Interview with Joseph Keller

Joe Keller at home in Stanford, CA.

Joseph B. Keller is one of the premier applied mathematicians of recent times. The problems he has worked on span a wide range of areas, including wave propagation, semiclassical mechanics, geophysical fluid dynamics, epidemiology, biomechanics, operations research, finance, and the mathematics of sports. His best-known work includes the Geometrical Theory of Diffraction, which is an extension of the classical theory of optics and inspired the introduction of geometric methods in the study of partial differential equations, and the Einstein-Brillouin-Keller Method, which provided new ways to compute eigenvalues in quantum mechanics. Keller has had about fifty doctoral students and many collaborators. With a publication list of over four hundred articles, he remains active in research; his most recent paper appeared this year in the *Proceedings of the National Academy of Sciences.*

Keller was born in Paterson, New Jersey, in 1923. He received his bachelor's degree from New York University in 1943. He was an instructor in physics at Princeton during 1943–44 and then became a research assistant in the Columbia University Division of War Research during 1944–45. After receiving his Ph.D. from NYU in 1948, he joined the faculty there and participated in the building of the Courant Institute of Mathematical Sciences. In 1979 he moved to Stanford University, where he is now an emeritus professor.

Keller has received many awards and honors throughout his career, including the von Karman Prize of the Society for Industrial and Applied Mathematics (1979), the Timoshenko Medal of the American Society of Mechanical Engineers (1984), the National Medal of Science (1988), the National Academy of Sciences Award in Applied Mathematics and Numerical Analysis (1995), the Nemmers Prize from Northwestern University (1996), and the Wolf Prize (1997).

What follows is the edited text of an interview with Keller conducted in March 2004 by *Notices* senior writer and deputy editor Allyn Jackson.

Early Years

Notices: How did you get interested in mathematics?

Keller: I was always good at mathematics as a child. My father, who was not educated but was a smart guy, used to give my brother and me mathematical puzzles when we were kids. Here's an example. A goose met a flock of geese, and the goose said, "Hello, 100 geese." The leader of the flock said, "We are not 100 geese. But if we were twice as many as we are, plus half of that, and you, then we would be 100. How many were there?" That was a typical example, which I happen to remember.

In my high school, which was East Side High School in Paterson, New Jersey, I had several good mathematics teachers. One of

Keller at age 4.

them was Mr. Dougherty. After I left high school, I didn't hear from him until maybe twenty years later. I was a faculty member at the Courant Institute. I got a letter from him saying that he had just purchased a copy of one of Morris Kline's books. On the dust jacket there was a curve, and he had tried to figure out the equation of that curve, and he didn't succeed. He wrote that he knows that Kline is a very busy man, but perhaps I could ask him about the equation of that curve. So I took the letter to Kline, and he remembered Dougherty because Doughtery used to bring a team from our high school for the Pi Mu Epsilon examination, which was held at NYU every year. I had been a member of that team. When Kline read Dougherty's letter, he said, "Joe, there is something funny about that figure. I had given the draftsman a portion of it in the first quadrant, and he was supposed to flip it over to fill out the whole picture. But he did it incorrectly, and consequently there is no simple equation that would give that figure." Kline was amazed that Dougherty had found this, and he sent him a copy of the book as a reward. Dougherty was then teaching at a state college in Pennsylvania. He was no longer at the high school—he had "graduated".

![Joe Keller, top right, with his brother Herbert B. Keller and their parents, early 1950s.](image)

Joe Keller, top right, with his brother Herbert B. Keller and their parents, early 1950s.

I was always interested in mathematics. When I went to college, I thought I would major in mathematics, but the first year I took a course in physics. I found that so attractive that I switched my major to physics. By the end of my stay I had majored in both mathematics and physics. Then I got an instructorship in physics at Princeton—that was during World War II.

Notices: Somebody told me that you were involved in a course that Einstein was teaching. Is that true?

Keller: No. When I was at Princeton, I had two contacts with Einstein. Once I walked past him on the street. Another time I went to a lecture by Bertrand Russell, and Einstein was in the audience. I fell asleep, but Einstein didn't. So I figured it was easy for me, and hard for Einstein.

Notices: Did you hang around much with the mathematicians in Princeton?

Keller: Oh sure, because during the war there were very few graduate students, so I hung around with all of them. I was taking half mathematics and half physics courses. I had courses from Lefschetz and Tucker and Church and Bohnenblust and Chevalley. One course I had was with Pauli. Of course many of the people were away during the war, some at Los Alamos.

