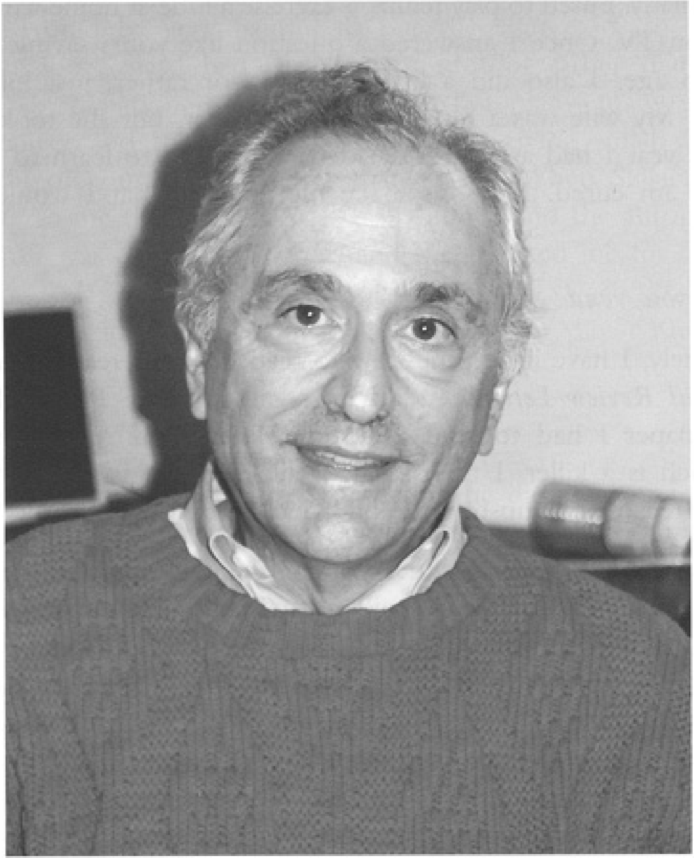


John N. Bahcall, 2002 (photograph by M. Hargittai).

# 11

# JOHN N. BAHCALL

John N. Bahcall (b. 1934 in Shreveport, Louisiana) is Richard Black Professor of Natural Science at the Institute for Advanced Study in Princeton. He received an A.B. in physics from the University of California, Berkeley (1956), an M.S. in physics from the University of Chicago (1957), and his Ph.D. from Harvard University (1961). He was on the faculty of the California Institute of Technology till 1970 and he has been at the Institute for Advanced Study since 1971. He is a member of the National Academy of Sciences of the U.S.A. (1976), the American Academy of Arts and Sciences (1976), the Academia Europaea (1993), and the American Philosophical Society (2001). He has received numerous awards, among them the NASA Distinguished Public Service Medal (1992), the Heineman Prize (1994), the Hans Bethe Prize (1996), the National Medal of Science of the U.S.A. (1998), the Russell Prize (1999), the Gold Medal of the Royal Astronomical Society (2003), the Dan David Prize in Cosmology and Astronomy (2003), the Benjamin Franklin Medal in Physics (2003), and the Presidential Enrico Fermi Award (2003). He was a member of the Hubble Space Telescope Working Group for more than 20 years. He was president of the American Astronomical Society (1990–1992). His home page contains relevant material, including popular articles: http://www.sns.ias.edu/~jnb. We recorded our conversation in his office at the Institute for Advanced Study in Princeton on October 23, 2002.*

*Magdolna Hargittai conducted the interview.

*What turned you originally to science?*

My path was unconventional. I did not show an early interest in mathematics or science. I was not a particularly interested student. My family and my family's friends were not intellectuals. They concentrated on making a living, a full time occupation.

In our high school, athletics were highly valued. I was excused every day at noon, instead of 3 p.m., to practice tennis. I did not take any science course in high school; my only technical training was a first-year course in algebra. There was, for me at least, no particular academic guidance. I only discovered that I had an academically-related talent in my senior year, when I joined the high school debate team. I rapidly became good in debating and my colleague, Max Nathan, and I won the national high school debate tournament (held in Boston in the summer of 1952). We beat a couple of students from some prestigious New York private school in the finals. This was the first and only time I know of when representatives from Louisiana went to the national debate tournament.

I went to Louisiana State University for my first year of college and studied primarily philosophy. I initially thought that I might become a reform rabbi, but I pretty quickly realized my interests were more academic than pastoral. Growing up in Shreveport, Louisiana, our reform rabbi and his wife, David and Leona Lefkowitz, were both inspirational and supportive. Looking back on that time, I think I must have believed that intellectual activities were practiced primarily by rabbis and their families. I was advised that the best preparation for a rabbinical career was to study philosophy, so that's what I studied. I was a straight A student in my first year of college, but I still did not study any science or mathematics.

During the summer after my first year of college, I attended summer courses at the University of California at Berkeley and I loved it. My mother elicited the financial help of a cousin, Clifford Strauss, who paid for my tuition. Because of Clifford, I was able to stay at Berkeley and finish there. I studied philosophy and made rapid progress. I began to think that maybe I could be a philosophy professor some day, but I ran into a severe problem. At U.C. Berkeley, there was a requirement that you had to take a college science course of some kind in order to graduate. I didn't have the academic pre-requisites necessary to take any of the college science courses, because I did not take any science courses in high school. So my advisor told me that I had to go to high school in the evening and take a high school science course.

I had been reading Bertrand Russell and Wittgenstein. Both of them described their great admiration for the achievements of physicists and the future of physics. I even imagined that I perceived some sense of regret on their part because they themselves did not become scientists. I remember statements to the effect that contemporary physics offered a great opportunity for an individual to make significant intellectual advances.

I really wanted to learn physics but I did not want to go back to high school. At Berkeley, there were three or four different courses in physics. One was for non-scientists, one for engineers and medical students, and one for those who wanted to become professional scientists. The person who taught the physics-for-physicists course was Burton Moyer (I believe). I went to him and said that I would like to take his course, but that I did not have any of the pre-requisites. I told him I was fascinated by the way physics was affecting philosophy. He told me, you are crazy if you want to take this course; it is for real science students. However, he said he would let me enroll in the course provided I dropped out as soon as I realized the course was too difficult for me. I did find that first physics course enormously difficult; it was the most difficult thing I had ever done in my life. I got a C in the course, but I fell in love with the subject. I loved the fact that you could use physics to understand phenomena in the world of experience, like why the sky was blue. I loved the fact that after a while everyone agreed what was the right answer to a question in physics. Because the course was so challenging for me, I loved it even more.

