

Interview with Louis Nirenberg

Louis Nirenberg is one of the outstanding analysts of the twentieth century. He has made fundamental contributions to the understanding of linear and nonlinear partial differential equations and their application to complex analysis and geometry.

He was born on February 28, 1925, in Hamilton, Ontario, Canada. After receiving his bachelor's degree from McGill University in 1945, he went to New York University as a graduate student, obtaining his M.S. in 1947 and his Ph.D. in 1949, under the direction of James Stoker. Nirenberg then joined the faculty of NYU and was an original member of the Courant Institute of Mathematical Sciences. After spending his entire academic career at Courant, he retired in 1999.

Nirenberg received the AMS Bôcher Prize in 1959 for his work on partial differential equations. In 1982 he was the first recipient in mathematics of the Crafoord Prize, established by the Royal Swedish Academy of Sciences in areas not covered by the Nobel Prizes. In 1995 he received the National Medal of Science, the United States' highest honor for contributions to science.

The following is the edited text of an interview with Nirenberg, conducted on December 8, 2001, by *Notices* senior writer and deputy editor Allyn Jackson. The assistance of Dieter Kotschick, Ludwig-Maximilians-Universität München, is gratefully acknowledged.

—A. J.

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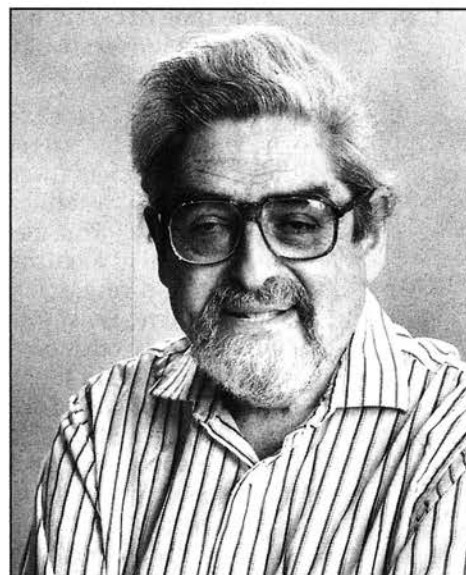
Early Experiences

Notices: What were your early experiences with mathematics?

Nirenberg: I always liked mathematics in school. My father was a Hebrew teacher, and he wanted me to learn Hebrew. But I foolishly resisted. I went to Hebrew school for a while, and that didn't take, and he tried to give me lessons, but that didn't take either. Then a friend of his gave me private lessons, and that man liked mathematical puzzles. So half of the so-called Hebrew lessons were spent on mathematical puzzles.

I went to a very good high school in Montreal called Baron Byng High School. It was full of bright students. It was during the Depression, and to be a high school teacher was considered a very good job, so there were good teachers who were very devoted. I especially liked the physics teacher, who actually had a Ph.D. in physics. His courses made me think that I might want to be a physicist. I didn't even know that there was such a career as "mathematician". I knew you could be a math teacher, but I didn't know you could be a mathematician.

When I finished high school, I decided I would do mathematics and physics. At that time one could do a major in both, which I did at McGill



Louis Nirenberg



L. Nirenberg at the time of receiving his B.Sc. from McGill University, 1945.

University. I graduated in 1945, just when the war ended.

Notices: *How was the mathematics at McGill at that time?*

Nirenberg: The training was pretty good. I guess the most prominent mathematician there at the time was Gordon Pall, who was in number theory. He was really an inspiration to the students around him. But I had planned to do theoretical physics.

I'll tell you the story of how I went into

mathematics. When I graduated, the war had finished in Europe but was still on in Japan. In Canada, the science students were not drafted, and that's why I wasn't in the armed services. In the summer of 1945, I got a job at the National Research Council of Canada in Montreal. They were working on atomic bomb research. Richard Courant's older son was there, Ernst Courant. Ernst had recently married a girl from Montreal whom I knew, and she was working there too. One day she said, "We're going down to New York for the weekend, to see Ernst's father." I had read part of Courant-Hilbert, so I knew about Courant. I said, "Could you ask him to recommend a place where I might study theoretical physics?" I knew nothing about where to apply or what to do. She came back and said she'd talked to Courant about me, and he had suggested I get a master's in mathematics at New York University, where he was, and then maybe go on to do physics. So I went down for an interview, and I met him and Friedrichs. They were very kind and offered me an assistantship. Then I stayed in mathematics.