I was able to continue studying at Princeton by teaching in programs in which the armed services put people to be trained as engineers before the end of the war. After I was at Princeton for one year, 1943–44, it was recognized that the war would end before the students ever came out as engineers. The program was terminated, so I had to find a new position. I went to the American Institute of Physics, which sent me to the Columbia University Division of War Research. The position was working for the Office of Scientific Research and Development, in offices on the fiftieth floor of the Empire State Building. The work was to analyze the use of sonar in submarine detection. That's what I worked on during the rest of the war.

I had very good colleagues there, primarily physicists. My boss was a physicist named Henry Primakoff, and I shared an office with another graduate student, Martin Klein, who is now a professor in the history of science at Yale. I got to meet a lot of people there who were working in various aspects of submarine detection. For example, I met Conyers Herring, now an emeritus professor of physics here at Stanford.

Incidentally, during the time I was working there, an airplane hit the Empire State Building. It happened on a Saturday, and I was late to work. By the time I arrived, the building was cordoned off and I couldn't get in. That was a precursor of the World Trade Center disaster, although this one was an accident. It was due to the fact that the cloud level was very low and the pilot couldn't see.

We worked there on the following kind of question: How much sound is reflected back from a submarine? We had devices, called projectors, which are like underwater loudspeakers and which sent out sound waves. The waves would hit whatever was out there, and some of the waves would be reflected back and picked up by a device called a receiver. The sonar officer would listen, oftentimes with earphones, to the reflected sound, if there was any. When he heard something, then he concluded there was an object out there, and he would try to locate it precisely. The problem was to calculate how the waves traveled through the water, spread out, became weaker, hit the object, and then how much was scattered back, and what the strength of the signal would be when it got back to the receiving sonar. We wanted to know, for example, at what range would it be possible to detect a submarine, and what was the best frequency to use? The projector sent out a beam, and the beam spread out, and if you didn't detect anything, then you would change direction and do it again. How much should you move each time, through what angle? How long should you wait for the signal to

come back? How deep is the submarine, and should you look down and up? There were all kinds of questions like that.

We had a laboratory in Mountain Lakes, New Jersey. Occasionally I was sent out there to test the projectors and receivers. One of the nicest jobs I had was testing an "underwater flashlight". It was an underwater, handheld sonar device the size of a large flashlight, and it was to be used by troops who were coming up on a beach to locate land mines and other obstructions underwater. I would have to go out swimming in the lake, which was great in nice weather, to test this device. It sent out a signal, and when the signal bounced back, I would hear it on earphones. With practice I could tell how far away the object was, and it worked with objects up to about 50 feet away.

That was the fun part of the laboratory work. The theoretical work that I did in the Empire State Building was on the analysis of sonar, scattering from objects, and things like that, which is what I did for many years during the rest of my career. I wrote my Ph.D. thesis with Primakoff, who moved to NYU, although he left NYU before I finished, so Courant ended up being my nominal advisor. What I did in my thesis was to use some of the methods we had worked on during the war in connection with sound wave propagation. I did the analogous things with electromagnetic wave propagation.

Explosions and Shock Waves

Notices: Did you also work on underwater explosions?

Keller: That was later, when I came to NYU. There had been a lot of work done there on explosions. *Supersonic Flow and Shock Waves*, by Courant and Friedrichs, was the book that resulted from their study of all kinds of explosions. In a cartoon that appeared in the book [see upper right], it's possible to recognize Courant and Friedrichs. The cartoon was drawn by Gabi Wasow, the wife of Wolfgang Wasow. He was a mathematician, a student of Friedrichs, who later was a professor at Wisconsin. You'll notice that the cartoon is not bound in the book. Probably someone thought it was too frivolous.

When I got to NYU, I worked on underwater explosions with Bernard Friedman and Max Shiffman. Shiffman and Donald Spencer had written a paper on water entry by aerial torpedoes. The question is, What are the forces exerted by the water on the torpedo? In order to analyze that problem, they studied a related problem. When part of a sphere is in the water, they reflected that part across the surface of the water. The reflection, together with the part under water, they called a lens. They studied the flow of water around a lens.

When I got to NYU, Shiffman and Friedman gave me that paper to read. I found I was able to obtain

Cartoon illustrating a shock wave, from *Supersonic Flow and Shock Waves* by Richard Courant and K. O. Friedrichs, Interscience Publishers, 1948.

some results in that paper by an elementary method, which surprised them, and that cemented my position. About fifteen years ago Spencer received the National Medal of Science, so I called him to congratulate him. I mentioned to him that the first problem I was given to work on when I went to NYU was that paper of his and Shiffman's. He said, "You know, Joe, you're the first person who ever mentioned that paper to me!"