Incidentally, I don't ever remember thinking about a job in physics. To a first approximation, there were no academic jobs in physics when I decided to go to graduate school. Those were pre-Sputnik days. I just wanted to have fun learning physics. However you look at it, my path to science was neither direct nor conventional. It was not well planned. I definitely would not recommend my trajectory as a way to become a scientist.

*Then what turned you to astronomy?*

I think it was again a combination of Bertrand Russell's influence and chance. I remember an article in one of his books of essays in which he wrote about two things that he thought were the most important in educating human beings. One area was to learn about the majesty of what the human mind was capable of, which was exemplified by what have been accomplished in atomic and subatomic physics. The other was to understand the insignificance of human beings by recognizing their place in the larger scale of the Universe that was revealed by astronomy. This statement made a huge impression on me. I wanted to somehow be associated with one of these two great enterprises.

After completing my undergraduate degree at the University of California at Berkeley, I got a Master's degree at the University of Chicago and a Ph.D. at Harvard, all in physics. I was fully supported financially by university grants; otherwise, I could not have attended graduate school. By accident, I did a thesis in atomic theory. David Layzer gave me a summer job, finding a way to calculate the energy levels of highly-ionized atoms that had recently been measured by Edlen. He then went off to England to work with Bondi on cosmology. I solved the atomic physics problem over the summer. When David came back to the States at the end of the summer, I showed him that my solution was in agreement with the abundant spectroscopic data. He agreed and suggested that I write it up as a thesis.

Then I went in the fall of 1960 to the University of Indiana, where I wanted to learn weak interaction theory. I listened to a course on weak interactions by Emil Konopinski, a great physicist and a great pioneer in the subject of beta-decay. In order to teach myself the theory, I made up problems for myself that I solved. My first published paper, in 1960 or 1961, was on different ways to determine the mass of the muon's neutrino. Then, I calculated the rate of electron capture from continuum orbits, which is different from the usual (in the laboratory) electron capture rate from bound atomic states. Konopinski discussed bound electron capture in his course. I also calculated the effect of the Pauli exclusion principle on beta-decay rates and the probability of beta-decay into bound, not continuum, orbits. These were all variations on the usual themes considered by physicists and they were useful exercises to check that I understood weak interaction theory. I had lunch one day with a friend, Marshall Wrubel, who was an astronomer. Marshall asked me how my work was going and I told him what I was doing. I told him that I was disappointed to find that, when I put numbers in the equations I had derived recently, it did not look like any of the processes I had considered (continuum electron capture, bound state beta-decay) could ever be measured. Marshall suggested that I look at a famous paper by Burbidge, Burbidge, Fowler, and Hoyle, universally known as $B^2FH$, on the formation of the elements by nuclear processes in stars. $B^2FH$ was the equivalent of the bible for nuclear and stellar astrophysicists. He suggested that maybe there would be applications to what happens in stars.

This lunchtime suggestion was a turning point in my career. There was a table at the back of the $B^2FH$ paper prepared by Willy Fowler, which listed the characteristic properties of nuclei that were involved with the formation of heavy elements. The beta-decay rates were particularly important because they were the slowest processes in the buildup of heavy elements and thus set the time scale for the slow transformation from light to heavy elements. Willy had assumed that the beta-decay rates in the stars were the same as in the laboratory. It was obvious to me that this was not the case. I recognized that ions would be stripped of their electrons at the high temperatures in the interiors of stars, so for example, they would not capture electrons from the bound atomic orbits as they do on Earth. They would capture electrons from continuum orbits. There were also some other differences, like the effect of the Pauli principle. I pointed out in a short paper sent to the *Physical Review* that, based on my calculations, the rates of beta-decay processes would be different in stars than the ones that currently were being used by astrophysicists and physicists. Probably, I did not write the paper too tactfully, at least that is what Konopinski suggested to me. Anyway, I never got a referee's report for this paper, just a formal acceptance letter after some time.

I did get a handwritten letter from Willy Fowler, which turned out to be very characteristic of him. Willy wrote that he had seen my paper — which meant that he had been the referee, because they were no preprints in those days — and he would like to invite me to come to Caltech as a senior research associate to work with himself, Fred Hoyle, Dick Feynman and Murray Gell-Mann on problems in physics and astrophysics. When I first showed up at Feynman's office in CalTech, he threw me out saying he had never heard of me. Willy had neglected to tell Feyman or Gell-Mann that he had used their names in inviting me. About the same time he wrote to me, Willy wrote to Ray Davis at Brookhaven, who was thinking about whether it was possible to detect solar neutrinos. Willy wrote Ray saying that there is a guy, John Bahcall, at Indiana University who knows about weak interactions in stars and suggested Ray get in touch with me about solar neutrinos.

Ray wrote me and asked if I could calculate the rate at which $^7$Be captures an electron in the solar interior, thereby producing neutrinos in the Sun. He wondered if those neutrinos would be detectable. I remember thinking about the question for a while, because there was quite a bit of nuclear physics that I had to learn in order to do the calculation. Finally, I realized that this was a unique opportunity to study the interior of stars with neutrinos and therefore decided that I'd like to spend a few months working on that possibility. When I did the calculation, I realized, but only after I wrote up my results and sent them off for publication, that this was really only the beginning. What I had done was to calculate the rate for neutrino emission from electron capture by $^7$Be as a function of the stellar temperature, density, and chemical composition. This was probably what Ray had in mind. But I did not have a solar model to put those nuclear reactions into in order to predict what Ray should really measure.

So I decided that I would go to Caltech and try to utilize the existing stellar evolution computer programs and expertise in Willy Fowler's group. I wanted to use those programs to calculate the neutrino fluxes. That's what I did in 1962. I added the nuclear physics required to calculate the neutrino fluxes predicted by the solar model. Willy again played a very important role. I had difficulty in getting the experts to agree to run their programs with my nuclear physics. They were not interested in the Sun. They were interested in the frontier astronomical problems involving the evolution of giant stars and the explosions of supernovae. So, at one point, Willy had to use his authority to get my programs run the first time.