Notices: *Was it the Courant Institute at that time?*

Nirenberg: No, it was just the Graduate Mathematics Department of New York University. The department was tiny, but there were several very good fellow students. Some of us who got Ph.D.'s there stayed, like Harold Grad, Joe Keller, Peter Lax, and Cathleen Morawetz. Peter Lax's wife, Anneli, was a student there when I arrived. She was the first person I met when I came there as a student. It was a remarkable group of people.

Richard Courant, A Complicated Man

Notices: *What are your impressions of Courant?*

Nirenberg: I remember him very well. He was a complicated man; he isn't easy to summarize. He was enormously intelligent and terrific with young people. He loved to be with young people and was very encouraging. As a teacher he was good when he prepared, which was seldom, but I enjoyed his classes. Very often on the weekend he would invite some graduate students to his home, which was in New Rochelle. I discovered that one of the reasons was to weed his garden.

Courant was a great lover of music, as was his whole family. They often played chamber music at home, and sometimes I attended concerts. The story went that when Courant hired somebody, he would ask if the person played an instrument. If so, the person had a better chance of being hired. But if the instrument was piano—no, because Courant played the piano. Probably the story isn't true, but that was the story at the time.

Some of the other students were closer to Courant than I was. Kurt Friedrichs was a big influence—I would say the major influence—on me in mathematics. His view of mathematics very much formed my view. I started with Friedrichs as an adviser, and he gave me a problem in operator theory. I thought about it for a while, but I didn't get anywhere. Some months later Jim Stoker suggested a problem in geometry. Stoker was my official adviser, and he was a very kind man. But I actually talked more with Friedrichs than I did with Stoker during the time I was working on the thesis. So I was really closer to Friedrichs.

Notices: *What was Friedrichs's view of mathematics that influenced you?*

Nirenberg: I'd have trouble saying. When you were trying to resolve something, it didn't matter so much whether you would prove it was true or false. The thing was to understand the problem. Also, Friedrichs was a great lover of inequalities, and that affected me very much. The point of view was that the inequalities are more interesting than the equalities, the identities. I also liked the things he did in partial differential equations, which I followed very closely. But he did other things, in quantum theory, operator theory, shock wave theory. When I was a graduate student, I felt that what Friedrichs was doing was where the action was. So I went to him to do a thesis, but in the end I didn't do the thesis with him.

Notices: *What was the problem you worked on in your thesis?*

Nirenberg: It was a problem that Hermann Weyl had worked on, a problem in geometry. Weyl had solved it partly, and what I did was complete the proof. Hans Lewy solved it in the analytic case. You're given a Riemannian metric on the 2-sphere, having positive Gauss curvature, and the question is, can you embed this 2-sphere isometrically into 3-space as a convex surface? Weyl worked on it,

around 1916 I think, and had made some crucial estimates. One needed some more estimates before one could finish the problem. What I did was to get the additional estimates, essentially using ideas of C. B. Morrey. Morrey's work was a very big influence on me, and later I got to know him. He was a very nice man. He didn't have many joint papers, but we did write one paper together. Morrey understood a lot of things, but he was hard to understand. I remember a story I heard. He ran a seminar every year at Berkeley. One year, the semester started, and the seminar met for the first time. He said, "Well, I'll use the same notation as last year."

Notices: *So if you weren't in the seminar last year, too bad?*

Nirenberg: You'd have to catch up. I once attended a conference in Pisa, and Morrey was there. A number of people gave a series of talks, and of course we spoke English; we didn't know Italian. When Morrey spoke he had a strong Ohioan accent, and the Italians found him very hard to understand. And he would use expressions like, "Well, if you try this kind of technique, you'll never get to second base." They had no idea what this referred to. They called him "The Sheriff". During the meeting the local newspaper published some photos from the lectures. There was a photo of Morrey standing at a blackboard lecturing, and the caption read, "Professor Nirenberg from New York University." Morrey saw this and said, "That's not Nirenberg! Those are my formulas!"

Notices: *In your later work did you follow up on your thesis on the embedding problem in greater generality?*

Nirenberg: No. The work on the embedding problem involved nonlinear partial differential equations. That's how I got into partial differential equations. After that I worked essentially in partial differential equations connected to other things.