When an explosion occurs under water, the explosive gets converted into a gas. The gas is under very high pressure, so it expands and produces a bubble, which gets bigger and bigger, until finally the pressure in the bubble has gone down so low that it stops expanding. Then the water pressure outside forces it back in. So the bubble oscillates. I worked on that problem with a colleague, Ignace Kolodner. A theory of that oscillation had already been developed by Rayleigh during the First World War. What we did was to modify the theory to include the loss of energy by the shock waves that are sent out from the bubble, which makes its oscillations decay in size. Also, when the bubble oscillates, the water motion interacts with the top and the bottom, and that causes the bubble to move. It turns out that often the first shock wave that the bubble sends out weakens the plates of the ship, and the second one breaks them. Before the second one occurs, the bubble will move. If you are
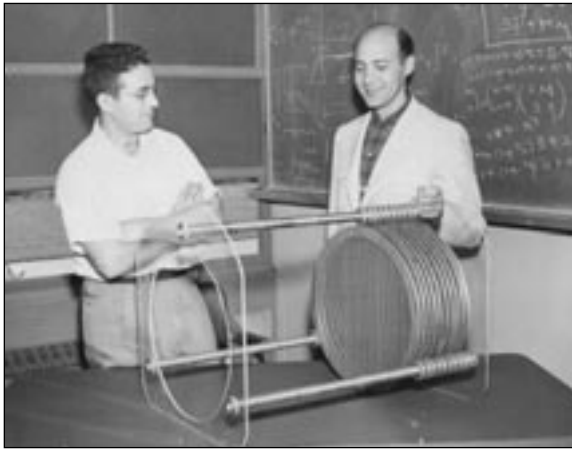
Keller, right, with Vic Twersky at the Sylvania Research Lab in Mountain View, CA, 1954.

Left to right: Hirsh Cohen, George Handelman, and Keller after lecturing at RPI, late 1950s.

trying to do the damage, you want it to be attracted to the ship and be closer when it emits its second shock. We analyzed questions like that.

My advisor, Primakoff, was working on underwater explosion of atomic bombs in connection with the proposed Bikini tests of the atomic bomb. A question we were asked, among others, was, Would that underwater explosion produce a tsunami that could cause damage all around the Pacific Ocean? We calculated that it would not cause any damage far away.

Notices: It wouldn't be strong enough?

Keller: Yes, with the size of bombs that were used then.

Notices: The atomic tests that were done at Bikini—were they underwater explosions?

Keller: No, they were done above water. But the point is, the explosion was done relatively near the water, and the blast wave pushes the water down and away. It could blow all the water out, down to the ocean floor, and that water would be moving outward.

Notices: But the force of the wave would dissipate over the distance?

Keller: That's right; it spreads out, and Bikini was not near any inhabited place. By the time the waves reached anywhere with a reasonable population, the amplitude of the waves had gone way down. Later, after the Bikini tests, in 1950 or 1951, a committee was formed to look at tests of underwater explosion of atomic bombs. Among the questions were, What damage would they do to a ship and at what distance? The committee was asked to advise whether or not such experiments should be done. von Neumann was a member of the committee. He appeared at the first meeting and at the last meeting. His point of view was interesting. He said, "The admirals want to have the tests, so we should vote yes; we should recommend it."

Notices: Is that the conclusion you came to?

Keller: Yes. We had spent a lot of time looking at questions about how strong the shock waves would be at the location of the ship, what they would do to the equipment, and other things.

During the summer of 1950 I spent one month at the Argonne National Laboratory, which was an Atomic Energy Commission Laboratory, and then another month at Los Alamos National Laboratory. When I got to Los Alamos I was asked to study explosions that were so strong that the shock waves would go far into the atmosphere, where the atmosphere becomes less dense. Obviously, the purpose was to study the effect of hydrogen bombs. They didn't tell me that, because I didn't need to know. I was given the previous work that had been done. One of the people who had worked on that before was Klaus Fuchs, who was ultimately indicted for having given information to the Russians about the atomic bomb project. So I worked on that problem. That same summer, Peter Lax, who was a student with me at the Courant Institute, was also at Los Alamos. He had been there in 1945, during the war when he was in the army. During that summer I worked with him on numerical methods for solving equations of gas dynamics, and that ultimately meant nonlinear hyperbolic partial differential equations. He continued doing that kind of work, but I didn't.