I also continued studying the effects of atomic electrons on beta-decay rates; processes that I called "overlap and exchange effects". (These effects arise because of the Pauli exclusion principle and because the initial and final state Hamiltonians are different.) There were a lot of laboratory data with which to compare my results; my calculations were successful in explaining measurements that were previously not understood. I got a lot of recognition, especially from experimentalists, for this work. But, on one occasion, Willy Fowler came into my office, asked what I was doing, and listened rather impatiently while I told him with great enthusiasm about my successful calculations. Willy rotated his head back and forth like he always did when he wanted to make some pronouncement that he thought was important. He said something like: "This stuff is wonderful, but you really need to do something in astrophysics if you want to have an influence on a wider scale. What you have done is an interesting intellectual problem but it's not going to change significantly how people think about the big questions."

I deeply resented Willy's advice at the time; it made me mad. But, when I cooled down and thought about it, I realized he was right. I regret that I never told him how crucial this conversation was to me. Anyway, after our conversation, I started to look around for other problems in astrophysics. There was a lot of work going on at CalTech with the newly discovered quasars, so I made, together with Ben Zion Kozlovsky, the first models in 1964 that explained quasar emission line spectra using photo-ionization calculations. Then in 1965, Ed Salpeter and I suggested that there would be multiple absorption line systems observed in the spectra of quasars and later I developed an empirical method of analyzing data to reveal those systems. I also did a variety of problems in atomic physics applications to astronomy. X-ray astronomy was new, so in the mid-1960s Dick Wolf, my first graduate student, and I calculated the cooling rates for neutron stars by neutrino emission caused by nucleon-nucleon collisions and by pion-like decays. Rates very similar to the ones we derived then are still being used today to discuss the temperatures of neutron stars observed by the Chandra satellite. And, of course, I continued to work on solar neutrino problems.

The bottom line answer to your question is that I got into astronomy by accident, found interesting problems by good luck and by being in a place where new discoveries were being enthusiastically discussed, and had excellent mentors to give me useful advice along the way.

*Solar neutrinos came into the limelight earlier this month, when the Nobel Prize in Physics for 2002 was announced. You just told me how you started to get involved with them. Would you care to tell us your further involvement with this topic?*

My involvement has lasted more than 40 years. I am still enjoying doing new problems in this subject. I told you how it started. We calculated the neutrino fluxes. But, to know whether Ray Davis could make a measurement or not, I also had to make myself an expert on calculating the cross sections for the capture of neutrinos in chlorine; which was the detector that Ray Davis wanted to use. I took the neutrino fluxes that we calculated from the solar model and I calculated what the rate of capture of neutrinos would be in a chlorine detector. When I first did this calculation in late 1962, the rate was much below what Ray thought that he could ever detect. That was very discouraging to both of us. It looked like the experiment could not be done.

For some reason, I did not publish my calculated cross sections. I remember, I thought at the time about whether I should include the neutrino cross sections in the paper that we did publish on the neutrino fluxes. But I didn't. I can't say precisely why. I don't remember. But, I continued to worry about the cross sections.

I guess about a year or so later, in August of 1963, I visited the Bohr Institute in Coppenhagen where Tommy Lauritsen, a senior nuclear physicist from Willy Fowler's laboratory, was spending a sabbatical year. I spent a week there and gave a talk about solar neutrinos. I described the neutrino fluxes and also the calculations that I had done but had not published on the rate for neutrino capture by the $^{37}Cl$ nucleus. Ben Mottelson, who together with Aage Bohr formed the best theoretical nuclear physics team in the world, asked a crucial question during my talk. He asked whether there could be a significant contribution from a transition to an isotopic analogue state that must exist, analogous to the ground state of chlorine, as an excited state of argon. I am sure that I must have answered something like it probably would not matter much because only $10^{-4}$ of the flux had enough energy to reach excited states in argon. That was my initial reaction, but I did not really understand the question. Probably, I had never heard before of analogue states. I talked to Mottelson after my talk and I decided to try to understand enough to evaluate the effect quantitatively. I studied some books and papers that were available at the Bohr Institute; I learned how to estimate the energy at which the analogue state would lie and I calculated the cross section approximately using some analytic approximations. I estimated that the transition to the analogue state, and other excited states in argon, would increase the expected neutrino capture rate by a factor of about 20. That was enough to make the chlorine experiment look feasible.

When I got back to Caltech, I wrote to Davis about this and we both got very excited. I refined my calculations and we gave two related talks, one after the other, at an astrophysics conference in the fall of 1963 at the Goddard Institute in New York. We wrote up a short joint paper on this subject that was published in the proceedings of the Goddard conference, but only came out two or three years later. We concluded that the enhanced rate that I calculated could be detected in an experiment that Ray thought was feasible.

After that, Willy encouraged us to write up our results fully as a joint paper for a refereed journal. We started to do this, but eventually we separated the results into two papers because the description was too long for one paper in the *Physical Review Letters.* The papers appeared back-to-back, theory and experiment, sometime in early 1964. That's how we got started. Incidentally, I never happened to be again at the same place at the same time as Ben Mottelson. I wrote him a couple of times updating him about the cross section calculations. I don't think I ever got a response. I have no idea whether he remembers asking the crucial question that led to the analogue state calculation.

The experiment got funded; I could tell you stories about how it happened. Ray and I collaborated on that also. In 1968, Ray obtained his first results, which were significantly less than what I predicted. This became known as the "solar neutrino problem". For the next 20 years, Ray and I tried to persuade people to do other experiments. I continuously refined my calculations, trying to find errors and estimating more and more accurately the uncertainties. In the refinement stage, I primarily worked with Roger Ulrich, for about 20 years. Roger had initially been a postdoc with me at CalTech, but he soon moved to nearby UCLA as a faculty member. Roger and I learned an enormous amount working together and I enjoyed that experience enormously. The first exploratory solar model calculations were done largely by Dick Sears with me supplying the nuclear physics. Much later on I worked with Marc Pinsonneault to include element diffusion in the solar model calculations.

During the time that Roger and I were refining our calculations, Ray made many tests of his experiment to see if it was possible that he was not measuring some of the argon atoms produced by neutrinos. He convinced everyone that looked carefully at what he was doing that he was not missing argon atoms.

Sometime around 1988, the first of the Japanese-American results with a large water detector also found fewer neutrinos than my calculations indicated. Matoshi Koshiba originally proposed this detector and the experiment was led by Yoji Totsuka, with important contributions from Gene Beier and Al Mann at the University of Pennsylvania, and many talented collaborators in Japan and the U.S. These results confirmed that there was really a "solar neutrino problem", that there were fewer neutrinos than predicted.