There is still a local embedding problem that has been open for maybe 150 years. If you're given a Riemannian metric in a neighborhood of the origin in the plane, can you embed that isometrically as a piece of surface in R^3 ? The general case is still open. If the metric is analytic, the answer is yes; you use the Cauchy-Kowalevsky theorem. If the curvature of the metric is strictly positive, the equations you get are elliptic, and again the answer is yes. If the curvature is strictly negative, the equations are hyperbolic; again the answer is yes. But if the curvature can change sign or might have a zero, then the problem is more difficult. Years later I gave a case of the problem to a graduate student, who did a beautiful thesis. He solved the problem in case the curvature vanishes at some point, but where its gradient is not zero. He solved that case in a beautiful paper. His name is Chang-

Shou Lin. He also worked on the case where the curvature is non-negative but might have a zero. Other people have worked on this too.

Notices: *Going back to Morrey, what did he work on?*

Nirenberg: One of his famous papers, which he did around 1932 or 1933, was to solve one of Hilbert's problems, which Hilbert had formulated in two dimensions. Morrey proved the analyticity of the solution of the nonlinear variational problem. The n -dimensional problem was done in 1957 independently by De Giorgi and Nash. De Giorgi did it first.

Notices: *You knew [John] Nash. There was one year when he hung around the Courant Institute.*

Nirenberg: He was officially visiting Princeton, but his girlfriend—I think they were not yet married at the time, but I don't quite remember—lived in New York. So he spent a lot of time in New York, and he hung around the Courant Institute a lot. I knew him pretty well that year. That's the year when he did the paper connected with the De Giorgi paper.

Notices: *Were you the one who suggested that problem to Nash?*

Nirenberg: I think Sylvia Nasar writes that in her biography of Nash, but I don't remember. I would say it's likely, because it was a problem that I was



New York City, ca. 1953. Left to right: K. O. Friedrichs, J. Stoker, M. Nagumo, F. John, L. Nirenberg.



Left to right: L. Nirenberg, P. Lax, O. Oleinik, F. Trèves.



Left to right: L. Nirenberg, C. Morawetz, L. Bers, M. Protter, 1960s.



Nirenberg with wife Susan and children Lisa and Marc, Spain, 1962.



With Joe Kohn around 1965.

interested in and had tried to solve. I knew lots of people were interested in this problem, so I might have suggested it to him, but I'm not absolutely sure.

Notices: What did you make of him at the time?

Nirenberg: About twenty years ago somebody asked me, "Were there any mathematicians you would consider as geniuses?" I said, "I can think of one, and that's John Nash." I first heard of him when he did his paper on the isometric embedding problem, and I studied that paper. I found that to be a remarkable paper. I met him after he had done it, and I heard him speak on it at a meeting in Seattle. When he was hanging around Courant and working on the other problem, he would come around and ask questions like, "Do you think such-and-such inequality might be true?" Sometimes the inequalities weren't true. I wasn't sure he was getting anywhere. But then in the end, he did it. He had a remarkable mind. He thought about things differently from other people.

Pure versus Applied?

Notices: How do you see the relationship between so-called "pure" and "applied" mathematics?

Nirenberg: That was one of the nice things about the Courant Institute—and very much due to Courant and Friedrichs—that there was hardly any difference between pure and applied. There was just mathematics, and people were interested in both pure problems and applied problems and didn't distinguish so much. There was a period when I was a graduate student when a number of people—Friedrichs, Stoker, Hans Lewy, Fritz John—worked on the theory of water waves. But the work was analysis, meaning partial differential equations or complex analysis.

Courant also was a great believer that you must not only do research, you must also teach. That's very different from, say, the Russian or Soviet system, where there were many institutes where people just did research and didn't teach. Courant always thought that was very bad. In fact, the young people at Courant taught less than the older people. In general the atmosphere is terrific at Courant. The graduate students are very fond of the place.

There's a very warm relationship between the faculty and the students.

Notices: You spent 1951–52 in Europe. Where did you go?

Nirenberg: I went to Zurich, to see Heinz Hopf, and I also went to Göttingen. This was arranged by Courant. I went first to Zurich, and then my wife and I went to Göttingen. She was

very unhappy to be there, and we stayed only a month in Göttingen and then went back to Zurich.

Notices: Why was she so unhappy?

