.D. production in 1972. I think that had to do with the ending of exemption from the draft for students in graduate school.

COLELLA: Yes.

LAX: But it did affect the mathematics population.

COLELLA: So, I guess there's another set of questions I want to ask you about that are a little more general. Computation is a discipline that kind of sits between a lot of other traditional disciplines. One thing that I would like to hear your views on is: where should it be in a university setting? Does it have a natural home, and how is that working out?

LAX: Well, of course each university works it out for itself. The computer science departments are not a natural home, for most of them. The one at Courant is an exception because they had people like Tony Jameson and Olof Widlund and Marsha Berger, many people who were deeply interested. [Malvin] Kalos was a member of the department. But that's in the past now, and computer scientists don't compute in the sense of scientific computations. Mathematics is the natural home. At Berkeley, it's in a healthy state in the math department. At Stanford who's interested in computing?

COLELLA: In the math department, at the moment, no one I can think of. I mean, George Papanicolaou, tangentially.

LAX: Yes, yes. At UCLA Stan Osher is deeply interested in it. At Wisconsin…

[noise interferes]

LAX: Maybe that was my computer telling me I have an incoming message. I think it would be worthwhile to look through the leading universities and see how they handle computing.

COLELLA: So the connection between computing and mathematics here has been obviously a salutary one.

LAX: Yes, yes. It can be traced back to Courant, the Courant-Friedrichs-Lewy paper. Although Friedrichs himself was not interested in actually doing computing, somewhat strangely. Courant was, but perhaps he was too old by the time the big machines came in to…although I guess in the 1940s he had the idea of one of the methods as it developed. But by the time that big computers came around he was out of doing technical work on it, but his spirit remained here. Bob Richtmyer. Courant was instrumental in hiring Bob Richtmyer, who then wrote his book, which did a lot to put solving PDEs on the map, so to speak. Isaacson, and Herb Keller wrote another book that was quite influential. Burt Wendroff wrote a book.

COLELLA: By the way, there's one student of yours that in our last conversation I forgot to ask about. It comes up because he's the only person I know who has a computer named after him, and that's Milt Halem. He was a student of yours?

LAX: He was my student, that's right –

COLELLA: Did he work in computing as your student?

LAX: Oh, what was his dissertation? I don't quite remember. I should have looked it up. But then he went to work in…just outside of Washington –

COLELLA: NASA Goddard.

LAX: NASA Goddard.

COLELLA: Yes. And they named one of their big machines after him when he retired. And so I compute on Halem all the time. [laughter]

LAX: I didn't know that. I'm very pleased.

COLELLA: It's a very good machine too, I might add. I guess another area that has a name is now what people call computational science. Again, do you see that as being a real thing? And how do you see the role of mathematics and numerical mathematics in that?

LAX: I think the role of numerical mathematics in computational science is very great because mathematicians have the most fertile imagination in inventing new methods. I think they are better than physicists or chemists. I hope that won't bring the wrath of physicists and chemists on my head.

COLELLA: I doubt it. I think the one thing that I see in this is that in going from a piece of physics or a piece of chemistry or whatever to a computer you have to go through mathematics, a formal model –

LAX: Oh, certainly.

COLELLA: And that's why I don't think we'll ever be out of that game, because in order to do that you have to understand how to reason mathematically about models.

LAX: I would like to amend. Many wonderful methods, computing methods, were invented by physicists like Monte Carlo, the so-called Metropolis algorithm, which is due to Marshall Rosenbluth.

COLELLA: I actually heard there's another person who discovered it independently, which was Berni Alder, whom I just interviewed for this series.

LAX: Yes, yes, oh I believe that.

COLELLA: Yeah, he discovered it while he was a graduate student at Caltech and it never appeared in his thesis because his thesis advisor, [John] Kirkwood, didn't believe in computing. [laughter] But it eventually did get published and Teller, actually, was instrumental in kind of making many people know that this had been discovered independently. Anything else? I mean, anything else you'd like to say about how computational science and numerical mathematics, where you think it's going?

LAX: Well it's quite clear it will be with us forever because new problems will come up, new machines will come up, new computing methods will be invented. I think it will always be on the leading edge of science. In a talk to the American Philosophical Society I made this point: it's a measure of the importance of computing that if you look at Nobel Prizes in physics, more and more of them depended on computing. The first one was perhaps CAT scan. It was honored by Alan Cormack sharing that prize, and his contribution was numerical.[26] Then in 1986 [Herbert] Hauptman and [Jerome] Karle were honored with a Nobel Prize for X-ray crystallography, in which the key idea was a numerical implementation of the Toeplitz/Caratheodory characterization of the Fourier-transfer positive-mass distribution.[27] And Walter Kohn and John Pople shared a Nobel Prize in chemistry for a computational study of large molecules.

COLELLA: So what is this article?