Notices: During this time when you did a lot of research related to war, did any moral questions come to your mind?

Keller: Well, yes and no. During World War II everyone who did war work felt that the atrocities committed by our enemies justified whatever we did to stop them. And later, we all believed that what we were doing was for good. We didn't think it would be used in Iraq, for example. We all believed then that any work we did would be primarily for the defense of the United States. So although people thought about it occasionally, there didn't seem to be any serious doubt about whether or not it was the right thing to do.

Notices: After the Vietnam War and even into the 1990s, there were a lot of debates among mathematicians about military funding in mathematics.

Keller: The Vietnam War was a breaking point. Up until then we all believed that the American military would only be used for what we thought of as the "right" purposes, the defense of the country. After the Vietnam War, attitudes changed completely. By that time the shock wave and explosion work that we had been doing was over. But I worked on electromagnetic wave propagation, which has many ramifications. For example, when I was working on those problems in the 1950s or early 1960s, I devised something called the Geometrical Theory of Diffraction, which was a method for solving all kinds of wave problems. At one meeting when I presented this work for the first time, Kip Siegel from the University of Michigan got up and said, "Okay, if your theory is so good, can you calculate the radar back-scattering from a cone?" He was thinking of a truncated cone of finite size, because that would be the shape of the nose cone of a missile. There had been great difficulty in calculating that. I applied my theory to it. It worked out very nicely and gave exactly the experimental results. In that paper I also looked at this question: If we were doing the shooting and didn't want the opponent's radar to see our missile, what should we do to diminish the radar signal that would be sent back? I described methods for doing that. After that work was published, it was picked up by aviation magazines. I got feedback from the Air Force contract monitor, saying that the big brass were unhappy that I had published that work, that it should have been classified. Those ideas became the basis for the so-called "stealth" technology.

Notices: Were there any other times when you received such letters from the military?

Keller: After that I was more careful! I once got a dressing-down from the contract monitor because I always wrote the footnote, "Supported by the Air Force" or whoever was supporting me. I included such a footnote on a paper on the mathematics of sports. There was a complaint, not because I was giving away secrets, but because the Air Force or Defense Department didn't want to be seen as supporting that kind of work.

Notices: It seemed too frivolous?

Keller: That's right. During the Vietnam War, when I was a faculty member at the uptown campus of New York University, I, together with other faculty members, signed an ad in favor of student demonstrations against the war. At that time I was visited by some agents; I don't know if they were FBI or Army intelligence. They questioned me, because I probably then still had clearance.

Notices: They just came and asked questions and left? That was the end of it?

Keller: They ultimately sent me a document describing our interview, and that's no doubt in my file somewhere. But it was not really a problem, because later I was a member of the JASON group, which was a group mainly of physicists advising the Defense Department.

Building of the Courant Institute

Notices: You were at NYU even before the Courant Institute started.

Keller: That's right. When I was an undergraduate student, I attended a class given by Courant. I had taken all the undergraduate mathematics courses, and I went to take a graduate course. I went with a friend of mine, Harold Lewis, later a professor of physics at Santa Barbara, who was in the same position. We attended Courant's initial lecture on Methods of Mathematical Physics. At the end of the lecture we went up and told him we were undergraduate students, and we asked, "Can we sit in on this course?" He said, "No, it's absolutely not allowed. However," he said, "if you sit in the front row there, maybe I won't see you. But just because I can't see you doesn't mean you can't ask questions." So we did attend the course, and obviously that was what he wanted, but he told us in that funny way to make evident the foolishness of the university rules.

Notices: What was your impression of Courant?

Keller: I liked him very much. He always invited us students to his house in New Rochelle to dinner—and to rake the lawn and do other chores! I took a few trips to Europe with him. In 1950 or 1951 we visited various laboratories in Europe—in France, Germany, and England—that had worked on explosion problems during the war. We went to interview people to find out what they had done,

Pitching horseshoes at an outing with students in the late 1950s.

Cross-country skiing at Lake Placid with George Morikawa, James J. Stoker, and a friend.

and we wrote a summary of our findings. That was very nice. Courant knew everyone, so when I tailed along with him, I automatically got to meet them. So, for example, we visited Göttingen, and we passed the house of Heisenberg. There was Heisenberg on the porch, and Courant spoke to him. Courant had been helpful to Heisenberg when Heisenberg was a young man. Heisenberg invited us to come to a party at his house that night, which we did.