Nirenberg: Just the idea of being in Germany. Both of us were not so happy with the idea, but Courant had arranged it, and we thought we should do it. In fact, she went back to Zurich a little before I did.

When I was in Göttingen, Carl Ludwig Siegel invited me to dinner, and he served white asparagus. I had never had white asparagus before, which I found delicious. I was told the next day he complained, "Nirenberg ate all the asparagus!" But I didn't have much contact with Siegel. Jürgen Moser was a student at the time I was there. He was a student of Franz Rellich, and, in fact, when I was there I spoke more with Rellich than with other people.

In Zürich I attended Hopf's lectures. He was my favorite lecturer for many years. He spoke absolutely gorgeous, musical German. He gave a wonderful course in geometry, and I later attended a course he gave at Courant. I kept up my interest in geometry, although I didn't work so much in geometry. In Zurich I also attended lectures by Nevanlinna and van der Waerden, who were at the University of Zurich. van der Waerden was giving a course in Riemann surface theory, which he then made into a book. At that time I could speak a little German, but since then I haven't spoken German, so I've forgotten. Yiddish was my first language, so it wasn't hard to pick up some German, and I still speak Yiddish a little and can understand it almost perfectly. I also met Hermann Weyl, but he wasn't teaching, he was just living in Zurich. I attended some lectures of Pauli in relativity theory. I found him hard to understand.

Newlander-Nirenberg Theorem

Notices: Around 1957 you worked on the integrability problem with your student Newlander. How did that paper come about?

Nirenberg: That happened in an interesting way. I heard of the problem from two people, first from André Weil. He said, "Ah, you people in partial differential equations! You're not working on the

important problems! Here is an important problem that we need in complex analysis. Why aren't you working on that?" And later Chern brought the problem to my attention. So I thought, okay, let's have a stab at that. So I suggested to Newlander that we work on it.

The problem, a local one, is this: In R^{2n} , can one recognize the Cauchy-Riemann operators if they are given in some arbitrary coordinate system? For $n = 1$, the long-known answer is yes. For $n > 1$, there are necessary integrability conditions, and these turn out to be sufficient. In the problem you have, sort of, Cauchy-Riemann equations with some extra terms. The idea was to get rid of those terms one at a time. In the simplest case we first looked at, there was just one extra term, and Newlander had the idea of how to get rid of that term. Then we worked together on the more general case, first in two complex dimensions. I was sure that everything would work in higher dimensions, but then when it came to writing it down, we discovered that the idea didn't work in higher dimensions. We then came up with a different proof, which got rid of everything at the same time, but by making a fully nonlinear transformation of the whole problem. But I was drawn to the problem because of André Weil and Chern.

Notices: *They were right, because this turned out to be an important result.*

Nirenberg: Yes, it's a very natural question, and the result has been used. I find it interesting that in recent years Gromov and others have done fantastic things with nonintegrable structures.

Notices: *Where is Newlander now?*

Nirenberg: He got a job in Seattle, but he had some psychological problems and he couldn't teach. After a few months he gave up his job and gave up mathematics. He moved to his hometown, Denver, Colorado, and we were in touch every New Year's for a number of years, but now I've lost touch with him. I don't know where he is. He stopped mathematics shortly after the Ph.D. thesis. That was the only paper he ever wrote. He was a very bright guy, but he had problems.

Notices: *Was he your first student?*

Nirenberg: No, my first student was Walter Littman. He's at the University of Minnesota and works in partial differential equations. I've had quite a number of students, about forty-five. I had a colleague at Courant, Wilhelm Magnus, and he was marvelous with students. Once he said to me, "You know, I don't mind writing a student's thesis. I object when they come to check on my rate of progress."

Notices: *You had a couple of especially influential papers with Agmon and Douglis. Can you say a little bit about these papers and why they have been influential?*



International Congress in Stockholm, 1962. Front row: M. Stone, J. Moore, N. Wiener. Second row: Nirenberg center (with glasses). To his (physical) left, H. Hopf.