Incidentally, the Japanese-American detector was not built to see solar neutrinos. It was called Kamiokande, where the "*nde*" stands for nucleon decay experiment. The detector was sensitive only to high-energy events, such as would accompany nucleon decay. When I first heard about it, I did not think that the conversion to detect low energy events could possibly be successful. But, it was. Koshiba wrote me a letter which I still have saying that he made the conversion in order to resolve the discrepancy between Ray's results and my calculations. He didn't do that single-handedly, but he certainly strengthened greatly the case for a solar neutrino problem.

Subsequently, solar neutrino experiments with gallium, involving large international collaborations of physicists and chemists, were performed in Italy and in Russia and reached a similar conclusion. These gallium experiments were beautifully done and they detected neutrinos from different reactions than those observed in the chlorine and the Kamiokande experiments. The gallium experiments were primarily sensitive to low-energy neutrinos whose flux I could calculate more accurately than the neutrinos that are observed in the chlorine and Kamiokande experiments. There was really a solar neutrino problem. Incidentally, there was an incredible collection of experimental talent that did the gallium experiments, led by the spokesmen, Vladimir Gavrin and George Zatsepin in the Soviet Union and Till Kirsten in Germany, with many expert collaborators.

By the early 1990s, it was certain that there were fewer neutrinos reaching us than predicted by our solar model calculations. But, I think that most physicists not in the field were betting that my solar model calculations were at fault.

*According to the Standard Model of electroweak interactions, the neutrino is supposed to have a zero mass. Now all these experiments indicate that the neutrino has a small mass. What about the Standard Model then? Is it a wrong model or does this only mean that we got to a deeper level?*

It's not a wrong model. It's an extraordinarily successful model. It predicts precisely the results of many sophisticated and probing experiments. It describes very accurately many phenomena and unifies electricity, magnetism, and the weak interactions. So it is a great theory, even if an incomplete theory. There is not a natural way in the Standard Model to give the neutrino a mass but one can, without going far beyond the model, incorporate a finite mass neutrino and that's adequate for all the phenomena we know about so far.

*Does the fact that the neutrino seems to have a mass predict that there will be a new physics?*

It's hard to know what the role of neutrino mass will be in a future, more complete theory until we have a more complete theory.

*What do you need for getting to this new generation of physical theory? Just thinking or more experimental data?*

I don't know. My colleagues here at the Institute for Advanced Study mostly concentrate on getting there by thinking. They are mostly string theorists; they are not experimentalists or phenomenologists. If you look back at the history of physics, very often breakthroughs have been achieved by experiments revealing things that were unexpected and that lead to new theoretical developments.

*What will be the next step in neutrino astronomy that you foresee?*

We want to study both very low and very high energies. The very low energies are characteristic of solar neutrinos. According to the standard solar model, more than 99.99% of the flux of neutrinos that is expected to come from the Sun is below 5 MeV in energy. So far we only have direct measurements of solar neutrinos with energies about 5 MeV. We must test at low energies our theories of neutrino physics and of stellar evolution. The astrophysics predictions are most precise for low energy neutrinos. At low energies, we expect to see dramatic and characteristic effects of new neutrino physics, not all of which may have been anticipated. The frontier for solar neutrino astronomy lies at energies less than 1 MeV.

For extragalactic and galactic neutrino astronomy, you really want to go to energies above 100 TeV ($10^{14}$ eV) and look for sources using very large underground detectors; under ice in Antarctica, under water in the Mediterranean, and in Russia in Lake Baikal. One plausible possibility is that high-energy neutrinos may be seen from the Gamma Ray Bursts that come to us from some of the most distant observed regions of the Universe. I am sure that with proper instrumentation we will observe neutrinos produced by distant cosmic rays interacting with the cosmic background radiation.

The next step in neutrino astronomy will be to increase the average distance from which the observed neutrinos reach us by fifteen orders of magnitude, from $10^{13}$ cm (the Sun) to $10^{28}$ cm (Gamma Ray Bursts or unknown distant sources).

*What do you expect to learn from these very high-energy experiments?*

We want to know what else there is in the Universe that we can't see in ordinary light, i.e. with photons. If we observe distant astronomical sources, like Gamma Ray Bursts, with neutrinos, we may learn entirely new things about the physics of neutrinos. This is possible because of the long propagation times, $10^{10}$ years, for cosmic neutrino sources instead of 10 minutes for solar neutrinos. There is more time for something exotic to occur. There is a large arena of unexplored territory that can be studied with high-energy neutrinos. There may well be surprises, unanticipated results, as there were with solar neutrino studies.

John Bahcall standing with Ray Davis at the tank of the Homestake Mine in the mid 1960s (courtesy of J. Bahcall).

*There are different neutrino experiments: one in the Homestead gold mine in the U.S. that uses chlorine in the form of perchloroethylene, there are experiments in Russia and Italy that use gallium as a detector, there are experiments that use pure water in Japan and even an experiment that uses heavy water, deuterium, in Canada. As I understand, they all measure a somewhat different energy range. Why is that so?*

The chlorine and the gallium experiments are both radiochemical experiments. An electron neutrino is captured by either a chlorine or a gallium atom and transforms that atom into either a radioactive argon atom, for a chlorine detector, or a radioactive germanium atom, for a gallium detector. That can only happen above a certain energy threshold and the thresholds are rather low. But the radiochemical detectors do not record the energy of the captured neutrino; any energy above the threshold, which in the case of chlorine is 0.8 MeV and in the case of gallium is 0.2 MeV, can cause the same reaction. You measure how many radioactive atoms are produced, but you don't know what energy neutrinos produced the signal.

In addition to having different energy thresholds, the chlorine and gallium detectors have very different responses, or sensitivity, as a function of energy. Chlorine has a great sensitivity to high-energy neutrinos, due to a so-called super-allowed transition, which I first recognized in 1963.

*How does it work?*

In 1963–1964, I predicted that there would be an isotope, not previously discovered, $^{37}Ca$, which would not decay rapidly by ordinary nuclear processes but rather would decay to $^{37}K$ by relatively slow beta-decay processes. Using the same ideas that led to the prediction of an enhanced sensitivity of chlorine to high-energy ($^{8}B$) neutrinos, I calculated the lifetime of $^{37}Ca$, which was potentially measurable in the laboratory. If it were found in agreement with my predictions, then the laboratory measurement would confirm my calculation of the neutrino sensitivity of the chlorine (Davis) detector. The phone call that I got telling me that $^{37}Ca$ had been discovered and that its properties were in agreement with my calculations was the most exciting event in my scientific career.