Keller receiving an honorary degree from Northwestern University, 1988, with Bernard Mathowsky looking on.

During that trip our clearance to visit a French laboratory had not come through, so while awaiting it we went skiing in Switzerland. I didn't know how to ski, but Courant said, "Oh, there's nothing to it." I rented skis and took a lesson, and then he took me up on the mountain. I broke one pair of skis, and when I brought them back, the people apologized for having given me defective skis. Then I broke the next pair! Later we used to go skiing all the time in Lake Placid in New York. A whole group of us would go—Courant, Friedrichs, Stoker, Peter Lax, Anneli Lax, and lots of others. We did that every winter for years.

Notices: You saw the Courant Institute from its beginnings to 1979. That must have been an extraordinary thing to witness. To what do you attribute the enormous success of the Courant Institute?

Keller: Partly due to Courant's ability to recognize good people, like Louis Nirenberg, who came as a graduate student from Montreal; Harold Grad, a graduate student from Cooper Union; Peter Lax, and later Cathleen Morawetz. Courant was able to attract all these good people and to recognize them and be helpful to them so they wanted to stay. Many people from other universities complained that Courant was keeping all the applied mathematicians at NYU, which they felt deprived them of applied mathematicians. It was partly true, but on the other hand other places weren't as congenial to applied mathematics at that time. People were offered jobs. I considered going elsewhere, but the Courant Institute was so congenial, and there was a group of us that worked on related things; we could talk to one another. That made it especially attractive. Also, Courant was helpful in that he worked hard on raising the money, getting research contracts, and thus protected us to a certain extent from that chore. Of course later we had to do it ourselves, but for quite a while he and his staff did all the work. Getting contracts at that time had some significance for faculty members. Our teaching load was originally 12 hours a week. When we complained, Friedrichs said that he had done his best work when he was teaching 12 hours a week. So then we had to shut up! However, after a while the rule was that anyone who was doing research had to teach just 9 hours a week. Then, if you had research contracts that could pay for it, you could buy off 3 hours and then get down to 6 hours a week, which meant two courses. When people did get offers from other places, Courant worked hard to get them matched, so our salaries gradually moved up. It was beneficial to all of us if one got an offer and his salary was raised. Courant could not keep it inequitable for very long, so ultimately we would all get raises from one person's offer.

Notices: You had a long-running applied mathematics seminar at Courant. When did you start that?

Keller: I don't remember, but it must have been in the 1950s sometime. We always held the seminar on Friday afternoons, and then it was followed by tea, and that was very convenient, because then we could discuss the seminar topic at tea. Then it was followed by basketball.

Notices: You went out to play basketball?

Keller: We went out to play basketball. Then we went to Chinatown to make up for any weight we might have lost playing basketball. Occasionally it was listed as the "Basketball Seminar" in the weekly bulletin. In the beginning, lots of young faculty members played basketball. As years went by, most of them dropped out. I was one of the last survivors. Until I came here to Stanford, I was playing basketball at our regular weekly basketball outing.

For the seminar we would get speakers from the Courant Institute, or from anywhere around the city or the area, or people who were coming by. There were many visitors at NYU, because anyone going from anywhere in Europe to anywhere in the United States stopped in New York. We oftentimes had our choice of speakers who were coming through. The seminar was in applied mathematics, and we often had people who worked in other fields. For example, I remember we had an early talk on chaos by Bob May, who is now president of the Royal Society. He was then a faculty member at Princeton. He had done numerical computation on some simple model of population growth that exhibited chaotic behavior. We always had a big audience, because there were lots of people who were interested in applied

problems and partial differential equations, so it was very effective. When I came to Stanford I continued the seminar, but without the basketball component.

Geometrical Theory of Diffraction

Notices: The Geometrical Theory of Diffraction you mentioned is one of your major results. How did you come upon that?

Keller: In World War II I worked on sonar, scattering of waves from surfaces, like the surface of a submarine. When I came to NYU, I read the work of a colleague there, Rudolf Luneburg. Luneburg had been a student at Göttingen before World War II, and he helped Jews escape from the Nazis into Holland. Ultimately he came to the United States, and he worked for the American Optical Company in Rochester, New York, for a while. Then he came to NYU, and he wrote a set of lecture notes—he also wrote some notes at Brown University—on the electromagnetic theory of optics. I had found during my work in sonar that it was possible to describe the reflection of waves from objects by means of rays and geometrical calculations. I could figure out the strength of the signal reflected from an object just by calculating how the rays hit the object and were spread apart and so on. In his theory Luneburg also emphasized this idea. But then, about 1950, I did a calculation together with a student, Albert Blank, and that calculation showed that indeed there were waves reflected, as Luneburg's theory said, but in addition there were waves that weren't predicted by the theory, waves that came off the edge of an object. I realized that that is a general feature and that ordinary geometrical optics was inadequate to describe all the rays. I introduced additional rays, which accounted for the waves coming from the edges, for waves coming from corners, and for other kinds of things.