Nirenberg: In the theory of second-order elliptic equations, there was famous work done by Schauder. Douglis and I decided we would like to extend the Schauder theory to higher-order equations. So we did that first for so-called interior estimates, meaning away from the boundary. Then we started working on the case on boundaries, and we discovered that Agmon was also working on that. So we thought the three of us could collaborate. The collaboration with Agmon was mainly by mail, because he wasn't at Courant then. This was shortly after the work of Calderón-Zygmund on L^p theory, and we knew that the Calderón-Zygmund theory could extend easily to interior estimates in L^p , so we thought we should do that up to the boundary also. And that was a lot of work. We decided to do it in general because we figured these estimates would be useful to people working in partial differential equations. Indeed, that's been the case; they have been useful. Douglis was also a student at Courant, but he left shortly after he got his Ph.D. We were good friends for many years.

Mathematical Taste

Notices: *When you were at Courant, people would come and talk to you about the problems they were working on.*

Nirenberg: One reason was that I was very good at catching mistakes. I'm no longer good at that. I don't catch my own mistakes anymore! I have to be very careful and check everything I do. But I was very good at catching mistakes, so people came for that reason. They would show me a proof to have it checked because I had a good nose for mistakes.

Notices: *Then out of these conversations also came collaborations.*

Nirenberg: Sometimes, yes.



Joint Soviet-American Conference in Partial Differential Equations in Novosibirsk, USSR, August 1963. L. Nirenberg provided partial identification of those pictured in photo above. Front row (left to right): J. Moser, A. Zygmund, L. Ahlfors, N. Brunswick, S. L. Sobolev, C. B. Morrey, C. Loewner, R. Courant, M. M. Lavrent'ev, N. Vekua, S. Bergman, A. N. Tikonov, G. I. Marchuk, D. Spencer, A. Dynin. Second row: 2) M. Krein, 3) O. A. Oleinik, 4) H. Weinberger, 5) H. Grad, 6) M. Schechter, 7) J. Douglas, 8) F. Browder, 9) M. H. Protter, 11) Ju. M. Berezanskii, 13) A. P. Calderón, 14) P. D. Lax, 15) E. B. Dynkin. Third row: 1) A. Ja. Povzner, 3) P. E. Sobolevskii, 6) B. V. Shabat, 11) S. Krein, 13) R. Richtmeyer. Fourth row: 3) R. Finn, 6) L. Nirenberg, 13) M. Vishik, 16) Yu. V. Egorov. Fifth row: 1) M. Agranovich, 3) N. Vvedenskaya, 4) L. Volevich, 5) T. Venttsel. Sixth row: 1) V. A. Solonnikov, 2) M. A. Lavrent'ev, 14) L. Faddeev, 16) M. S. Birman.

Notices: What would guide you in choosing what to work on? What kinds of things would interest you about problems you would hear about?

Nirenberg: I myself don't understand so very well. I remember meeting a young Frenchman years ago, and he had been trying to do research for several years. He asked me, "How do you do research? How do you start on a problem?" I said, "Well, sometimes it happened to me that I read a paper and I didn't like the proof. So I started to think about something that might be more natural, and very often this led to some new work." Then I asked him, "What about your case?" He said, "I never found a proof I didn't like." I thought, "This is hopeless!"

Notices: Did you then give him a few proofs you thought were especially bad?

Nirenberg: No.

Notices: There is a question of taste there.

Nirenberg: Yes, taste plays a very important role in mathematics. Some mathematicians I think have very good taste, others I am not so drawn to the kind of problems they work on. Taste is very

important, and it's very hard to define or even to describe.

Notices: But what usually appeals to you? A general theoretical question? Or a specific problem?

Nirenberg: I greatly admire people who develop theories in mathematics, but I am not one of those. I am more of a problem solver. I hear a problem, and if it appeals to me, I work on it.

I remember the work I did with Joe Kohn on pseudo-differential operators. We were trying to extend his work on regularity of the so-called $\bar{\partial}$ -Neumann problem to other degenerate problems. We were trying to work with singular integral operators, and we seemed to need facts about products and commutators of singular integral operators, which were then not in the literature. We said, "Well, we'll try and develop what we need." That's how we did the work that we then called pseudo-differential operators—by the way, the name "pseudo-differential operators" is due to Friedrichs. We needed this for a specific problem. But in the case of the work with Agmon and Douglis—there we felt we should develop the

general estimates for the general systems under general boundary conditions because we thought those estimates would be useful.