Anyway, with regard to your previous question, the water detectors in Kamiokande use a different kind of reaction; they use the scattering of neutrinos by electrons, which was also something that I studied in 1964. I calculated the angular dependence of the scattered electrons that would result from neutrinos from the Sun hitting a target that contained electrons in the water. I showed that the recoil electrons from neutrino-electron scattering are very forward peaked, lying preferentially in the direction of the Sun-Earth axis. That directionality is used today to detect solar neutrinos in the Japanese experiments. Electrons scattered by solar neutrinos move away from the Sun, while background events are essentially isotropic.

These neutrino electron scattering events are sensitive not just to electron type neutrinos as is the case for the radiochemical chlorine and gallium experiments, but they are also sensitive somewhat to muon and tau type neutrinos. So they measure not only electron neutrinos but, to a lesser extent, also the other type of neutrinos.

A much larger version of the Kamiokande detector, called Super-Kamiokande, began operating around 1995 or 1996. This detector was used to study solar neutrinos with great precision; the work of a very large and talented team of physicists was led by Yochiro Suzuki and Yoji Totsuka.

Finally, the Sudbury Neutrino Observatory, universally called SNO, in Sudbury, Canada, uses 1000 tons of heavy water, in which deuterium replaces ordinary hydrogen, as a unique detector. This experiment has two great advantages. SNO can measure separately just neutrinos of the electron type and determine their approximate energies (above about 5 MeV). SNO can also use their heavy water to make a separate measurement of the total flux of neutrinos of all types. Together, these two measurements are a supremely powerful test of whether something new happens to neutrinos on their way to the Earth from the interior of the Sun. This experiment is a Canadian-US-British collaboration, with Art McDonald as the spokesperson for a superb team of physicists and chemists.

The first direct proof of new physics with solar neutrinos was obtained by comparing the results of the SNO measurement of just electron type neutrinos with the Super-Kamiokande measurement with measured electron type plus other neutrinos. The difference between the two measurements indicated that about two-thirds of the electron type neutrinos that originate in the center of the Sun change to other types before they reach detectors here on Earth.

*What makes these different types of neutrinos transform into each other; that is, what makes them oscillate?*

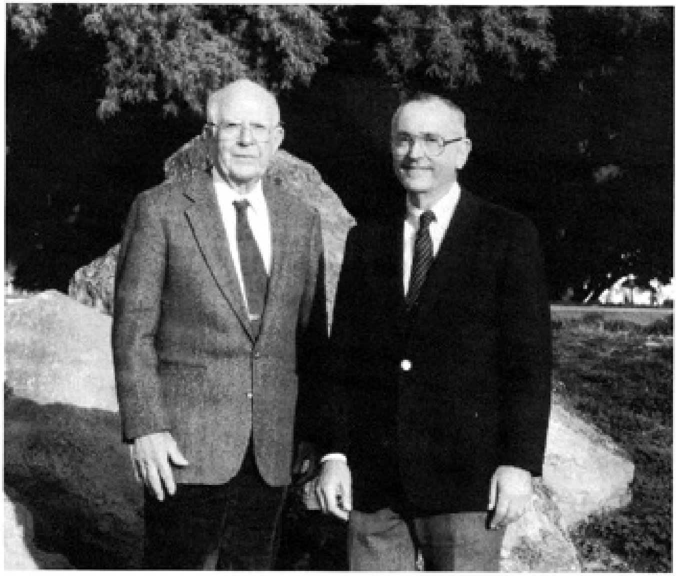
Oscillation is a quantum mechanical phenomenon. It is caused by the fact that neutrinos of different types can have different masses and the neutrinos we normally see in the lab, like electron type neutrinos or muon type neutrinos, are really linear combinations of neutrinos with different, definite masses. One factor causing oscillations to occur is that neutrinos of different masses travel at different speeds so the phases of the terms in the linear combinations can change. Another factor causing oscillations is that neutrinos of different types interact differently with the electrons in the Sun and in the Earth; this is what is known as the Mikeyev-Smirnov-Wolfenstein effect.

*Please, tell me something about Raymond Davis. I understand that you cooperated with him for over 40 years.*

Ray is an extraordinary human being. He is helpful to everyone; he is respectful and pleasant to everyone; he treats the janitor who works in his building with the same courtesy, respect and kindness as he does the most famous professor. Ray is modest; he has no personal ambition. His goal in doing science is to understand the way the Universe operates. He is very close to all the members of his family.

Ray always looked for ways to help other people do their science. He would often make measurements that would be useful to others in their experiments. He is absolutely honest and extremely stubborn. If he hadn't been so unambitious and so stubborn, he would never have devoted himself to such an out-of-the-mainstream experiment.

In the early days, the 1960s and the 1970s and into the early 1980s, all of the people who were seriously committed to solar neutrinos could, and frequently did, ride in the front seat of Ray's car. Only Ray and I



John Bahcall and Ray Davis on the occasion of Davis' Tinsley Prize and Bahcall's Heinemann Prize in 1995 (photograph by and courtesy of Jacqueline Miton).

were committed in the sixties and seventies. I rode with Ray because I had confidence that Ray could do a reliable experiment, one that would justify my spending so much time on making precise theoretical calculations. Only Ray was willing to take the risk and devote himself to such an exotic experiment. And, probably, only Ray had the set of talents and the character to make it work.

Ray did an experiment which most people thought was impossible when it was proposed. He treated every criticism of the experiment with seriousness and he made measurements to demonstrate that each criticism was not valid. He is not a person who is particularly quick. He instinctively distrusts theory, but is curious about ideas. We gave hundreds of talks together in the first thirty years of the subject. We were a good complement to each other. He addressed the experimental side and I covered the theoretical side. I was privileged to work with Ray and to learn from him about science and about how to be a decent human being. I admire him immensely. He is a role model for me.

*I would like to talk a little about this year's Nobel Prize.*

All three people, Davis, Giacconi and Koshiba, richly deserved the Prize. Both Ray Davis and Toshi Koshiba made extraordinary contributions to science. They've written not just new sentences in the history of science but whole new chapters. Their contributions are made all the more extraordinary by the fact that they are such extraordinary human beings, who are enormously appreciated and admired by their colleagues.

Many particle physicists do not know the revolutionary role Riccardo Giacconi played in the field of X-ray astronomy. I worked in X-ray astronomy for many years and I know both Riccardo and his contributions very well. Riccardo energized and inspired two decades of X-ray astronomy, which has led to a multitude of discoveries. X-ray astronomers throughout the world greeted his receipt of the Nobel Prize with jubilation.