I found it is possible to use these rays to construct asymptotic approximations of solutions to Maxwell's equations and other wave equations. That led to a general theory for linear partial differential equations, namely, a method for constructing various aspects of the solutions by geometrical methods. That work was subsequently developed much further by Donald Ludwig, Robert Lewis, and Cathleen Morawetz, who were at NYU. Of course there were precursors in the work by Gerard Friedlander in Cambridge, Vladimir Fock in Russia, and others. My geometrical theory incorporated a lot of the work of those other people. Later, Lars Hörmander in Sweden, Michael Taylor in this country, and many others carried it much further and made a more comprehensive theory of linear partial differential equations in which these geometrical ideas played a helpful role.

Receiving an honorary degree with Luis Bonilla and the president of the Universidad Carlos Tercera, Madrid, 1997.

The Einstein-Brillouin-Keller Method

Notices: One of your major results was the Einstein-Brillouin-Keller method.

Keller: Yes. I devised a certain method for solving eigenvalue problems in quantum mechanics. After I devised it a colleague of mine pointed out that one of the formulas had a great similarity to something that Einstein had done. So I mentioned in my paper that Einstein had done a certain thing, and also in the course of my method I used some arguments of Leon Brillouin. So subsequently people called that method the EBK method, Einstein-Brillouin-Keller. I did that work in 1953 and published it in 1958. In that work I developed a certain index, which subsequently was rediscovered by the Russian mathematician Maslov, and ever since it has been called the Maslov index.

Notices: What was this index?

Keller: It was an index that, in my theory, had to do with the number of times a path touched a caustic curve. A caustic is a point in optics where rays come together. That number plays a role in calculating eigenvalues or, in quantum mechanics, calculating the energy levels. Maslov discovered the same number, and the Russian mathematician V. I. Arnold named it the Maslov index. Jean Leray, the famous French mathematician, complained that it was really the Keller index, so for a while people called it the Keller-Maslov index, but the Keller got dropped. Leray wrote a letter to a journal about this. I subsequently met Maslov, and he told me that he was sorry he didn't know about my work when he did his work, which is something that happens all the time.

Notices: How did you get interested in problems in quantum mechanics?

Keller: I had studied physics as an undergraduate and graduate student, and the problems of quantum mechanics were long-standing. I found that the methods that I had devised to solve wave propagation problems, like scattering and diffraction and so on, could with a slight twist enable me to solve quantum mechanical problems. This is

Signing the membership book on becoming a Foreign Member of the Royal Society, 1988.

be-cause the wave function satisfies a differential equation like the differential equations that I had been solving for wave problems. So it was natural. My long-term colleague Sol Rubinow and I illustrated the method by applying it to a number of examples. One of the examples was finding the eigenvalues and eigenfunctions of the interior of a closed curve. In quantum mechanics this corresponds to the motion of a billiard ball bouncing off the sides of a billiard table. We introduced two kinds of solutions. One corresponds to motions in which the ball goes almost along the boundary. We called them "whispering gallery modes", after a phenomenon that had been known for many years at Saint Paul's cathedral in London. A person could put his mouth near the wall of the cathedral and speak, and he could be heard by someone at the opposite side of the cathedral, but not by anyone in between. The explanation, given by Lord Rayleigh, was that the sound waves stayed in a layer, hugged the wall, and came around to the other side. So when we discovered these solutions for more general shapes, we called them "whispering gallery modes", and that's what they are called nowadays. Similarly, we found another kind of mode that pertains to a ball bouncing back and forth inside the curve. We called these "bouncing ball" modes. That terminology has caught on, and that's what they are called nowadays. Those modes have been used in solving wave guide problems, designing laser mirrors, and things like that.

Notices: There was a paper of Einstein's that figured in your work on the EBK method but that had been neglected for a long time.