Notices: *You said you are problem oriented and you choose problems that appeal to you. Can you say what the appeal is, or which type of problem appeals to you?*

Nirenberg: That's hard. Inequalities, certainly. I love inequalities. So if somebody shows me a new inequality, I say, "Oh, that's beautiful, let me think about it," and I may have some ideas connected with it.

Let me say a word about the paper with Luis Caffarelli and Bob Kohn on Navier-Stokes equations. There was a paper of I. M. Sheffer, a mathematician at Rutgers who had very interesting results on the dimension of possible singularities. One day I was walking through Chinatown with Caffarelli and Bob Kohn, and I said, "You know, we should study that paper. Why don't we study it together?" So that's how that came about—we decided to study Sheffer's paper.

Notices: *The Millennium Prize Problem about the Navier-Stokes equations asks whether or not the solutions have singularities. How do you see this problem?*

Nirenberg: It's a great problem. I think it will be settled in the not-too-distant future, one way or another. I won't bet which way it will go. Maybe twenty years ago—before I had done the work on the Navier-Stokes problem—I asked Jean Leray which way he thought it would go. He didn't predict. He was a great mathematician and greatly influenced me. I first met him at the International Congress at Harvard in 1950. He had also worked on the isometric embedding problem, and I couldn't understand his paper, so I made an appointment to talk about it with him. We met in his office at Harvard for one or two hours. He was extremely kind, but I never understood that paper.



Moscow, 1988, at a conference organized by "refuseniks".



Receiving the Crafoord Prize, Stockholm 1962, with Mrs. Crafoord and the King of Sweden.

Notices: *With the Navier-Stokes problem, do you think that the existing methods in PDE are enough to eventually crack it, or is some new idea needed?*

Nirenberg: My feeling is one needs more harmonic analysis. But I haven't worked on it since the work we did in our paper.

Notices: *But your result is about the best that's been done.*

Nirenberg: About the nature of the singularities, yes.

Notices: *It's strange that the problem is so open, that one isn't leaning one way or the other.*

Nirenberg: When I was working on it, I sometimes felt one way, sometimes I felt the other way. At the moment I don't have any particular feeling about which way it should go.

Mathematical Vision

Nirenberg: In 1963 there was a joint Soviet-American meeting in Novosibirsk on partial differential equations. That was one of the best meetings I ever went to. I met many Soviet colleagues and made friends, and we've remained friends to this day. Afterwards several of us went to Moscow for a few days and attended Gelfand's famous seminar. It goes on for hours, with different speakers, and Gelfand interrupts the speakers to make comments and ask questions. When we returned to New York, Friedrichs said, "You know, we should run a seminar like that, and we could take turns playing Gelfand."

Gelfand is still active, doing research, running a seminar, and working with different people. It's just incredible—he is now 88 years old. I saw him just a few weeks ago. He's always been the sort of person who sparks ideas, and then other people carry them out. Whenever I saw him in Moscow, he would ask me, "What do you consider important now in mathematics? What are the future directions?" These



September 11, 2000, the International Conference on Partial Differential Equations in Celebration of the Seventy-fifth Birthday of Louis Nirenberg (Nirenberg in center, front row).

were questions I could never answer. It was always embarrassing to me, because I never think in those terms. But he does think in those terms.

Notices: *You would have things you were working on then. But you didn't necessarily think that that would be the future direction?*

Nirenberg: No, one doesn't know. And I don't have such a vision of mathematics as he does.

Notices: *But do you think his vision has been good and accurate?*

Nirenberg: Oh yes, I think he has remarkable vision. He's worked on so many different things, and he helped develop so many different fields.

Notices: *Do you know of other mathematicians who have that kind of vision?*

Nirenberg: Maybe André Weil had that kind of vision. Perhaps Hirzebruch, Atiyah, Milnor, Smale.



Nirenberg with colleague Peter Lax, 1999.

This reminds me of a story I heard about von Neumann. Somebody once asked him, "Today, how much of all of mathematics can a mathematician know?" He replied, "Uh—two-thirds."

The Joy of Collaboration

Notices: *You wrote one paper with Fritz John.*

Nirenberg: I think I was the first person he wrote a paper with. He then wrote several with Klainerman, towards the end of his life, but mostly he worked by himself. He never followed any fashion. In fact, he always would apologize, "Oh, I haven't read this, I haven't read that." But he created fashions, because he had such wonderful ideas, and people then followed his ideas and developed what he did. He was very independent, at the same time very modest.