LAX: This was a symposium by the Philosophical Society. It was all of these millennial things, and all areas of knowledge, useful knowledge…you see, twenty minutes to describe what happened in the last hundred years or what will happen in the next hundred years. So that was quite a challenge. I talked about mathematics and computing. It's a good article. I described the importance of experimental computing and how von Neumann first saw that. I said I realize that the majority of the audience doesn't understand the language of partial differential equations. Therefore I translated it into a more familiar but equally concentrated form: a haiku. So this is the haiku.

COLELLA: Please read it.

LAX: 'Speed depends on size, balance by dispersion, oh solitary splendor."

COLELLA: Splendid, very good.

LAX: There are some of Marsha'a calculations and Tony Jameson's. There is that fabulous discovery that the zeros of the zeta function follow the distribution of the eigenvalues of random matrices; it was a numerical discovery.

COLELLA: That's an extraordinary discovery.

LAX: I discuss how much confidence one can place in a calculation. I listed some of the ideas….Strauss's method.

COLELLA: Do you think that…one of the things that's been a recent controversy, or at least discussion, is the extent to which large computations are going to appear increasingly in pure mathematics, in theorems.

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[26] Cormack shared the 1979 Nobel Prize in Physiology and Medicine.
[27] They actually received the 1985 Nobel Prize in Chemistry.

LAX: Yes, yes. That is a very interesting…I think I remark on that also. When I wrote this list, the most striking example was the Four Color Theorem. I remark that the proof was criticized because it does not give insight into why the result is true, but such criticism can be leveled against proofs that don't use computing. I also observed that when logicians were looking for a rigorous definition of a mathematical proof, they came up with Turing's definition of calculation carried out by a Turing machine. [laughter] So calculation carried out by an imaginary computer is the epitome of mathematical exactitude, but that leaves open the option to argue against proofs employing a real computer. But it's a problem that has to be addressed, both problems: very complicated proofs and computer-assisted proofs, how to make them more transparent. On the question of reliability, computer science can be of help. There is program verification, which is a branch of computer science. I know a couple of people in it; I've urged them to apply their wares to the Appel-Haken proof. I don't think anyone did, especially since there were all kinds of glitches, which they eventually corrected.

COLELLA: But then you had this potential for an infinite regress: how do you verify the verification program?

LAX: Sure, sure. Well, what could you say if you have a complicated proof, say the classification of simple groups…at the Phoenix meeting in January I listened to an interesting lecture by [Michael] Aschbacher from Caltech (who was one of the leaders in this effort), who said that there were some gaps in it but now it has been closed, and he showed how. And you could ask the same question: how do we know it is really closed? That's not unique to computer-assisted proofs, but they have to be tackled by different means – to certify a proof as correct, people really have to understand it.

COLELLA: I would like to think (and maybe you can comment on this) that one of the unspoken riffs – or perhaps not always unspoken – between computational mathematicians or numerical mathematicians and the more traditional pure mathematicians, is that in computing there are, if not no proofs, precious few. And that makes us a breed apart, from the point of view of traditional – or what is viewed as traditional – mathematics. Is there is some hope that maybe that rift will be closed as pure mathematicians use computers to manage their complexity as well?

LAX: Well, at one point there is really a different point of view. If you take, say, three-dimensional or even two-dimension compressible-flow calculations in air, or even a very simple problem (it could be the Riemann problem, nothing could be simpler), but it produces complicated patterns like that, or diffraction by a wedge, which produces a double Mach refraction. I don't think we will ever be able to prove that this is within a described epsilon of the true flow. That's hopeless, and if there were such a proof it would be unreadable. It's far more convincing that, say, in calculation carried out by an entirely different numerical method may give very similar results. And in that respect the computational mathematician does indeed from a theoretical mathematician. It's a different style. I don't think it needs to be reconciled. Von Neumann, who was certainly a marvelous theoretical mathematician, also enjoyed doing calculations. It never entered his head to prove rigorously that they were within epsilon [of the solution]. He made some calculations for the design of the implosion employed in the plutonium bomb. They were not used by the physicists because they were so novel, they were a little suspicious.

COLELLA: Was it ever verified to what extent they were correct?

LAX: Well, yes. It was the place where shock capturing was first introduced. But von Neumann made the mistake of using a discursive method, so the solution that he computed was oscillatory. This did not bother him because the oscillation was in the velocity field and he said a shock is an irreversible conversion of mechanical energy into internal energy, and internal energy is represented by the oscillation of particle velocities around the mean, so that's what he had produced. This argument is not quite correct. Later on the oscillations were suppressed by artificial viscosity. So his original calculations were not quite correct.

COLELLA: I see. Anything else you'd like to add to this?

LAX: Well, computational science, computational mathematics, has a very bright future, and I warmly recommend them to young people to go into that field.

COLELLA: Well, thank you. As someone who took that recommendation thirty years ago, I'm happy to endorse it myself. Thank you, Peter.