*You said that Riccardo Giacconi energized discoveries. What was his direct involvement?*

Yes. He was at the center of the group that was making the discoveries. When there is a group of people collaborating on a particular project, they each have different roles. Riccardo led collaborations building telescopes and detectors and collaborations making discoveries. In fact, for about two decades Riccardo led the entire field of X-ray astronomy and much of astronomy in general. I called him at home on the day the Nobel Prize was announced, congratulating him and telling him that I shared the widespread view that he richly deserved the award. I have great admiration and affection for Riccardo.

*Was it logical to combine these two fields, neutrino astronomy and X-ray astronomy?*

In both fields, the prizes were awarded for opening a new window to the Universe.

*I talked to people before the Prize was announced this year and you were mentioned as possible candidate. How do you feel about it?*

I am pleased to be mentioned in this distinguished context.

*What do you think, had you written up your results back in 1963 in a joint paper together as originally planned and not in two back-to-back papers, would the outcome be different?*

I don't think so. The Nobel Committee quoted both Ray's paper and mine from 1964 in their technical notes.

*Let's go back to science. The solar neutrinos are supposed to account for part of the dark matter in our Universe. Any ideas about what the rest of it is?*

If I had any idea about that, I would immediately excuse myself and rush away to write a paper on the subject!

*You have also been involved with different models of the Galaxy. What are these?*

I served for more than 20 years as the member of the scientific guiding committee that was responsible for the Hubble Space Telescope. To help us think about how to make the inevitable tradeoffs between different instruments or developments, I began calculating, together with a young colleague from our Institute, Ray Soneira, what we expected the telescope to see with the HST cameras.

At one point, the science committee considered how the telescope could be pointed accurately as required. I began looking into the question of how well known was the density of bright stars on the sky. The bright stars could be used as guide points to fix the telescope's direction. Ray Soneira and I found that there were errors in the standard published tables of these data. That was in the late seventies. We concluded that the star density was much less than what had previously been published. There was a lot of resistance to our results at first because they required an expensive change in the guide-star equipment and because we were just amateurs

John Bahcall receiving the National Medal of Science from President Clinton in April 1999 (courtesy of J. Bahcall).

with no previous record in the subject. But, eventually our pessimistic calculations were accepted and the guide-star system was redesigned to reach the level of sensitivity that we specified. In fact, the Hubble Space Telescope requirements document stated, among many other things, that the guide-star system had to be sensitive to the lower star density that was specified in our paper.

As a natural generalization of the answer to the practical question of what was the bright star distribution on the sky, we made a model of the star distribution of the entire Galaxy at different brightnesses, colors, distances, and stellar types; we used that model to compare with lots of data. The basic premise of our model was that our own Galaxy was like other galaxies that we could see which had disk and spheroidal components. We took the initial model parameters from whatever measurements were available. We refined our model iteratively by comparison with the observed distribution of the stars in the Galaxy.

I had enormous fun with the Galaxy model project because there was a continuous interaction between the modeling and the data. We could summarize huge amounts of data that had been previously accumulated with just a few meaningful parameters. The model is still useful today, almost a quarter century later.

What I love most to do in science is to explain quantitatively things that are measured or observed.

*Other topics that you've been involved with?*

In 1965, Ed Salpeter and I suggested that we could use absorption lines in the spectra of quasars, which had recently been discovered to be the most distant known objects in the Universe, to learn about the gaseous material along the line of sight between the distant quasars and us. The basic idea was to use the quasars as a sort of flashlight to illuminate the medium between the quasars and us. We predicted that gas clouds or clumps along the line of sight would produce discrete absorption lines. We predicted which lines would be strongest.

Within about a year, the absorption lines were discovered and ever since quasar absorption lines have been observed in abundance. Quasar absorption lines are now a standard cosmological tool. But, in the early years, most astronomers, including the astronomical experts, believed that the lines were produced by material associated with the quasars, i.e. they thought that the lines were not produced in the way Ed and I had suggested, by the cosmologically distributed material between the quasar and us. So, I invented a technique of analyzing simulated absorption line spectra that looked like the real spectra to prove statistically that the real spectra had clumps of gas at many different redshifts along the line of sight. I am not sure, but this could have been the first application of Monte Carlo simulations to cosmology or astrophysics. This was a multi-year project because the opposing viewpoint was held by people who were experts in astronomical spectra.

I also got involved, I think in 1964, with making the first models of quasar emission line regions that were ionized by the strong light emitted by the quasar itself. Ben Zion Kozlovsky and I did this work together. All of the quasar work was a lot of fun because there was a lot of interaction between new data and the modeling.

*You mentioned earlier that when you first went to CalTech, both Gell-Mann and Feynman were there. Did you know them?*

Yes. They were the great scientists to whom all of us looked up. We knew that we were privileged to be working in their vicinity. In retrospect, just thinking about what they understood and created, they seem like even greater giants than they did in person.

I can tell you a personal story about Feynman from 1968. Before Ray Davis' first result was published, Ray came to CalTech for a week. We were again writing papers to appear back-to-back in *Physical Review Letters.* Willy Fowler arranged for a small, private presentation by Ray and myself. He invited Dick Feynman, Murray Gell-Mann, Bob Leighton, Maarten Schmidt, and maybe one or two nuclear physicists. It was a small group in a small classroom in Bridge Hall.

That was a very tense time for me. I was a young assistant professor, without tenure. My best-known work was predicting the rate of neutrino capture in Ray's tank and Ray was getting an answer different from what I calculated. So I presented my theoretical discussion of what was expected and Ray described what he'd measured and why it was clearly less than my calculated rates. There were questions during the talks and a follow-up discussion; then the meeting broke up with no particular conclusion.

I was enormously depressed. I was young and ambitious. I had made a striking prediction that was not confirmed. Dick Feynman was not a person who wasted his time. According to some of his biographers, he was also not characteristically altruistic. But on this occasion, he saw that I was depressed and he did something about it. He said to me, "Let's go for a walk." We did that. We walked for more than an hour; I still remember where we walked. He mostly talked to me about things that were not very substantive.

After walking for a while, Feynman told me that he could see that I was upset. He said I should not feel bad because no one had found an error in my calculations. He said he did not know what the explanation would be for the discrepancy between Ray's measurements and my calculations, but it could be important. He tried to cheer me up by saying that I had not performed badly.

That walk, and that talk, his kindness on that difficult occasion, meant an awful lot to me.