The paper I wrote with Fritz John started when he came to me and said, "I believe such-and-such inequality should be true" and that something should be in L^p . I was able to prove that, and then he improved what I had done. So we wrote a joint paper.

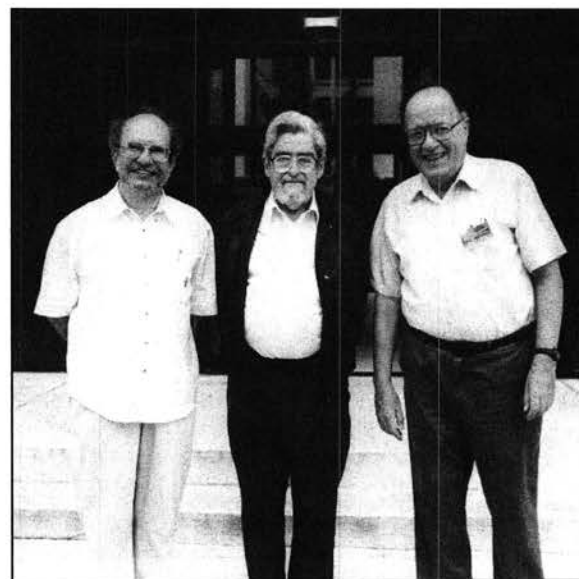
Notices: *This was the paper in which you defined BMO [bounded mean oscillation].*

Nirenberg: Yes. That was the only paper I had with him. He was a wonderful mathematician—extremely deep and original.

Notices: *How did he come upon the problem you worked on together?*

Nirenberg: He did several papers in elasticity theory, and it was one problem in particular that came up.

Notices: *How have the BMO spaces been used subsequently?*



With Haim Brezis and Joe Kohn, Taiwan, September 2000.



Nirenberg: They have been used in harmonic analysis and in martingale theory. More and more in analysis people are working on something called VMO—which I call the son or daughter of BMO—vanishing mean oscillation. That's due to Donald Sarason at Berkeley, and it's turned out to be an extremely useful tool. A few years ago I did a paper with Haim Brezis in which we extended degree theory to mappings belonging to VMO.

I've worked with several French mathematicians and written lots of papers with Brezis and Henri Berestycki. And a lot of papers with Luis Caffarelli.

Notices: *What is Caffarelli like as a mathematician?*

Nirenberg: Fantastic intuition, just remarkable. We haven't worked together for several years now, but when we worked together, I had a hard time keeping up with him. He somehow immediately sees things that other people don't see, but he has trouble explaining them. He says things and writes very little, so when we were working at the board, I would always say, "Luis, please write more, write down more." Once I said to him, "Luis, to use a Biblical expression, 'Where is it written?'" Somebody said he once heard a talk in which Luis proved something in partial differential equations—using nothing! Just somehow out of thin air, he can come up with ideas. He's really fantastic—and a very nice person.

I must say all the people I've worked with have been extremely nice. It's one of the joys of working with colleagues. For instance, Peter Lax—although we wrote only one paper together, he seems like a brother to me. He was a big influence on me. He always knew more mathematics than I did. I learned a lot from him.

Speaking of collaborating, let me just mention a few other people I enjoyed working with very much. I wrote several papers with François Trèves, and they were a great pleasure. We worked on certain classes of equations that came out of work of Hans Lewy—equations that have no solutions at all, even locally. We wrote several papers on this problem. That was fun. I would like to mention also David Kinderlehrer, Joel Spruck, and Yan Yan Li. I wrote one paper with Philip Hartman that was

elementary but enormous fun to do. That's the thing I try to get across to people who don't know anything about mathematics, what fun it is! One of the wonders of mathematics is you go somewhere in the world and you meet other mathematicians, and it's like one big family. This large family is a wonderful joy.



Photograph courtesy of Yan Yan Li.

Above, photos left to right: Nirenberg with Jürgen Moser; with Yan Yan Li and Chang-Shou Lin in Taiwan, Sept. 2000; with Irene and Luis Caffarelli, 2000; with Yan Yan Li, October 2001.

Except where otherwise noted, all photographs in this article are courtesy of Louis Nirenberg.



Photograph by Rachel S. Brezis.

A mathematical discussion—Haim Brezis on left, Louis Nirenberg on right, Haifa, January 2002.