Keller: That's correct. I had a colleague at NYU, a physicist, Fritz Reiche. He had occupied the chair of physics at the University of Berlin after Einstein. He was at NYU first as a faculty member and then as a research associate; he had retired already. Upon reading my manuscript, he was reminded of Einstein's 1917 paper, which he brought me. I read Einstein's paper, and I found that one of his formulas was similar to one of mine. I mentioned this in my paper, and that's how people came to know

Einstein's paper. It had been completely ignored up to then.

Notices: Why was it ignored?

Keller: That's not clear. It described a more elegant way of formulating quantum conditions, and it also had precursors of concepts of chaos in it. But somehow or other it was ignored. It was published in the *Proceedings of the German Physical Society*.

Pure versus Applied

Notices: How do you see the supposed dichotomy between pure and applied mathematics? At Courant, the emphasis was on applied mathematics. Now you are in a pure mathematics department.

Keller: Before 1900 there wasn't any sharp distinction; many mathematicians worked on applications. Poincaré, Hilbert, and others did both pure and applied mathematics. But from 1900 to about 1950, a schism developed. I believe the schism was greater in the United States than in Europe. But since 1950, pure mathematics departments in the United States have begun to incorporate some applied mathematicians. Here at Stanford the department has incorporated more applied mathematics, and the interests of people have shifted a little bit.

Notices: Do you think the schism is closing?

Keller: A little bit. String theory and gauge field theories in theoretical physics have provided another link between mathematics and science so that many mathematicians see scope for what they are doing in the realm of physics. Also, many departments have been hiring applied mathematicians because they feel they provide a needed link with engineering departments. But there is still a wide gap. I would say that the attitude of pure mathematics departments toward applied mathematics has become more friendly. But when it comes down to the question of whether to hire another algebraist, or a person who works in applications of differential equations in fluid mechanics, it's not so clear how they will come down.

Notices: Why do you think the schism was less in Europe?

Keller: I don't know the answer. In France, for example, where Bourbaki developed, mathematicians have been very pure. But, stimulated by Jacques-Louis Lions, a big group of young Frenchmen who were trained in pure mathematics and then began working on applied mathematics have developed a very strong school of applied mathematics. Before that, there weren't so many applied mathematicians in France. In England there has always been a strong applied mathematics component. The same is true in Russia. Much of my work was studied by Russians, so whenever Russians came to this country, they were much more familiar with my work than many American mathematicians. Russians were doing pure

and applied mathematics together. In the United States the applied mathematicians would know about my work, but the pure mathematicians would not. I had lots of correspondence and interactions with Russian scientists, mathematicians, and physicists, which was very nice.

Runners and Crawlers

Keller: I was always interested in athletics—I mentioned the "Basketball Seminar". One time I devised a theory of running. How should a runner expend his oxygen supply in a race to run it in the shortest possible time? Initially the runner has a certain amount of oxygen in his body, and he breathes at a steady rate. What limits the runner's speed is running out of oxygen, it turns out. So I made up a theory for how he should dole out his stored oxygen in order to run at the fastest rate. It led to a nice calculus of variations problem. I fit the theory to data on the world records to determine various physiological constants: how much oxygen is in the body, the breathing rate, the friction constant. I plotted the theoretical average speed in a race against the distance. This gave a very nice curve. The average speed went up to a maximum and then down. The first part of the curve corresponds to sprints: there is no strategy; you just run as fast as you can. But I found that when the race is longer than 291 meters, the average speed goes down. The world's records lie right on the curve I found.

Notices: Did any runners use this theory?

Keller: No, but it was discussed in various newspapers. Here is what a runner should do. Up to 291 meters he should run all out. But for longer distances, his speed as a function of time starts at zero; then he gets up to speed and runs at a constant speed throughout the race. A second or so before the end he should slow down. That's what I found. Now, why should he slow down? The answer is, he should run out of oxygen, and then he should coast during the last bit.

Notices: Wouldn't you think you should speed up at the end?

Keller: That's what the runners often say. No, because you shouldn't have any oxygen left at the end of the race. You should have used it all up. In fact, it should all be used up a hair before the end of the race; then you should coast. If you think of driving ten miles on a limited amount of gas, you should speed up, run along, and then run out of gas right before the end, because if you run out right at the end, then that last bit of gas isn't doing you any good. You should have used it up to go a hair faster. Some coaches said, "Oh, we knew it all along." And others said it was nonsense.

Notices: What kinds of problems have you worked on in the life sciences?

Keller and family skiing in Alta, Utah. Top, with wife Alice, and below, with Alice, daughter Sarah, and Alice's daughters, Gayle and Margot.