*Talking about mentors and advisors, whom would you like to mention?*

A lot of people have taught me science and helped me along in my career. Emil Konopinski, at Indiana University, introduced me to weak interaction theory. Willy Fowler was a very strong mentor at CalTech, as I already mentioned. Dick Feynman was always helpful when I had a particular scientific question. Like everyone else at CalTech, I went to Feynman to get his insight and advice whenever I thought I had a new idea that might interest him. Murray Gell-Mann was always extraordinarily generous to me at CalTech. He was the one who arranged for my coming here, to the Institute for Advanced Study. I regret very much that I have seen very little of him since I left CalTech. He was a great inspiration. Conversations with Murray were very different from conversations with Feynman. Feynman would listen and work out for himself what you told him. Murray would tell me what I should be working on, what problems I should solve.

When I came here, there were several people who were very helpful to me. Marshall Rosenbluth who had the office next to mine for almost a decade, is widely regarded as the world's leading plasma physicist. But, he is also a great human being. Marshall was the person I most talked to about technical things in astrophysics. Martin Schwarzschild was a great astrophysicist, a stellar evolution theorist. He didn't do things in the detailed style that was required for the solar neutrino problem. But, he was always enormously supportive and insightful. He was the one I could talk to about "big-picture" aspects of the solar modeling. Lyman Spitzer, with whom I worked for about a quarter of a century on the Hubble Space Telescope, was a close scientific friend and a mentor. Even today, when I have a tough decision to make, I ask myself: how would Lyman have addressed this problem? Very often, that question makes obvious the answer to my problem.

John Bahcall with Willy Fowler at Fowler's 80th birthday celebration at CalTech (photograph by and courtesy of Stan Woosley).

Lyman and I worked together to sell the Hubble Space Telescope to the Congress; we lobbied every relevant congressman and senator together. We went together on innumerable trips to NASA meetings.

Lyman Spitzer, Martin Schwarzschild, and later Jerry Ostriker, Scott Tremaine, and I worked actively together to maintain a supportive and collaborative scientific environment here in Princeton. Astrophysics at the University and the Institute are very closely linked to each other.

In astronomy, my principal scientific mentors were Ed Salpeter (Cornell University), Peter Goldreich (CalTech), Jerry Ostriker (Princeton University), Jim Peebles (Princeton University), Martin Rees (Cambridge University), and Scott Tremaine (Princeton University). But, I learned an enormous amount from all of the young people who were postdocs at the Institute for Advanced Study.

*You chaired a committee about the future of astronomy and astrophysics. What was the outcome of that?*

We set priorities for the next decade, from 1990 to 2000, in astronomy and astrophysics. We recommended a prioritized list of 20 projects; the top 18 were funded. We didn't recommend a lot of other good projects; I think we may have turned down as many as 10 ideas for each project that we put on our list of priorities. Congress and the federal agencies were all appreciative that we ranked things according to priorities. They understood that we had made tough choices and that they would get good return for their money. Our top priority was the space infrared telescope facility, which is to observe in the infrared region from space in the same way the Hubble telescope observes in the ultraviolet and visual. The infrared telescope is going to be launched within months.

*Does your group need more funding?*

I was asked the same question once before, in a public forum, by the president of the World Bank, who is also the Chair of the Board of Directors of the Institute for Advanced Study, Jim Wolfenson. He was obviously very supportive when he asked: "Do you and your group need more funding?" I thought about it and I understood that this might be an opportunity for fund raising, but I answered him honestly. We need additional brainpower more than we need more funding. We can use more funding, but the real bottleneck is our limited theoretical understanding.

*How about experimental research?*

I think that the projects I most believe in, and to which I am willing to devote myself to help make sure that they will happen, are going to get funded by the normal processes. That was not true in the late 1970s and early 1980s, when Ray and I were trying to get a gallium experiment funded in this country. That experiment did not get done in the U.S., but gallium experiments were eventually performed in Italy and in Russia, with international participation.

It is not generally known, but I was a principal investigator on a proposal to do a gallium experiment in this country. The proposal was essentially Ray's proposal, and included both techniques that were used later in Russia and Italy. The reason why I was the principal investigator was that it was a proposal to the National Science Foundation. Since Ray was at a Department of Energy institution, he could not apply to the NSF.

If you really told me now that I had a budget with which I could do anything I thought was important, I would establish the National Underground Science and Engineering Laboratory. I think that for the world and for the United States that's a great opportunity. We need a large, flexible, deep, and dedicated national and international facility that can do all the scientific and engineering experiments that require a deep underground facility. The scientific program includes biology, the discovery and study of species which are unique and exist without sunlight, which may even be the most primitive forms of life; we don't know, they have very different energy cycles. The program includes studying the transport of water over large distances and the geophysics of rocks under high pressure. It includes experiments to study dark matter, perhaps discovering dark matter experimentally, to study double beta-decay, to help determine the characteristics of neutrinos, to do solar neutrino astronomy at low energies, to do many different kinds of science in a unique environment.

The National Underground Science and Engineering Laboratory is the highest priority future project with which I am currently involved. I think it is going to happen; I don't know how soon, but it will.

*You run an astrophysics group at the Institute for Advanced Study. Can you tell us something about that?*

When I first came to the Institute, there was no astrophysics program, just particle physics and plasma physics. But, over the years, we have built a postdoctoral program in astrophysics that is generally regarded as one of the best in the world. We have been really lucky; many outstanding young people have chosen to work here. We try to make the environment supportive and stimulating. Since we don't have students, we can focus entirely on the postdocs. My primary job as an IAS professor is, in my view, to be helpful and supportive to the postdocs.

Running the astrophysics group has been enormous fun. I get great pleasure from being helpful to the young people and watching what marvelous things they come up with. Moreover, the postdocs are extremely stimulating for me; I learn an enormous amount from talking to them. They have become an extended family for Neta and myself.

*I would like to ask you about your family background.*

I grew up in Shreveport, Louisiana, and lived there until I went away to college. My mother got a bachelor's degree in music at the University of Illinois and later a Master's degree in social work. She was the only person in our close family with a college degree. My dad was born in Appleton, Wisconsin a few months after his parents came to the U.S. from Russia. He grew up in Maywood, Illinois, near Chicago. He also went to the University of Illinois for a year or two, where he met my mother. My dad worked as a traveling salesman for a wholesale produce company, which sold fresh fruits and vegetables. The company was owned by one of my mother's uncles in Shreveport. There were very few Jews in Louisiana, and especially in Shreveport, but somehow one of my mother's uncles ended up there during the great depression. He acquired an open cart, successfully sold fruits and vegetables in the street, and was able, subsequently, to provide employment to his family and other families as well. I have one brother, Bob, an older brother, who like our father became an expert salesman. He sells primarily cleaning materials and lives together with his family in Baton Rouge, Louisiana.