Keller: I had several very good students in that field. One was John Rinzel, who is now a professor at the Courant Institute, and we worked on the propagation of nerve impulses. There are some equations, the Hodgkin and Huxley equations, which describe the propagation of nerve impulses, and those equations are complicated to solve. We solved a simplified version of them and were able then to describe various interesting features of nerve conduction and nerve pulse propagation.

Later I had another student, Ken Miller, who started working with me, and he ultimately finished with an advisor in the neuroscience department here at Stanford. He was interested in optical dominance columns in the visual cortex. It turns out that some cells in the visual cortex respond only to right-eye stimulation, and others right nearby respond only to left-eye stimulation. Those cells are arrayed in columns, and so they are called optical dominance columns. It turns out that in a newborn kitten these cells respond to both right-eye and

Joe Keller hiking in France, 1996.

left-eye stimulation. But after six or twelve weeks, these optical dominance columns develop, so that the cells that were originally responsive to stimulation from both eyes become responsive only to one or the other. It has to do with the way in which synapse strength changes with usage. This change only happens when the eyes are exposed to light. It does not develop if the kittens are left in the dark. Miller made up a model having to do with how the synaptic strength changed with time and with continued stimulation, and I helped him with it. It was tricky, because the stimulation that is applied is somewhat random, and that randomness had to be taken into account in making up the model. He is now a professor at the University of California at San Francisco in neuroscience, although he started out as a physicist.

I have worked on other biological problems: crawling of worms.

Notices: What is the question there?

Keller: The way a worm crawls, say an earthworm, is that it stretches and contracts, stretches and contracts. How does it do that? If we want to move, we have bones that enable us to do it. But a worm is soft; it has no bones. How does it extend itself? It's like toothpaste: the worm has some muscles that go around its body, and when it squeezes them, it sort of extrudes, just like when you squeeze toothpaste. Then it relaxes those muscles and contracts other, longitudinal, muscles to pull up the rear. It does that again and again, so it is sending waves of expansion and contraction along its body. To analyze that motion, we formulated an optimization problem: What is the fastest that the worm can crawl, and what motion should it make in order to do that? I worked on this with a very good postdoc, Meira Falkovitz, who came here from the Weizmann Institute in Israel. The pictures we got looked very much like the photographs showing how worms actually crawl.

Notices: A snake moves differently.

Keller: A snake has a backbone, and therefore it can't stretch. So what it does is push off from pebbles and grooves and twigs. It needs something to push against. If you put a snake on a glass plate, it can't get anywhere, because it has nothing to push on. I studied that motion at one time, and I found that if a snake were to move on a flat surface, there would have to be some friction not only in the direction opposite to its motion, but also at right angles to it. That was contrary to the known theory of friction.

Notices: How do you choose things to work on?

Keller: That's a good question. First of all, I have to understand the phenomenon, so that limits me right away. Then I have to recognize there is a mathematical aspect to it, and I have to be able to make some progress on it. Oftentimes it has happened that I have had students or colleagues or postdocs who are interested in a certain subject and come around with problems, and I would see if I could help them. The hard part is to pick out problems that are interesting and for which the results would be significant—problems that are not mathematically impossible, on the one hand, nor mathematically trivial on the other hand. It has to be the right order of difficulty so that it is possible to make progress, but it's not just routine so that anyone could do it.

Notices: Isn't it difficult to learn about all these different areas?

Keller: Sure, but that's the fun too. I find that having studied physics as an undergraduate and graduate student and continuing to do that has enabled me to understand a lot of things that would have been prohibitive to learn otherwise. Biology is a whole new discipline, and I have only learned those small areas where I have been able to work. But in coming years we will see many mathematicians who study biology along with their mathematics so that they will be right at home with it.

Notices: How have you seen computers change applied mathematics?

Keller: Computing has become an automatic part of applied mathematics, another technique like analysis and differential equations. On the one hand it's being used as it was in the old days to calculate results obtained by analytical methods. But, going beyond that, computing is used to provide numerical results for problems that we can't do by hand or by ordinary analysis. One of the big areas where that is the case is fluid flow that involves turbulence. There are now methods to compute solutions of some of those problems, but the hardest problems are still too hard. We need further mathematical insight. The insight won't tell us the solution to the fluid flow problem but will enable us to develop new computational methods that will make more complex problems accessible to computers. In that realm, the job of the mathematician is to assist the computer or to enable the computations to be performed on problems that are otherwise too difficult for ordinary computational methods. That's where there will ultimately be a lot of developments.