*Did your parents experience anti-Semitism?*

My parents never discussed anti-Semitism with us. If discrimination affected us, we accepted it as a fact of life. We lived in a completely segregated society when I grew up. Blacks were not allowed to attend schools with whites, nor to eat in the same restaurants, play in the same sport facilities, or use the same water fountains. The restrictions on Jews were much more subtle. My brother and I were the only two Jews in the elementary school that we went to in Shreveport. Our family was much less well-off economically than the other Jews in Shreveport and we lived in a different section of the town, a neighborhood for people with very modest economic resources. I can remember occasions when my brother and I had to run home from school in order to avoid getting beat up. The majority of the kids in our school were Baptists, but there was a minority of Catholics and every now and then the Baptists would beat up the Catholics, who then beat up the Jews, my brother and I. But, these were isolated incidents.

*What was your name originally?*

Bachalor, which I think means a wine goblet in Russian.

*Your wife is also an astronomer. Did you ever work together?*

Yes, we did. In fact you can see on my bookshelf a picture of my wife, our two sons, and me; that picture was taken just after we made a discovery together in the early 1970s. We had exclusive use for a summer of the 1 m telescope in Mitzpeh Ramon in the Negev desert of Israel. The telescope was just being brought into operation and our task was to convert the facility into a working scientific observatory. I had to develop the darkroom; we worked together on the first instruments.

X-ray astronomy was in one of its golden ages of discovery; Riccardo Giacconi and his group had found a number of pulsing neutron stars in binary systems (two stars going around each other) that emitted X-rays. But no one could find the optical counterparts of any of these X-ray binary systems. So, we didn't really know what they looked like, how far away they were, what type of ordinary stars were going around the neutron stars.

Since the observatory had only one operating instrument, a camera, the only thing that we could do was to take pictures. We took repeated images of the directions on the sky where the X-ray binaries were located. Neta noticed that one of the stars in our observing program had a period equal to the binary period of the X-ray binary, Her X-1. Bingo, we had made an important discovery. What luck! With rudimentary observing equipment, we succeeded ahead of all the astronomers in the rest of the word who were trying to identify the X-ray binaries with sophisticated equipment and clever techniques. No one else tried to look at the slow light variations, analogous to the phases of the moon, due to the binary motions of the X-ray star and its companion (Hz-Herc).

We collaborated together on a number of projects over the years, including studies of quasars and of globular clusters. We have not collaborated scientifically in the last decade or so, because Neta's interest has become more focused on cosmology and my interest has become more focused on neutrinos. But we talk about astronomy a lot.

*Did your children mind that when they were small?*

I think they may have thought that our conversations were limited. Both Neta and I have a scientific view of the world. However, when our kids were growing up, they probably didn't know enough to protest. We were the only family they had ever lived in. They just took things as they were.

*Please, tell us something more about your family.*

I consider myself lucky in life because, after an enormous effort on my part, Neta agreed to marry me. I am still feeling lucky every day. I have always admired her smile, her intelligence, her practicality, her logic, and her ability to get things done. We share the same cultural, intellectual, and ethical values. Our kids are great; they are a source of wonderful pleasure. Now, they are more like our good friends than our children.

We have three children. Our oldest son, Safi, is 34; he has a Ph.D. in theoretical physics and is the CEO of a bio-tech company. Our middle

The Bahcall family: Dan, Orli, Safi, John and Neta, 2003 (courtesy of J. Bahcall).

son, Dan, is 31; he has a Ph.D. in cognitive psychology and is involved with non-profit organizations that support environmentally-friendly activities and other socially desirable projects. Our daughter, Orli, is 26 and is finishing her Ph.D. in epidemiology at Imperial College in London.

*How did you first meet with Neta?*

Yuval Ne'eman arranged for me to give some lectures on nuclear astrophysics in Israel. He wanted to start astronomy in that country. So, in 1965, I came to the Weizmann Institute. The first day I was there, I went to the basement of the physics building where they had a van der Graff accelerator. I was looking for a friend of mine, Gabi Goldring, a nuclear physicist I had met earlier at CalTech. Gabi was not in the lab, but I saw this beautiful young woman with a wondrous smile, working on some apparatus. When I finally found Gabi, I asked him to introduce me to the smiling young woman who, it turned out, was his student. Gabi asked Neta to show me the laboratory and she did. I invited her for a coffee, I invited her to go for a walk, I invited her for lunch, I invited her to a movie. Over the next several days, I asked her out many times but she always said that she had too much work to do on her experiment. I was to be at the Weizmann Institute only for 10 days; I began to feel desperate. Finally, I called Neta at home and she said, "OK, my mother has tickets for the opera tomorrow, but she can't go. Do you want to go with me?"

Of course, I said yes and we went to the opera. We were attracted to each other, but it was a great struggle to get her to agree to marry me.

*What was the greatest challenge in your life?*

The most difficult and challenging situation I ever faced, and the problem I worked hardest to solve, was to persuade Neta to marry me. If you ask me what was the best idea I had in my whole life, it was the following. After Neta and I met in Israel, we continued corresponding for about half a year. I wrote asking her whether she would like to come to Caltech to visit. She replied: you should come to Israel and settle here. I wrote back saying: there is no astrophysics in Israel; I can't work there. She said she would never leave Israel. Then I got the best idea I have ever had. I scraped together nearly all of the money I had saved up and bought a roundtrip ticket from Israel to the U.S. with an open return date. I sent the ticket to Neta, with a note saying I hoped she would use it. That was in December of 1965. At that time no one left Israel because nobody had money to travel. Neta said for her going to the U.S. was almost like going to the moon. So it was a rather dramatic thing to do.

I remember driving to the post office to mail the ticket with a friend of mine, a physicist named Joe Dothan, who had known Neta from the time that he was her graduate student instructor. He assured me that there was no chance that she would come — but she did.

*What do you do when you are not doing science?*

My hobby is literature; I like to read novels. Most everything I read recreationally nowadays is in Hebrew; I read primarily modern Israeli novels. I like the challenge of reading in a language that is not my native tongue. But, until I was in my middle or late forties, my recreational reading was almost entirely English language novels.

*When did you learn the Hebrew language?*

After we were married. First, I learned to speak conversationally, but later I taught myself to read.

*Would you like to add anything?*

No, I think we've covered a lot more than I expected.